Annus LXV Vol. 106

"SCIENTIA"

(RIVISTA DI SCIENZA)

Rivista Internazionale di sintesi scientifica - International Review of Scientific Synthesis

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1971

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Annual subscription: Italy Lire 15,000 - Abroad $32.00

All subscriptions are entered on a calendar year basis only from January to December.

Single copy price, Lire 3,500 - $8.00
Specimen copy, Lire 500 - $1.50

C/C Postale No. 3-20176 "SCIENTIA,, Rivista di Scienza - MILANO

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“SCIENTIA„

(RIVISTA DI SCIENZA)

Rivista internazionale di sintesi scientifica - International Review of Scientific Synthesis

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STAMPATO IN MILANO PRESSO LO STABILIMENTO DI ARTI GRAFICHE SO. PO. AL.

1907 - 1971

I 65 ANNI DI "SCIENTIA„

Nell'invito a collaborare al 65° anniversario della Rivista « Scientia », la Direzione afferma che nei decenni che furono i più aspri della recente storia italiana, la pubblicazione è stata al centro della comunità degli studiosi tra i quali si è posta come connettivo. Condivido pienamente questo giudizio. In realtà le sue pagine sono servite a far conoscere all'estero il più avanzato pensiero scientifico italiano e ad offrire agli studiosi italiani i risultati più maturi dell'opera degli scienziati stranieri, e l'attività di « Scientia » è stata definita giustamente come « una conversazione mondiale tra grandi pensatori ».

Per quel che riguarda gli argomenti trattati, essi hanno sempre rappresentato quanto di più avanzato si poteva pensare o dimostrare in tutti i rami del sapere, sicchè i volumi di « Scientia » segnano delle tappe importanti nell'evoluzione del pensiero scientifico.

È interessante ricordare quanto scrivevano nel primo fascicolo della Rivista, apparso nel 1907, i fondatori - G. Bruni, A. Dionisi, F. Enriques, A. Giardina, F. Rignano - delineandone il programma:

« I resultati dello sviluppo analitico della Scienza furono celebrati fino a ieri come incondizionato progresso, imperocchè la tecnica differenziata e l'approfondita preparazione di coloro che coltivano un ordine di studi ben definito, recano in ogni campo del sapere acquisti importanti e sicuri.

« Ma a tali vantaggi si contrappongono altre esigenze che il particolarismo scientifico lascia insoddisfatte, ed alle quali si volge con maggiore intensità il pubblico contemporaneo.

« L'azione individuale e sociale, per cui il sapere è richiesto come istrumento, e la tendenza ad unità del pensiero, tutti i bisogni reali ed ideali della vita onde la Scienza procede e di cui la Filosofia si fa interprete, convengono nell'affermare la sintesi meta superiore di ogni progresso ».

Questi concetti non soltanto sono tuttora validi, ma si sono ulteriormente rafforzati. Mano a mano che la scienza progredisce attraverso specializzazioni sempre più spinte, è sempre più profondamente sentita l'esigenza di una sintesi del sapere. Tale sintesi, se pure è divenuta oggi di difficile costruzione, appare tuttavia indispensabile se si vuole tentare di intravedere dove vanno l'uomo e la società.

Infatti la più recente evoluzione della scienza ha posto in primo piano il problema dei rapporti tra l'uomo, la società e l'ambiente.

Molti risultati pratici resi possibili dalla scienza hanno aspetti negativi per l'uomo e i suoi valori.

Si è posto così il problema dei rapporti tra scienza ed umanità.

Astrattezza e neutralità della scienza, distinzioni tra conoscenza da una parte e possibili uso ed abusi di essa dall'altra, sono problemi che si pongono oggi all'attenzione di tutti, affinchè non si determinino mutamenti irreversibili nel sistema ambientale, siano individuate possibili alternative tecnologiche e si pervenga a un auspicabile controllo sociale delle tecnologie.

Orbene, i controlli sociali dell'innovazione tecnologica non saranno facili fino a quando essi non saranno basati su sicure conoscenze obiettive.

Qui la scienza è chiamata, pertanto, a ridurre al minimo l'ambito di errore e di arbitrio.

Ne deriva quindi l'esigenza di una continua visione globale dei vari problemi, non soltanto a fini operativi e di azione politica, ma anche al fine di far salvi i valori di personalità, libertà e giustizia. Come scrive lo psicologo americano James Frank: « L'ideologia della scienza è probabilmente il più potente antidoto contro il complesso di conoscenze tribali che ancora minaccia di mettere in fiamme il mondo. Poichè questa ideologia - che apprezza lo sforzo di ricerca invece della certezza dogmatica, che rispetta le differenze di opinione invece di condannarle, che unifica gli uomini invece di dividerli; - produce benefici sempre maggiori per il genere umano, essa è destinata ad affermarsi sempre di più nelle menti degli uomini di tutti i paesi ».

Questo il quadro suggestivo del recente progresso scientifico e della laboriosa definizione del ruolo che la scienza assume per l'uomo e per la salvezza del suo destino.

In tale contesto la Rivista « Scientia » ha avuto la ventura di partecipare e concorrere alla grande vicenda.

V. CAGLIOTI
Presidente del C.N.R.

TRANSLATION: J. LUBBOCK - ROME

THE 65 YEARS OF "SCIENTIA,,

In the invitation to contribute towards the 65th anniversary of the Review « Scientia », the Editorial Board states that, in the decades constituting the hardest years in recent Italian history, the publication stood at the centre of the community of scholars, among whom it formed a kind of connective tissue. I fully share this opinion. In reality, its pages have served to acquaint readers abroad with the most advanced scientific thought in Italy and to offer Italian scholars the most mature results of the work of foreign scientists, and the activity of « Scientia » has rightly been defined as « a world conversation between great thinkers ».

As regards the subjects covered, they have always represented the most advanced thought and activity in all branches of knowledge, such that the volumes of « Scientia » mark important stages in the evolution of scientific thought.

It is interesting to recall what was written by the founders - G. Bruni, A. Dionisi, F. Enriques, A. Giardina and F. Rignano - in the first number of the Review, which appeared in 1907, in outling its programme:

« Until recently, the results of the analytical development of Science had been celebrated as unconditioned progress, inasmuch as the differentiated technique and thorough training of those that cultivate a well-defined order of studies make definite and important contributions to every field of knowledge.

« However, set against these advantages are other needs that scientific particularism leaves unsatisfied and to which the contemporary public turns with greater intensity.

« Individual and social action, for which knowledge is required as an instrument, and the tendency towards unity of thought, all the real and ideal needs of the life from which Science proceeds and of which Philosophy becomes the interpreter, agree in affirming synthesis as the higher goal of all progress ».

These concepts are not only still valid today but have been further strengthened. As science progresses towards increasingly extreme specialisations, the need for a synthesis of knowledge is more and more deeply felt. This synthesis, even if it has become difficult to construct today, nevertheless appears indispensable if an attempt is to be made to try to see where man and society are heading.

In fact, the most recent evolution of science has given extreme prominence to the problem of the relationships between man, society and environment.

Many practical results made possible by science have negative aspects for man and his values.

Thus the problem of the relationships between science and mankind has arisen.

The abstract and neutral nature of science and the distinctions between knowledge on the one hand and the possible uses and abuses of it on the other, all these are problems that are nowadays drawn to the attention of all, in order that irreversible changes may not take place in the environmental system, that possible technological alternatives may be identified and that a desirable social control of technologies may be achieved.

Now social controls of technological innovation will not be easy until they are based on sure objective knowledge.

Here, science is therefore called upon to reduce to a minimum the field of error and arbitrary action.

Hence, therefore, the need for a continuous overall view of the various problems, not only for operative purposes and aims of political action but also to safeguard the values of personality, freedom and justice. In the words of the American psychologist James Frank: « The ideology of science is probably the most powerful antidote against the complex of tribal notions that still threaten to set fire to the world. Since this ideology - which appreciates the effort of research instead of dogmatic certainty, which respects differences of opinion instead of condemning them, and which unifies men instead of dividing them - produces ever greater benefits for mankind, it is destined to assert itself increasingly in the minds of men of all countries ».

This is the impressive picture of recent scientific progress and of the arduous definition of the role assumed by science for man and for the salvation of his destiny.

It is in this context that the periodical « Scientia » has had the fortune to participate and compete in this great adventure.

V. CAGLIOTI
Presidente del C.N.R.

LA CULTURA E LA VITA DELLA SOCIETÀ

I COMPITI DI "SCIENTIA,,

Il primo fascicolo di questa Rivista vide la luce nel 1907: non era trascorso mezzo secolo da quando l'unità d'Italia aveva ricevuto il suo formale suggello. Era, del resto, una unità fondata piuttosto su antiche propensioni oratorie che su una reale coerenza delle parti sommariamente giustapposte. E appena nove anni erano passati dalle sanguinose giornate milanesi del Bava Beccaris: non molto era cambiato da allora. Restava immutata la condizione di povertà che stringeva gran parte degli italiani; i moti di protesta, non ancora illuminati da consapevolezza politica, nascevano per filiazione diretta dall'indigenza. Restava immutato il sostanziale dispotismo di una borghesia che, per assolversi da ogni colpa, si fingeva l'immagine di una società popolata di padroni generosi e di servi paghi e grati. In realtà da questa Italia serena e composta, incline alla solennità, confortata da infinite certezze, molti fuggivano sospinti da bisogno: nel solo 1906 erano emigrati 788.000 italiani.

Che senso poteva avere, in un paese ancora prevalentemente contadino, parlare di cultura? Poco meno della metà degli italiani (il 48 per cento nel 1901) erano analfabeti; per gran parte degli altri lo scrivere e il leggere erano rozzi strumenti appena adeguati a necessità elementari, non certo veicoli di conoscenza o articolati mezzi di espressione. Il territorio culturale italiano era, così, rotto da un rigido confine: di qua, in una regione angusta e segregata, i pochi ai quali il censo aveva dischiuso le porte del sapere; di là i più, poveri e ignari.

Ma accadeva, di più, che lo stesso discorso fra i detentori della cultura non fosse più coerente di un dialogo fra sordi. Innumerevoli differenze, distinzioni, suddivisioni confondevano i linguaggi, creavano diaframmi e barriere. Il patrimonio culturale italiano, già impoverito dalla lunga immobilità, era scomposto in frammenti: e chiunque ne possedesse uno non si curava d'altro. Da un lato, intanto, i devoti al culto della parola - retori intenti a operazioni verbali sempre più sottili, sempre più vacue - lasciavano che la realtà, le cose, i fatti scorressero sotto i loro piedi; dall'altro gli uomini di scienza, divisi dai fili spinati di chiuse discipline specialistiche, coltivavano ciascuno il suo esiguo campo, senza mai volgersi a guardare quello attiguo.

Fra quegli steccati, fra quelle distinzioni, « Scientia » volle porsi come tramite, assumendosi il compito di comporre in una immagine articolata, ma unitaria e coerente, quel che era artificiosamente diviso.

A darle vita fu un gruppo di giovani studiosi: G. Bruni - A. Dionisi - F. Enriques - A. Giardina - E. Rignano - di matrice diversa e diversi per interessi, ma uniti dall'esigenza di ricondurre il discorso della scienza nell'alveo di una ricomposta unità, così da restituire ad ogni sua parte - insieme alla intellegibilità perduta - consapevole interezza.

« L'organamento attuale della produzione scientifica - scrissero nell'illustrare i fini che la Rivista si proponeva - trae la sua propria fisionomia dal fatto che i rapporti reali vengono circoscritti entro discipline diverse, le quali ognora più si disgiungono secondo gli oggetti e i metodi della ricerca ». Di qui, aggiungevano, la perdita di ogni capacità di sintesi: « A questa fa ostacolo la differenziazione delle discipline particolari, sia perchè lo sviluppo del linguaggio tecnico rende ognora più inaccessibili i risultati di una disciplina ai cultori di un'altra, sia perchè la stessa preparazione approfondita richiesta nei singoli rami di studio restringe la veduta dei problemi a taluni aspetti che lo studioso è tratto a contemplare troppo esclusivamente ».

« Contro codesti criteri ristretti intende reagire soprattutto il movimento nuovo di pensiero verso la sintesi; una Filosofia, libera dai legami diretti coi sistemi tradizionali, sorge appunto a promuovere la coordinazione del lavoro, la critica dei metodi e delle teorie e ad affermare un apprezzamento più largo dei problemi della Scienza. Pel quale il particolarismo stesso viene compreso in un aspetto più adeguato nella interezza del processo scientifico. Espressione ed organo di questa tendenza vuole essere la nostra Rivista, che nella misura del possibile si volge appunto a congiungere gli sforzi degli studiosi, innalzando la visione degli scopi scientifici sopra le forme particolari della ricerca ».

Considerare il cammino della scienza come un processo unitario e non più come una serie di itinerari sommariamente paralleli, ma fra loro dissociati, significava, in qualche modo, ancorare il sapere ad una globale realtà umana, sottraendolo al clima rarefatto di un aristocratico disinteresse, di una siderea neutralità; significava, in definitiva, fare di esso un patrimonio non più frazionato in mani privilegiate, ma, nella sua interezza, semplicemente umano.

Non paia incongruo il richiamo: ma almeno due dei fondatori della Rivista - Eugenio Rignano e Paolo Bonetti - avevano misurato con la realtà il loro convincimento che la cultura sia fatta per l'uomo e a tutti gli uomini spetti: da esso nascevano le iniziative legate all'Università popolare, quali la pubblicazione di volumetti di limpida divulgazione o i corsi tenuti, per un uditorio composto prevalentemente di operai, in umili sedi. In tempi ancora immaturi quei tentativi potevano avere soltanto efficacia ed ampiezza limitate; e tuttavia ad essi si deve attribuire il senso di uno sforzo inteso a connettere la cultura alla vita della società.

Verso questo stesso fine, pur se lungo altra via, « Scientia » procedette ben oltre negli anni successivi, pubblicando una inchiesta sulle cause della crisi internazionale che stava ormai per dare l'avvio - si era nel 1914 - a cinque anni di massacro. Vi collaborarono illustri uomini di scienza diversi per origine - erano francesi e tedeschi, inglesi e austriaci, russi e ungheresi - per interessi, per esperienze: li accumunava il desiderio di impiegare gli strumenti metodologici della scienza per scoprire, al di là dei fatti, le loro cause e intenderle nella loro reale tessitura, nel loro intrecciarsi: ben al di fuori, comunque, da ogni schema partigiano. A questa prima inchiesta, mentre ancora durava il conflitto, ne seguì un'altra, ugualmente intesa a valutare l'amara realtà di quegli anni con serenità e rigore. Nè l'intento della oggettività escludeva una partecipazione viva e dolente; non l'avrebbe consentita, del resto, l'argomento stesso: quali sarebbe stati, per l'Europa lacerata, i frutti della lunga guerra.

« Scientia », in definitiva, chiamava gli uomini di studio ad assumere un ruolo ben diverso da quello di spettatori, quanto si voglia acuti e attenti, ma distaccati e lontani. Anticipava così sviluppi che alla cultura italiana erano ancora sostanzialmente ignoti; superava, comunque, il formalismo delle classificazioni astratte, con la piena consapevolezza di attenersi in tal modo fedele ai propositi espressi dai suoi fondatori. Nè da questi propositi si scostarono coloro i quali ressero le sorti della Rivista negli anni che seguirono, che furono densi di amarezza, di asprezza. E vennero, infine, le inquietudini di un mondo che la catastrofe aveva cambiato dalle radici e al quale si rivelavano nuovi orizzonti.

A « Scientia » furono di conforto, anche allora, la collaborazione e l'appoggio di studiosi insigni, che alla Rivista andavano offrendo, ben più che i ragguagli delle loro ricerche, la testimonianza di un impegno non soltanto intellettuale. La Rivista assolve, sia pure in parte, ad un obbligo di riconoscenza ricordando, in un indice generale, il prezioso contributo di ciascuno; di uno in particolare - Federigo Enriques, che le fu guida autorevole nel corso di decenni - ritiene doveroso ricordare la figura e l'opera con una serie di scritti che appariranno nei fascicoli di questa annata.

A tutti « Scientia » deve la serenità con cui può guardare oggi ai 65 anni trascorsi, come anche ai compiti che la attendono nel futuro. A questi compiti vuole assolvere ponendosi come tramite tra gli studiosi, così da ricostituire in unità quella complessa immagine del mondo che il procedere delle singole discipline è parso talvolta scomporre in frammenti; nè vorrà ignorare le responsabilità che spettano all'uomo di scienza in un mondo mutato, stimolato da ansie fin qui sopite, tormentato da infinite contraddizioni; in un mondo che ha visto cadere antiche certezze e conosce oggi nuovi timori e nuove speranze.

« SCIENTIA »

Translation: G. Gabella · London

THE TASKS OF "SCIENTIA„

The first issue of this Journal appeared in 1907 - less than half a century since Italy's unity had been formally constituted. It was in fact a unity founded rather upon long-lasting vague arguments than on real fitness of parts summarily brought together. And it was just nine years since Bava Beccaris' blood-stained days in Milan: since then little had changed. Unchanged was the grinding poverty holding down the majority of Italians; protests and riots, still not enlightened by political consciousness, were arising directly from poverty. Unchanged was the essential despotism of a middle class which, to absolve itself from any guilt, imagined a society inhabited by generous masters and contented, grateful servants. As a matter of fact, many were forced out from this Italy which was composed and serene, and somewhat prone to solemnity; only in 1906 788,000 Italians emigrated.

What sense would it make, in a country still mainly agrarian, to talk about culture? Slightly less than one half of the Italians (48 per cent in 1901) were illiterate; for the majority of the others writing and reading were rough instruments adequate to elementary needs, hardly vehicles of knowledge or articulated means of expression. The Italian cultural territory was then divided by a rigid barrier; on one side, in a narrow and élite group, some of whose incomes opened to them the doors of knowledge; on the other side the many, ignorant and poor.

Moreover, it happened that the talk itself among the cultural class was similar to a dialogue between the deaf. A great number of differences, distinctions, divisions, mixed up languages, creating schisms and barriers. The Italian cultural heritage, already impoverished by long-lasting stagnation, was split into fragments. Each of these group was inward-looking and unimaginative. Meanwhile on one side those people who worshiped the power speech, consisting of a great deal of subtle but empty rethoric, let things drift on; on the other side scientists, split up by the barbed wire of closed specialized subjects, cultivated their own narrow field, without ever running their eyes over the adjacent fields.

Among these divisions, « Scientia » aimed to make connections, undertaking for itself the task to build up an articulated, but unitary and consistent picture of what was artificially split. It was brought to life by a group of young scholars: G. Bruni, A. Dionisi, F. Enriques, A. Giardina, E. Rignano - with different backgrounds and interests, but united by the eagerness of leading scientific culture into a reconstructed unity, giving back to each one of its parts their lost intellegibility and a conscious whole.

« The present organization of scientific production - they wrote illustrating the aims of the Journal - is shaped and characterized by the fact that actual relationships between various disciplines are circumscribed and more and more separated into the objects and methods of the research ». Hence, they added the loss of all ability to synthesis. « This is hampered by the differentiation of particular disciplines, because the growing of technical language makes the findings in a field less and less accessible to the students of another, and because the refined training needed in the individual branchs narrows the vision of the problems to aspects which the student tends to consider single-minded ».

« The new cultural movement towards synthesis mainly reacts against these narrow criteria. Its Philosophy was to free itself from direct relationships with the traditional systems, to promote co-ordination of work, criticism of methods and theories, and to establish a broader appreciation of scientific problems. Thanks to this particular aspects of research became more adequately integrated in the scientific process as a whole. Our Journal intends to be an expression and mouthpiece of co-ordinating the efforts of scholars, raising the aim of scientific ends above singular aspects of research ».

The course of science as a unitary process rather than a set of routes roughly parallel but dissociated - this amounted in some way to a linkof knowledge with an overall human reality, releasing it from the rarefied climate of aristocratic indifference, and neutrality. This achieved, ultimately, a structure no longer split up in priviledged hands, but, as a whole, truly human.

Two at least of the founders of the Journal (Eugenio Rignano and Paolo Bonetti) had matched with this reality their belief that culture is made for men and that it is due to all men. From the same belief arose those initiatives related to the Popular University, like the publication of clear simple books of popular exposition or teaching courses given in humble places for an audience composed mainly by workers. Times, however, were not yet ripe for this and those attempts were doomed to have limited range and effectiveness; but they clearly showed an effort to link culture with social life.

Towards the same end, although along a different line, « Scientia » went much further on during the next years, publishing an inquiry on the causes of the international crisis which was about to start five years of massacre (we were in 1914). Scientists with different backgrounds contributed to it - they were French and German, English and Austrian, Russian and Hungarian - with different interest, with different experience: they had in common the eagerness to use methodological instruments of science to uncover the structure of facts and to understand without bias their interrelationships. This first inquiry was followed, while the war was still going on, by a second one equally orientated towards evaluating the bitter realities of those years with objectivity and impartiality. But the aim at objectivity did not exclude a deep and sorrowful participation. As a result of the long years of war, however, it was impossible to remain dispassionate on this subject.

« Scientia » urged scholars to undertake quite a different role from that of spectators; that is acute and attentive, but at the same time distant and uncommitted. It was thus anticipating developments still unknown to the Italian culture; and it surpassed the formalism of abstract classification, fully aware of faithfullness to the aims formulated by the founders. Those who ran the Journal in the following years, of bitterness and harshness, did not stray away from these aims. And finally there were the disturbances of a world which had been radically altered and was discovering new horizons. It was a great encouragement for « Scientia » to have the support and contribution of outstanding students, who were giving to the Journal not just technical reports of their investigations, but a token of intellectual commitment.

« Scientia » now wants to fulfil partly a moral obligation of gratitude by remembering the valuable contribution of each one in a general index. On of them was Federigo Enriques, who headed the Journal over many years. We would like to honour this man and his work with a series of papers to appear during this year.

« Scientia » owes the satisfaction with which it can look back on its 65 years of life, as well as to its future tasks, to all these collaborators. Among its future tasks is an attempt to make connections between the scholars so that it is possible to build up as a unity that complex picture of the world which the growth of the individual field seemed sometimes to split into fragments. Moreover it will pay full attention to the responsibilities of the scholar in this changed world; this world stressed by worries so far soothed; tormented by endless contradictions; a world which is seeing ancient certainties crumble and is now experiencing new fears and new hopes.

" SCIENTIA „

FREE OF INFLUENCE

We speak sometimes of things being free of others or we speak of things as free of influence from something else. The most effectively explicative case of this is where we refer to physical things and events. Upon examination, this physical sense of freedom reveals a need for showing varieties of interpretation and the relevance of particular contexts and circumstances. This physical case can then be employed as a sort of model for examining: (1) political and social freedom, (2) personal freedom (voluntary and involuntary actions), (3) moral problems of freedom (guilt, responsibility, punishment, identity, obligation). It may then be concluded that most, if not all cases of freedom can best be understood if seen as closely analogous with the physical sense. This means that we must ask ourselves what *it is that is free, what it is supposedly free of influence from, and what the end, purpose, aim, prevision or hypothesis is for which the freedom is being considered.*

We sometimes speak of the freedom of inanimate physical things — or animate things considered as mere physical entities — and we say one such physical thing is free of another, or *free of influence from* another, meaning that it is not causally connected or conjoined with another in such a way that we can or need make significant statements about their connection, or the results of their connection. We would get at something of the same notion of freedom if we said that something need not be taken into account in the circumstances; or if we said that in a particular case something was irrelevant to the circumstances or to our purposes. And somewhat similar to this use of freedom as freedom of influence from, is the sense in which we say a thing is free from harm.

We may argue and dispute about such freedoms, but it is always in relation to specific, particular problems. That is to say, it is a matter of practical or purposive consideration. To what extent, we might ask, is an earth satellite free of influence from Jupiter? Well, for practical purposes of navigation on a specific mission, perhaps we need not consider Jupiter at all, but for purposes of great accuracy of prediction over a long period of time, the influence of Jupiter may be relevant or significant, and may have to be taken into account.

To take more obvious examples; we would never dispute a ship was free of the mooring-buoy once the cable had been slipped: or the satellite free of the launching pad once it was in orbit. More obvious still; we in Canada are free of any polluting influence from the smokestacks of Perth, Australia. But again, in any of these instances we can say that for certain purposes or in certain contexts there may *not* be a total freedom of influence of one thing over another. Indeed, we may be explicitly and practically interested in showing where or in what sense such freedom is *not* relevant.

It is worth noting that our new scientific techniques may improve our ability to see the significance of data previously unknown or thought to be of no importance. The Van Allen belt, enzymes, X-rays, electricity, all are examples of discoveries which have had an effect on our understanding of freedom from certain influences; for at one time, our ignorance of their existence might have led us to think certain things were free of influences which we now claim to be associated with radiation, electricity, etc. Negatively, one supposes we have largely eliminated the influences of such things as phlogiston or witches' brews. For, in believing in witches or phlogiston we presumably would not have been free of influence from them. And we would have taken them into account in our deliberations and choices.

Perhaps then, we may say that we very often apply a sort of relativity to our notion of freedom. Yet this relativity is clearer than any notion of relativity applied to morals or values: for it seems that with *freedom* we can delineate any situation in quite unequivocal practical or purposive terms; and even when we are inclined to argue, we do so only when we suspect that we do not have all the materials of argument available — as we might do if we suspected the influence, for example, of a yet to be fully discovered planet or a yet to be isolated chemical constituent. But when we apply this same sense of relativity to morals we must assume a particular sort of interpretation of the very nature of moral pronouncements, an interpretation which may rob them of their particular character by ignoring the fact that we may value a moral system in which we are not participants, more than we value the one in which we *are*. Our moral preferences are not simply relative to our cultural position in the sense in which they *are* nothing more than our cultural position.

An uneasiness about this relativity is perhaps why I shall avoid the Free Will, Determinism, Indeterminism sort of controversy. In these battles the Determinist camp seems methodologically the safer place. But until we think of ourselves as contained within some very definitely unified, consistent whole of things, we cannot think of Determinism as an assured doctrine involving its concept of absolute predictability. In other words, the fundamental metaphysical commitment of any completely Determinist position seems so much the point at issue that any subsequent argument would seem to have to be clarifying rather than controversial.

Surely, too, we must be tired of realizing that no experience will ever thoroughly bear out what we painfully assume for most of our practical moves, namely that the world will *continue* in the same form or order. What Determinism deals with is the theoretical projection of this practical attitude. And it is a theoretical cul-de-sac. For even to come to some proof or 'end' of the argument would have to be at the same time to come to the end of all experience and to *know* it. And what an obvious paradox is here!

So I shall stay with the less disputable sense of « free of influence from » and examine it further not only from the already more obvious physical point of view but from others where it works rather more than by mere analogy.

(1) *The physical sense.* Although this is the sense I have already set up as the most effective explicative case, we can nevertheless realize a little more systematically how such freedoms can be categorized. A few examples should suggest the general sense.

Sometimes, when we say that a thing is physically free of influence from another we may simply be pointing out that it is so spatially distant from another that it would not influence it to any interesting degree or for most practical purposes. The distance at which influence seems beyond consideration will differ as it would with the different powers of magnets and the spatial extent of their fields, and it will differ among the various classes of influence. Sometimes, indeed, the *closer* an object gets, the less it will influence another. For certainly, beyond a certain point, the closer I get to a lens, turned towards the sun, the less its ability to concentrate heat and light will influence me.

When a physical event or thing is temporally a great distance from another we may think of one as free of influence from the other. This applies to distances in both the past and the future. With some past events we would quite obviously never consider their influences on particular present facts. The ice jams on the Volga in 1300 would only facetiously be linked with my present sneezing. However, the death of a dinosaur a million years ago has an obvious enough connection with my discovery of its bones just yesterday. But both of these examples might serve just as well in the spatial sense. Are there, then, more purely temporal instances? I think perhaps this would vary with our views on the nature of the physical world and the way in which we would co-ordinate time determinations.

It is sometimes difficult to see freedom of influence from certain physical events of the past. It is more difficult to see how any influence at all can come from the future except through present anticipation. Yet tomorrow's weather may affect today's planting as significantly as yesterday's. But the weather for 12th May 1998 is something we seem reasonably free of influence from in our anxieties over this year's crop of beets. Again, as with spatial distance, the *nearness* of a time may remove it from influence; for certainly my most recent heart beat is less influential in the present state of circulation of blood in my feet than the previous beat would be.

Freedom of physical influence is often understood in terms of certain 'categories' of influence, as with the freedom of platinum form the chemical and corrosive effects of certain acids. However, no one would want to claim that the same platinum was not, say, stirring (influencing) acid when moved about in it. Again, if we think of light as physical we may want to say that it does not normally influence the *speed* or *taste* of something upon which it shines. Weight, heat, light, density, mass, speed, all measurable items, may or may not be relatable as influences.

Within any of the categories such as weight, we can speak often as though the differences between two entities were so great that their influence would be so small and insignificant as to be beyond measurement. If a feather blows against a ten ton truck one hardly sees the need for seeking its influence on the behaviour of the truck. The driver does not feel the wheel wrench under his grip.

(2) *Political and Social Freedom.* Political freedom, by analogy may turn out to be not unlike the above sense. We are, as individuals, politically free in those areas where the police or other state structures and organizations are unlikely to influence or interfere with our activity — to cause us to behave or not to behave in certain ways. Even borders may define our freedom.

But freedom from influence in the political sense may also call for an examination of attitudes or states of mind as they control us or as they release us from control by causing us to avoid certain influences. The psychological effects of state control by education, laws, propaganda, rewards, punishments or threats of punishments and promises of rewards always have to be examined and applied. And here, I should maintain, we would apply them just as we would in the case of the physical elements in the fundamental, explicative example. That is to say, I would say that I was free of influence from some of these specific promises of reward, punishment, etc. or even free of influence from educational techniques now operating in schools, but which did not operate when I was a student. My age, sex, health, vocation, residence, wealth may all play a part in determining how free I am of particular possible influences.

The fact that the psychological elements of a political analysis are more difficult of access or more susceptible to metaphysical speculation shouldn't cause us to alter our theoretical understanding of their application to the problem of freedom. Furthermore, whatever speculations and theories are employed they will come up as part of the fabric of influences.

Again, it would be wrong to think of all political laws and/or systems as restrictive — always curtailing or directly influencing our activity, or always taking away our freedom. Some of them produce activities and are creative. In other words, they may free us in some way to do something we would otherwise find obstructed (Even the rules we must follow in driving our cars, for example, free us from the most frightening possibilities). But in saying that our rules may free us, we must not suppose that we are doing so by way of defending democracy. Democracy is not necessarily the source of system of the highest freedoms for *all* individuals or types of individuals. There are cases where creative persons may be the sorts who will be somehow freed from certain influences and will operate in their fields more effectively under rather dictatorial conditions. Ordinarily, however, we define a dictatorship as a political condition where few freedoms prevail, however much there may be personalities who thrive in their special aptitudes only when the state paternally and dictatorially removes or adjusts certain of their responsibilities. And lastly, some people obviously take delight in risking breaches of rule and dangerously ignoring possible influences of custom or law.

Social as contrasted with political freedom is simply tacit rather than constitutional or legislative. We can cover our understanding of social freedoms by seeing them as tacit in this way, and by remembering the great complexity of attitudes, states of mind, etc. as they control and influence our social reactions outside the more mechanical areas of political influence.

Political and social freedoms are not fixed. They change as our techniques of influence and power change. Psychology and sociology, for example, will allow us new powers over others (or freedom from others) which may have to be dealt with by law, custom, or counter-action. Again, as in the case of the physical sciences, the social sciences may even help discover how we were influenced in the past when we thought we were free. We certainly seem to be discovering that we were more profoundly influenced than we suspected by our up-bringing and educational systems.

Some of our increasing knowledge allows us to treat political problems like scientific ones, and we may treat them for positive or negative political ends, or we may simply objectively observe them. That is to say, the social sciences are Janus-faced; while, on the one side, they may favour our freedoms, they also provide instruments for our political bondage.

There is another rather rarer sense in which we may speak of political freedom; for we may ask how political groups (entities) themselves are free and we may perhaps answer that they are quite obviously relatively free or not free of influences from *other* political groups in the sense in which Canada, for example, would be relatively free of influence from Albania, but not all that free of influence from the U.S.

From another perspective, states themselves seem to be free to the extent that their system of law allows for internally *agreed* change or stability. Constitutional freedom of this latter sort would exist if a plebescite in any form were allowed and honoured, etc.; or if political parties *always* suggested and represented the most appropriate alternatives. This is to say that the system is free of static influences from its own structure and constitution, and that different and opposing causal complexes will operate within its changing orders.

But under this heading (2) of Political and Social Freedoms I am not trying to elaborate any particular political theory. I am simply trying to suggest that whenever freedom is a significant issue we see this issue most effectively as clearly analoguous with the case of physical freedoms. So there is a very real sense in which we, as political creatures, do achieve freedoms regardless of the political system we favour or exist in; but we always do so only by regarding certain possible influences on specific occasions. Indeed, we *achieve* freedom by manoeuvering away from or towards the most appropriate influences and not by trying to eliminate influences altogether in an spurious attempt at empty independence and anarchy. And I don't think it is viciously regressive to suggest that our attempts to achieve freedom will also come under the very analysis we are discussing. That is to say, we will be free or not free of influences from the various factors we are able to introduce but we will never be free of the fact that we are trying for a specific freedom — trying to avoid a specific influence, force, obligation, etc. The reflective individual is affected by his own reflectiveness.

(3) Throughout the history of ideas we have persistently and effectively clarified freedom construed in terms of voluntary and involuntary actions — seeing this as a matter of the freedom of the individual. Here again, the first case will serve as the explicative one. But now we substitute for physical things, « self », « body-mind » or « person » etc.; and these entities may be seen as free of significant influence from other similar entities or from physical things, political things, and so on. Of course, in the Aristotelian manner, we would also have to say that I would be free of influence from something external to me and of which I was understandably *ignorant,* even though that thing clearly *did* influence my actions. That is to say, my particular *intentions* and *decisions* would not and could not be influenced by such items. In this section however, I exclude discussion of possible ignorance of my subconscious paraphanalia when it presumably includes a « knowledge » of something external of which I am consciously ignorant: for, if we accept the appropriate psychology, we must accept the influence of such items upon intentions, decisions, and actions, etc. And we will explain our freedom accordingly. Difficulties with such explanations would have to form the subject of another paper.

There are many ways in which we may see the freedom of the individual as dependent upon our particular psychology. If we accept the existence of an 'intellect', 'will', 'superego', 'dispositions', 'appetites', 'desires', etc., we may wish to say that one or more of these is free of influence from one or more of the others. Hence, the Freedom of the Will may be construed in this way and although one might not wish to accept the existence of a Will at all, we can nevertheless understand how, if one *did*, it could be imagined as more or less free of influence, say, from Intellect, Desire, or even God. It is in this same sense that we may argue about freeing ourselves from the influences of our emotions. And it is in this sense that Freedom itself gets the character of a substantive.

(4) Fourthly, there is the most puzzling sense of freedom suggested in the context of moral actions, decisions, and responsibilities. And

here I don't think we are ever quite so sure of ourselves; and I think this is because such freedoms involve a consideration and inclusion of all the other types so far encountered. As well, the exercise, as it were, of these moral freedoms, is dealt with in the confines of a set of regulations which might sometimes seem or even *be* (by anachronism) quite arbitrary. Moral rules and moral influences are influences and causes of an unusual sort.

But what specific sorts of problems do we really have in this area? I suggest the following as among the more significant; and I present them in what seems to me a reverse order of importance.

(a) Guilt and Freedom. Because I *feel* guilt, that is to say, a *subjective guilt* having nothing to do with the law, I naturally suppose I either should or could have done otherwise. But when I say to myself that I should have done otherwise I simply mean that, upon reflection, nothing seems to have been overwhelmingly and causally in the way of doing otherwise. That is to say, I was free of influences which might have *forced* me to refrain from what I ought to have done. Alternatively, I might very well recognize that the stronger causal case could be made for doing what I did do, and that my reasons for feeling guilt are that the more important causal elements for doing what I did do were from *within* my person, and that I am, in that sense, weak, and that I feel guilty for being the person I am and not so much for doing the thing I did do. Indeed, the fewer and/or less powerful the causal elements from within my person, the less guilty I am likely to feel. And all of this suggests that the freedom involved in my consideration of guilt is a freedom from social and external pressures. That is to say, when I use my sense of guilt as « evidence » for my freedom, I am sometimes referring to the fact that I largely caused *myself* to act in the way I did and was not coerced or forced by external causes and influences.

But guilt may be *objective*, when, in the legal sense, I am guilty or not guilty of something. Is there a sense of freedom relevant here? Surely there must be, for although one may be guilty or not guilty of doing a particular thing in the sense that one as a matter of fact did or did not perform the action, we normally mean 'guilty' to apply only to those cases where causal influences from external pressures *or* internal mental deformities were not present i.e., various forms of 'involuntary' or 'not voluntary' actions (Aristotelian). Legally, we worry about freedom of influence from these certain factors: and it is only as a *prelude* to legal procedure that we determine what was actually done, and what *actually* took place.

Much pathological guilt and confusion over moral responsibility comes from a confusion over the subjective and objective (legal) notions of guilt. The emotional involvements of the former pass over into the latter. Thus, we may induce in ourselves those frequently damaging states of mind where we think of ourselves as weak, unworthy, evil influences, and so on. Or we may be rightly perturbed over the way in which the law can hold certain unstable people responsible where we know their instability can be traced to this confusion.

(b) Punishment and Freedom. In some forms of punishment we suppose and hope that we are attempting to cause a person to alter his behaviour. We free him from some causal influences within his person by introducing a fear of repeated punishment or an awareness of the futility of his action.

In purely retributive punishment, the question of individual freedom does not seem to arise in any very special sense: although certainly it will be *justified* with reference to responsibility, guilt, etc.

(c) Identity and Freedom. Concern about identity gives rise to a peculiar ideal — the creature without an environment, the self-sufficient entity. We hope to reinforce our sense of identity and moral autonomy by asserting our individual freedom of influence from externals. But equally, we hope to support our concept of freedom by reference to our personal identity, In any case, from a moral point of view, we want our identity and individualism (our selves) to be very closely analogous indeed to the primary case of the purely inanimate, isolated system: the ship free of the mooring buoy, the city free of distant pollutions, etc. The very nature of identity (and individualism) demands freedom from external influences. He who becomes most self-determined therefore, is said to be the most free. He attempts either to eliminate or identify with the external world.

Historically, then, the relations of significance between personal identity and freedom have produced philosophers with the strictest sense of world order; and, frequently and paradoxically, the order of the individual has been applied as a model to the « whole of reality ». (Spinoza's Substance-God, Leibniz's Monads, the Existentialist's *existence,* and Whitehead's *actual entities*).

(d) Obligation and Freedom. This is surely the most difficult problem. But let us first realize that it would seem strange to think of obligation in connection with actions to which we were universally and/or unequivocally inclined or disinclined. That is to say, we would never say one ought to do something that everyone under all circumstances would do by inclination; nor would we say one ought not to do something that everyone under all circumstances would be disinclined to do.

We should now realize that obligation makes sense only in circumstances where significant causes or influences come into play *against* the relevant action. And *this* is why freedom comes so problematically and conspicuously into our thoughts about obligation. It is as though we always played a rather evenly matched *game* over each circumstance where the obligation applied — and with it and the other causes as players. But of course, each cause, as a « player » will have had antecedent causes affecting its playing ability; and our own decisions to use players in such-and-such ways will affect the wins and losses on each occasion.

Nevertheless, « to be obliged » is simply to be enjoined to go against, or to cause oneself to go against something one supposes one need not have gone against had the obligation not been introduced as a new course. Indeed we only too often have our obligations pointed out to us *after* we have unwittingly « misbehaved ». It therefore brings us closer to the truth to realize that in knowing what one « ought to do » we have introduced or become aware of another possible cause of our action. We have deliberately (and sometimes rather perversely) introduced another « player » into the game; and it sometimes looks like an exercise of imagination to do so; for we may attach « you are obliged to » to the most outlandish, unatural, notions.

Although the ought-can controversy is sterile, I should like to add that I find it impossible to conceive of an obligation which would not be shorthand for a sort of advice; if you want such-and-such, you ought to do this; i.e., if you want such-and-such, then the only sensible thing to do is this. And if this is an acceptable interpretation then there may very well be things we cannot do which we nevertheless ought to do — *if*..

This interpretation would then suggest that the significant influences operating in obligations are desires and needs. And these desires and needs conflict along a temporal, anticipatory and regretful course as well as immediately and among themselves. We may even have to anticipate them or project them for future generations.

Obligations may free us from certain other pressing causes and influences. Or obligations may bind us to their own influence. And sometimes, in forgetting the practical foundation of obligations, we allow meaningless « obligations » to direct our behaviour. That is to say, we mistakenly suppose that something is an obligation merely because the language of obligation is used in its connection. And we are mistaken, because the obligation makes no sense as advice. If this is pointed out to us, we normally try to drop the matter from our moral considerations, unless, of course, it has bound us emotionally or unless it is a condition for our holding to a larger obligation.

To this section we could add that, even though we were to construe obligations in a non-advisory sense and suppose that they somehow had their own rather special or metaphysical character, these obligations would then be no more than further factors from which we would or would not be free. But freedom is something the pursuit of which we may or may not subordinate to moral considerations. At our peril however, would we ignore these considerations altogether. And although some moral notions and pronouncements have and others will go the way of witches, it is hard to conceive of a world in which the notion of mere social engineering would take over the realm of morality.

(5) Without developing this as a part of the thesis, there is a last area in which I should say we have our greatest *sense* of freedom and where the most general questions comes to rest. And this is where we find ourselves exercising our philosophically objective skills upon the

many problems of philosophy, including the problem of freedom itself. For then we see the free « play » of theory, and the marvelous arbitrariness of many of our own choices of theory-in-practice.

What I wish to say is that we *choose* to be free or free of influence from things in accordance with some specific concept of freedom and in accordance with a specified set of objects. But the choice of relevant things and theories was a sort of higher choice; certainly it was a choice above specific choices, and certainly a peculiarly « free » awareness of our human condition. To be aware of those things which influence us and of those things which do not, is to have a state of mind independent of those things. This is to say, one supposes, that Reason, as a deliberate instrument or technique for achieving objective understanding, seems to be most capable of becoming free of influence. This is only to say we are free of influence from ignorance.

Reason and truth then, are the most likely things to set us free. And we can define our responsibilities, moral and otherwise, only in accordance with an objective view of our particular freedom. And we define responsibility sometimes rather arbitrarily only because we cannot always trust ourselves to detect the psychological equivalent of a missing planet or an as yet to be detected chemical constituent.

But the examination of freedom as outlined in this essay has resulted in my arguing that most and perhaps all cases of freedom are to be seen as valid analogies with the explicative case where we set out an interpretation of the freedom of inanimate things or animate things considered as mere physical entities. This means then, that we should always ask ourselves, *what* it is that is free, what it is *free of influence from*, and what the end, purpose, aim, prevision is for which the freedom is being considered. And we must humbly remember that, along with all the mundane objects of our environment, the most subtle beliefs, the most elusive errors, even the most extreme of imagined metaphysical or mythical entities may, as purely imagined things, lose, or, even more surprisingly, *gain* us freedoms. And although Reason may be our greatest possession, the irrational elements with which we wrestle and reason, may some-times be our saving grace.

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EINSTEIN'S PHILOSOPHY OF SCIENCE[1]

Although Einstein's chief interest for philosophers lies in his contributions to physics, he did write a number of avowedly philosophical articles. These are worth considering not merely on account of the eminence of their author but because of their intrinsic merit. Most of them are comparatively short but together they form a fairly coherent view of the nature and function of science; and one which will probably command the sympathy of many physicists and some philosophers. This article attempts to expound Einstein's views on the philosophy of science as coherently as possible. Various critical suggestions are made but these are, for the most part, subordinated to the task of exposition.

Einstein clearly regarded science as an extension of common sense. Both ordinary everyday thinking and science had, for Einstein, the same purpose: namely, to make sense of our sense experience. He defined science as 'the attempt to make the chaotic diversity of our sense experience correspond to a logically uniform system of thought'[2]. Science differs from common sense in that it involves (i) a more precise definition of concepts than common sense; (ii) a more painstaking and systematic choice of experimental material; and (iii) greater logical economy[3]. 'The whole of science is nothing more than a refinement of everyday thinking'[4]. In view of this continuity of purpose which Einstein finds between science and sommon sense it is perhaps best to start with the common sense construction of the concept of the external world.

Einstein deals with this problem most fully in his long essay 'Physics and Reality' (1936)[5]. He starts off by accepting as given 'sense experiences, memory pictures of them, images and feelings'[6] (these I shall refer to, inaccurately but conveniently, as « sense experience »); these he regards as the fundamental entities out of which science (and everyday thinking) grows. 'All knowledge of reality starts from experience and ends in it'[7]. Einstein makes two statements about the relationship of these sense experiences to the concept of a physical object which together are puzzling. He first of all denies that the concept of a physical object is logically identical with 'the totality of sense experiences referred to'. But immediately afterwards he asserts that 'this concept [of a physical object] owes its meaning and its justification *exclusively* to the totality of the sense impressions which we associate with it'[1]. This implies that whilst we may not regard bodily objects as logical constructions out of sense experiences, statements about physical objects have a meaning solely insofar as they refer to sense experiences. Einstein never demonstrates how statements about physical objects get their meaning from statements about sense experience and the relation between the two is left rather obscure at this stage.

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[1] In this article quotations will be enclosed within single quotation marks; double quotation marks will be used for shock quotes.

[2] 'The Fundaments of Theoretical Physics' (1940), in *Ideas and Opinions*, edited by Carl Seelig, (Redman, 1956), p. 323; hereafter cited as *IAO*. Also in *Out of My Later Years*, (Thames and Hudson, 1950), p. 98; hereafter cited os *OOMLY*. Cf., for a similar statement of intent, Carl Hempel, 'The Theoretician's Dilemma', in *Minnesota Studies in the Philosophy of Science*, vol. ii, edited by H. Feigl, M. Scriven and G. Maxwell, (University of Minnesota Press, 1958), p. 41.

[3] *Ibid.*, *IAO.*, p. 324; *OOMLY.*, p. 98.

[4] 'Physics and Reality' (1936), *IAO.*, p. 290; *OOMLY.*, p. 59.

[5] *IAO.*, pp. 290-323; *OOMLY.*, pp. 59-97.

[6] *Ibid.*, *IAO.*, p. 290; *OOMLY*, p. 60. This epistemological starting point has been severely criticized by several modern philosophers. Cf., for example, N. R. Hanson, *Patterns of Discovery*, (Cambridge University Press, 1958), chapter 1.

[7] 'The Method of Theoretical Physics' (1933), *IAO.*, p. 271; also in *The World As I See It*, (Bodley Head, 1935), p. 133, hereafter cited as *TWAISI*.

It becomes clear in the course of Einstein's essay that he is not championing a completely reductionist thesis. 'I do not consider it justifiable', he writes, 'to veil the logical independence of the concept [of a physical object] from the sense experience'[2]. In other words, a physical object is more than a collection of sense experiences. But what is the extra ingredient which seems to constitute the essence of a physical object? Einstein is not here making a distinction like Locke's distinction between primary and secondary qualities, although presumably the Einsteinian essence is immutable (as Locke thought his primary qualities were) whilst the sense experiences associated with a physical object change. But Locke's primary qualities were empirically grounded whilst Einstein's mystery ingredient isn't. Even Locke's « super-primaries », solidity and extension, won't do because both can be detected by the senses. It seems clear that the psychological origin of Einstein's mystery ingredient is Kant's notion of a *noumenon*[3], to which it has a superficial resemblance. I am not claiming the two notions are the same, but merely that Einstein probably had Kant in mind when he came to talk about physical objects.

It seems that Einstein's mystery ingredient is completely inscrutable and could be pruned away without empirical loss. The objection to this is that Einstein treated it as essential and we are not entitled simply to ignore it. '...we attribute to the concept of a bodily object', Einstein wrote, 'a significance which is to a high degree independent of the sense impressions which originally give rise to it. This is what we mean when we attribute to the bodily object « a real existence »'[4] Here he has named his mystery ingredient, though without dispelling the mystery. It would seem that a physical object consists of sense experience plus real existence. He continues:

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[1] 'Physics and Reality' (1936), *IAO.*, p. 291; *OOMLY.*, p. 60. (My italics).

[2] *Ibid.*, *IAO.*, p. 294; *OOMLY.*, p. 64.

[3] Cf., in particular, Kant's *Critique of Pure Reason*, translated by J. M. D. Meiklejohn, (Everyman Edition, 1959), pp. 186-191. We know that Einstein read Kant's *Critique* at a very early age and attended a course of lectures on his philosophy at University. It is very likely that we will find traces of Kant in Einstein's philosophical writings.

[4] 'Physics and Reality' (1936), *IAO.*, p. 291; *OOMLY.*, p. 60.

> 'The justification of such a setting rests exclusively on the fact that, by means of such concepts and mental relations between them, we are able to orient ourselves in the labyrinth of sense impressions. These notions and relations, although free mental creations, appear to us as stronger and more unalterable than the individual sense experience itself, the character of which as anything other than the result of an illusion or hallucination is never completely guaranteed'.[1]

The difference that « real existence » makes to a collection of sense experiences is that it ensures that the sense experiences are related to a physical object and are not hallucinations. But this is little practical use, for in attributing « real existence » we may make a mistake and thus Einstein should include the proviso that we attribute it correctly. But the correct attribution of « real existence » is nothing but the attribution of it to a physical object and not to an hallucination. This is perhaps what Einstein had in mind when he said that sense experiences are 'never completely guaranteed' to be anything other than hallucinated.

Thus it would seem that, according to Einstein, we formulate a concept of a physical object in something like the following manner: We are first of all confronted by a mass of sense experiences. We put them into order by *postulating* a physical object which will account for all the sense experiences (presumably by some causal theory of perception). The concept of a physical object cannot be abstracted from the sense experiences, it is postulated non-empirically and contains some reference to « real existence » which is nowhere related to the sense experiences. An illusion is presumably a similar collection of sense experiences which can't thus be put into order. Einstein might want to say that it lacked the attribute of «real existence» but as we have seen we have no means of knowing when to attribute « real existence » to such a collection. The empirical criteria for distinguishing between illusions and physical objects seem quite adequate. The central property of a ghost is its ability to walk through walls which we can't account for if we use our concepts of physical objects. But a ghost that couldn't walk through walls would be no different from an ordinary person and might safely be endowed with « real existence ».

Einstein's concept of a physical object is a 'free mental creation'[2], a 'free creation of the human intellect, which cannot be justified either by the nature of that intellect or in any other fashion *a priori*'[3]. Moreover, the formulation of all concepts is in this way non-empirical. (I think Einstein would agree on all concepts being 'free mental creations' but he never says so explicitly). Einstein's task here seems to be in several respects similar to that attempted by Russell in *Human Knowledge*. Russell had originally hoped to reduce physical objects without residue to sense-data but by the time he came to write *Human Knowledge* he had given up this programme and 'contented myself with a picture of the world which fitted physics and perception harmoniously into a single whole'[1]. Naturally Einstein did not want to sacrifice physics to skepticism any more than Russell did. Radical empiricism proved inadequate as a foundation for physics and therefore had to be replaced. Russell wrote:

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[1] *Ibid.*

[2] *Ibid.*

[3] 'On the Method of Theoretical Physics' (1933), *IAO.*, p. 272; *TWAISI.*, p. 134. Here, again, Einstein's philosophy seems to me to resemble that of Kant.

> 'It seems clear, therefore, that we all in fact are unshakeably convinced that we know things which pure empiricism would deny that we can know. We must accordingly seek a theory of knowledge other than pure empiricism'[2].

Russell worked out this programme in *Human Knowledge* where he investigated what minimum of non-empirical postulates would be required to save science and common sense. The answer he came to accept was formulated in his five principles of non-demonstrative inference. Einstein's treatment of the same problem was considerably less formal; he held that the gap between empiricism and science could be bridged imaginatively by intuition. The advantage of Russell's formulation is that it allows a rigorous reconstruction of a concept without any mysterious gaps in the deductions. Einstein's view is inherently mysterious; though I believe it corresponds better with the actual psychology of creation than Russell's view.

The way in which concept formation goes beyond empiricism leads Einstein to consider the relations between empiricism and rationalism. Both empiricists and rationalists have claimed Einstein as a champion but the truth is that his philosophy contains elements of both views. 'Pure logical thinking', he wrote, 'cannot yeild us any knowledge of the empirical world: all knowledge of reality starts from experience and ends in it'[3]. But shortly afterwards he writes: 'I hold it true that pure thought can grasp reality as the ancients dreamed'[4]. Stated flatly like this the contradiction seems complete, but it is necessary to disentangle Einstein's thought on the subject a little more carefully. Einstein's most explicit comment on the problem was in response to a remark by Henry Margenau. Margenau had said: 'Einstein's position ... contains features of rationalism and extreme empiricism'[5]. Einstein replied:

> 'This remark is entirely correct. From whence comes this fluctuation? A logical conceptual system is physics insofar

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[1] Bertrand Russell, *My Philosophical Developmant*, (Allen & Unwin, 1959), p. 105.

[2] Bertrand Russell, 'The Limits of Empiricism', *Proceedings of the Aristotelian Society*, (1936), vol. 36, p. 148.

[3] 'On the Method of Theoretical Physics' (1933), *IAO.*, p. 271; *TWAISI.*, p. 133.

[4] *Ibid.*, *IAO.*, p. 274; *TWAISI.*, p. 136.

[5] Henry Margenau, 'Einstein's Conception of Reality', in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*, (Harper & Row, 1959), vol. i, p. 247; hereafter cited as Schilpp.

> as its concepts and assertions are necessarily brought into relationship with the world of experiences. Whoever desires to set up such a system will find a dangerous obstacle in arbitrary choice (*embarras de richesse*). This is why he seeks to connect his concepts as directly and necessarily as possible with the world of experience. In this case his attitude is empirical. This path is often fruitful, but it is always open to doubt, because the specific concept and the individual assertion can, after all, assert something confronted by the empirically given only in connection with the entire system. He then recognizes that there exists no logical path from the empirically given to the conceptual world. His attitude becomes then more nearly rationalistic, because he recognizes the logical independence of the system. The danger in this attitude lies in the fact that in the search for the system one can lose every contact with the world of experience. A wavering between these two extremes appears to me unavoidable'[1].

This passage together with the remark we quoted earlier that science is the attempt to make our sense experiences 'correspond to a logically uniform system of thought' suggests that Einstein regarded a fully developed theory of physics as an empirically interpreted deductive system. The construction of the system is a task for the reason alone, but the interpretation is empirical. He emphasized this doctrine in an earlier essay 'Geometry and Experience' (1921):

> 'As far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.... geometry treats of objects which are denoted by the words « straight line », « point », etc. No knowledge or intuition of these objects is assumed but only the validity of the axioms... which are to be taken in a purely formal sense, i.e., as devoid of all content of intuition or experience. These axioms are free creations of the human mind.... Mathematics as such cannot predicate anything about objects of our intuition or real objects'[2].

In view of the close links which Einstein's general theory of relativity forged between geometry and physics we may take it that some of the attributes of geometry, described in the above passage, were taken over, in Einstein's view, by physics. Moreover, Einstein's later work on the unified field theory - in which the mathematical formalism is very highly developed and the physical interpretation very obscure - suggests that he thought that theories in physics should contain both rationalistic (or logical) and empirical elements. Of course, he did not mean that physics actually proceeded by deciding on axioms and then deducing theorems from them and finally giving the whole an empirical interpretation. The development of physics was, for Einstein, a marvellous blend of fact, logic and intuition: intuition being the most important. He merely claimed that the best scientific theories had a rigorous mathematical framework which could receive a valid physical interpretation. He did not lay down rules of interpretation, or even say in what way the interpretation ought to be worked out, that was for the physicist to discover as best he could using his intuition.

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[1] Albert Einstein, 'Reply to Critics' (1949), in Schilpp, vol. ii, p. 680.

[2] 'Geometry and Experience' (1921), *IAO.*, pp. 233-4; also in Albert Einstein, *Sidelights on Relativity*, (Methuen, 1922), pp. 28-31.

In view of the fact that Einstein denies that even « thing-concepts », let alone theoretical concepts, can be derived either logically or inductively from sense experience, he has to find some way of justifying the concepts that the physicist uses. He has three criteria by which he judges them: (i) No consequence deducible from the concepts - or, more accurately, from the statements of the theory which employ the concepts - should conflict with experience; (ii) The concepts of the theory should all be self-consistent and consistent with each other; and all the statements of the theory which employ the concepts should be both self-consistent and consistent with each other; (iii) The concepts of which the theory makes use should be useful in making sense experience comprehensible. By making sense experience 'comprehensible' he meant 'the production of some sort of order among sense impressions'[1]. The first two criteria are exactly what one might expect, but the third (which may be called the « pragmatic criterion ») is more distinctively Einsteinian. Einstein was prepared to distinguish between the value of scientific theories according to how much order they produced among sense experiences (or: among theoretical results - for we have now reached a stage of sophistication where it seems a little naive to talk of « sense experiences »).

The concept of ordering data was an important one for Einstein and one that deserves our consideration. Three factors seem to be involved in determining how well a theory satisfied the pragmatic criterion: (i) The quantity of experimental data which the theory orders; (ii) The logical tightness with which this data is connected to the concepts of the theory; (iii) The paucity of the concepts of which the theory makes use. We ought also to add the proviso that the third requirement should not be satisfied at the expense of the other two. I think some such criterion as Einstein's pragmatic criterion is actually in use (though perhaps unconsciously) in assessing scientific theories. In certain respects it might be called a criterion of depth. A lot of sciences seem to have started their history as what might be termed « classificatory sciences ». An example would be natural history which, in the eighteenth century, constituted the most important part of biology. The strongest instrument which natural historians possess for ordering their data is their system of classification.

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[1] 'Physics and Reality' (1936), *IAO.*, p. 292; *OOMLY.*, p. 61.

This is perhaps a poor relation of a scientific theory. As sciences develop, however, they become deeper and we now have explanatory theories of biology, and molecular biology and even mathematical biology[1]. In physics the development might be illustrated by the change from Tycho Brahe's observations of the planets, to Kepler's laws of planetary motion, to Newton's theory of gravitation. If we take explanation to be the subsumption of a phenomenon under a law, we can see that the explanation gets deeper with each stage of development. For example, Tycho's answer to the question « Why is such and such a planet in that part of the sky tonight? » would be because it was always in that part of the sky at this time of the year; Kepler would reply that it was because the planets followed his three laws; Newton, most grandly, would reply that it was because the universe was governed by his law of universal gravitation. At each stage (i) the law becomes more general, that is, the number of phenomena to which it applies increases; (ii) the formalism of the system increases, it becomes more mathematical (one wonders whether this applies to statistical thermodynamics); and (iii) the number of fundamental (i.e., primitive) concepts or laws is reduced (if we take into account the increased scope of the theory)[2]. It must be admitted that the universe becomes more ordered as the science used to describe it becomes deeper. The return to a (merely) descriptive theory in the Copenhagen interpretation of quantum mechanics, in an attempt to avoid metaphysics, was Einstein's most fundamental reason for rejecting the theory.

According to Einstein, science makes use of three sorts of conceptual device: (i) what he calls 'general concepts'; (ii) relations between these concepts; and (iii) relations between these concepts and sense experiences.[3] He gives no examples of general concepts but I expect the following would be examples: table, electron, space, time, gravitational field, gene, solubility etc. The relations between these concepts must be the sort of relations that science (or common sense) postulates between, say, an electron and an electromagnetic field. Einstein writes:

> 'nothing can be said *a priori* concerning the manner in which the concepts are to be formed... In guiding us in the creation of such an order among sense experiences, success alone is the determining factor. All that is necessary is to fix a set of rules, since without such rules the acquisition of knowledge in the desired sense would be impossible'[4].

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[1] Cf. Hempel's distinction between 'empirical generalization' and 'theory formation' in Hempel, *op. cit.*, pp. 41-3; also Northrop's between 'the natural history stage of inquiry' and a 'deductively formulated theory'. F. S. C. Northrop, *The Logic of the Sciences and the Humanities*, (Macmillan, 1947), chapters 3 and 4.

[2] One wants to say 'the ratio of laws to phenomena decreases' but this would raise the question of what constituted a phenomenon for which I have no ready answer. To reformulate it in terms of observation statements instead of phenomena doesn't make the situation much clearer. This problem is, it seems to me, one that the philosophy of science will have to solve.

[3] 'Physics and Reality' (1936), *IAO.*, p. 292; *OOMLY.*, p. 61.

[4] *Ibid.*, *IAO.*, p. 292; *OOMLY.*, 61-2.

I'm note sure what Einstein is talking about here. Are the rules to be rules whereby the postulated concepts can be either derived (or reconstructed) in something like the way Mill hoped might be be possible by means of his canons of induction? But Einstein always lays great emphasis on the fact that concept formation can only proceed intuitively and is not, therefore, rule guided. Does he perhaps mean the rules of interpretation by which the intuitively postulated concepts are related to experience? This, however, seems unkikely because this task is done by 'the relations between the abstract concepts and sense experience'. Moreover, Einstein has said of the interpretation made by means of these relations:

> 'The connection of the elementary concepts of everyday thinking with complexes of sense experiences *can only be comprehended intuitively* and is unadaptable to scientifically logical fixation. The totality of these connections - none of which is expressible in conceptual terms - is the only thing which differentiates the great building which is science from a logical but empty scheme of concepts'[2].

It seems to me perfectly possible to fix logically the laws of interpretation of the theoretical concepts, and that any scientific theory does this by means of deductive chains which connect the hypotheses of the theory with observation statements. Without such chains, of at least a minimum logical rigour, no theory would be testable.

It is only when Einstein comes to amplify his comments about the rules it is necessary to fix that his meaning becomes visible, if not clear. He uses a metaphor:

> 'One may compare these rules with the rules of a game in which, while the rules themselves are arbitrary, it is their rigidity alone which makes the game possible'[2].

It is still not precisely clear what Einstein is referring to, but perhaps an example might come from the theory of relativity. In the theory of relativity Einstein assumes '*by definition*' that the time taken by light to travel from a clock A to another clock B is the same as the time it takes to travel from B to A[3]. In other words, the rules Einstein has been obscurely referring to are the conventional definitions which are required before any scientific theory can get underway[4]. I'm not certain this is what Einstein has been talking about but it seems the most reasonable interpretation I can find. But I certainly don't think that they could be either rules of interpretation,

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[1] *Ibid.* (My italics.)

[2] *Ibid.*

[3] Albert Einstein, 'On the Electrodynamics of Moving Bodies' (1905), in A. Einstein, H. A. Lorentz, H. Weyl and H. Minkowski, *The Principle of Relativity*, translated by W. Perrett and G. B. Jeffery, (Dover, 1923), p. 40.

[4] It is the failure to recognize these conventional definitions in special relativity that has led operationalists to claim the theory as an example of a theory constructed on completely operationalist lines. No theory could be completely operationalist.

or very general scientific principles such as the law of causality, the principle of relativity or the cosmological principle.

Einstein further schematized his conceptual system, dividing it up into primary and secondary concepts. The primary concepts are those concepts which 'directly and intuitively connected with typical complexes of sense experience'[1]. The secondary concepts consist of all other concepts which 'are possessed of meaning only in so far as they are connected, by propositions, with the primary notions'[2]. The propositions which provide this connection are either definitions or laws of nature, that is, propositions 'which express at least indirect relations between « primary concepts » and in this way between sense experiences'[3]. These propositions will be the ones which relate theoretical concepts to « thing concepts ». They might be regarded as secondary laws of interpretation: the first link primary concepts to sense experience, the second relate the secondary to the primary concepts.

The problem with this stratification of Einstein's conceptual system lies in the fact that Einstein regards the primary concepts as 'directly and intuitively connected with typical complexes of sense experience'. The first problem is with the word 'direct'. The problem is at least partly exegetic, for elsewhere Einstein treats sense perception as giving only indirect knowledge:

> 'Since, however, sense perception only gives information of the external world or of « physical reality » indirectly, we can only grasp this latter by speculative means'[4].

This seems to suggest that by speculation we have direct knowledge of reality - but this interpretation is belied by Einstein's statements (quoted on page 25 above) that all knowledge of reality comes from sense experience. Hence speculation gives no knowledge of reality at all; whilst sense perception gives only indirect knowledge. It would seem that direct knowledge is nowhere to be found. But if we have direct knowledge (« direct acquaintance » would be a better term) with anything it must be with sense experience; after this come the primary concepts and then the secondary ones (of various degrees of depth).

Because Einstein regards its as the task of science to bring about order amongst our sense experiences, he also regards it as better that there should be more order rather than less. This is the pragmatic criterion again. He sees the physicists' task as two-fold:

> '... a comprehension as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim *by the use of a*

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[1] 'Physics and Reality' (1936), *IAO.*, p. 293; *OOMLY.*, p. 62.

[2] *Ibid.* The distinction is analogous to Hume's distinction between ideas of impressions and ideas compounded of ideas of impressions; and to Russell's between knowledge by acquaintance and knowledge by description.

[3] *Ibid.*

[4] 'Maxwell's Influence on the Evolution of the Idea of Physical Reality' (1931), *IAO.*, p. 266; *TWAISI.*, p. 157.

minimum number of primary concepts and relations. (Seeking, as far as possible, logical unity in the world picture, i.e., paucity of logical elements)'[1].

It seems strange that Einstein should talk of a 'minimum of *primary* concepts' because we are committed anyway to a large number of such concepts. Presumably there is one for each physical object with which we are acquainted - or, at least, for each type of physical object. (Einstein doesn't make it clear whether we have a separate concept for each table or just one concept (a universal) for all tables: I think probably the former, the universal being arrived at afterwards). I don't see how the vast number of primary concepts might be reduced. What I suspect Einstein meant was « logically independent primary concepts »: in other words he hoped that large numbers of primary concepts could be logically deduced from other other concepts - either from other primaries or from secondary concepts.

He throws some light on this problem when he goes on to speak of the stratification of science. Science goes through several stages of development: at the first stage there are only primary concepts and relations between them. This is the level at which everyday thinking always remains; and the level of sciences like natural history[2]. 'Such a state of affairs cannot, however, satisfy a spirit which is really scientifically minded; because the totality of concepts and relations obtained in this manner is utterly lacking in logical unity'[3]. To introduce logical unity a secondary system is created in which all the previous primary concepts and relations are derived from a smaller collection of concepts. The price that this secondary system pays for its logical unity is its distance from sense experience. Science develops through secondary and tertiary stages until it reaches 'a system of the greatest conceivable unity, and of the greatest poverty of concepts and of the logical foundations, which is still compatible with the observations made by our senses. We do not know whether or not this ambition will ever result in a definitive system'[4].

This brings us to one of the most famous or notorious aspects of Einstein's philosophy of science, his so-called simplicism. Einstein believed that a simple theory was best so long as it did not conflict with any of the empirical evidence[5]. This simplicity thesis follows from his pragmatic criterion: the simplest theory, i.e., that which has fewest independent elements, is the one which provides the greatest order among our sense impressions. I think it would be foolish to assert that anyone ever believed that the simplicity of a theory was evidence of its truth. But I think, for example, that most physicists would prefer to explain some phenomenon as a natural conqequence of a new theory rather than to add a new concept *ad hoc* to the old theory in order to explain it. An example would be Einstein's own introduction of the cosmological constant, which he introduced into the field equations of general relativity in order that the theory could provide a satisfactory cosmological theory. The cosmological constant was, for Einstein, an unnatural addition to his field equations and he was relieved to give it up when it proved unnecessary[1]. This example indicates that Einstein was prepared, in practice, to give up the simplicity thesis when empirical considerations required it. It would, of course, be perfectly possible to account for any phenomenon by the simple expedient of multiplying *ad hoc* assumptions. For example, suppose a certain theory of physics fails to account for a certain phenomenon, p, then this situation might be remedied by introducing a statement to the effect that p occurs into the list of fundamental principles of the theory. (This remedy would not work if the unamended theory denied that p occurred for then the addition of the statement that p occurred would make the theory inconsistent). Scientists, however, only adopt such an easy way out with extreme reluctance. Moreover, if we pressed this method of explanation to its limit we would have a theory in which there was a one-to-one correlation between the statements of the thory and the empirical facts for which the theory provided an explanation. Assuming that we made no mistakes in establishing the facts, such a theory would not be open to test except by showing that the deductions of observation statements from the theoretical statements were faulty.

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[1] 'Physics and Reality' (1936), *IAO.*, p. 293; *OOMLY*, p. 63. (Italics in the original).

[2] See above, p. 31.

[3] 'Physics and Reality' (1936), *IAO.*, p. 293; *OOMLY.*, p. 63.

[4] *Ibid.*, *IAO*, p. 294; *OOMLY.*, p. 63.

[5] This whole doctrine has been attacked in detail by Mario Bunge, *The Myth of Simplicity*, (Prentice-Hall, 1963) and Rom Harré, *The Anticipation of Nature*, (Hutchinson, 1965). Harré seems to imagine that simplicists regard simplicity as a criterion of truth.

There are two main objections to Einstein's simplicity assumption. The first is the problem of formulating it correctly; the second is the possibility that he may have been carried away by its success and have thought that simplicity was, in itself, some criterion of truth. In suggesting simplicity Einstein almost certainly had at the back of his mind classical thermodynamics. Of all parts of science this was the one that most impressed him in his youth by the simplicity of its assumptions and the wealth of empirical evidence that could be deduced from it.

> 'A theory is the more impressive the greater the simplicity of its premisses is, the more different kinds of things it relates, and the more extended its area of applicability. Therefore the deep impression which classical thermodynamics made upon me. It is the only physical theory of universal content concerning which I am convinced that, within the framework of its basic concepts, it will never be overthrown....'[2].

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[1] See, Nicholas Griffin, 'Relativistic Cosmology', *Science Progress*, (1968), vol. 56, pp. 382-3 for details.

[2] 'Autobiographical Notes' (1948), in Schilpp, vol. i, p. 33.

Here Einstein wavers between saying that the simplest theory is correct and saying that it is preferable, other things being equal. He says 'a theory is the more *impressive*' not 'more likely to be correct'; but later he says that he's convinced that thermodynamics, presumably on account of its simplicity (for that's what he's talking about) will never be overthrown, which is tantamount to saying that it's right.

Other passages in his writings can be used to support either view:

> 'It is the grand object of all theory to make these irreducible elements as simple and as few in number as possible, without having to renounce the adequate representation of any empirical content whatsoever.... Our experience hitherto justifies us in believing that nature is the *realization* of the simplest conceivable mathematical ideas. I am convinced that we can *discover* by means of purely mathematical constructions the the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical utility of the mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality as the ancients dreamed'[1].

This is the strong thesis that simplicity is some criterion of truth. I can't believe that Einstein really meant what he said when he wrote 'nature is the realization of the simplest conceivable mathematical ideas', except in a rather specialized sense. His own general theory of relativity is of considerably greater mathematical complexity than Newton's theory of gravitation. Elsewhere, however, he writes: 'The sense experiences are the given subject matter. But the theory that shall interpret them is man-made'[2]. As it can only be the theory that can be simplified it would seem that here Einstein held that the simplicity is not of the world but of the (man-made) theory. As Bishop Butler said 'Everything is what it is and not another thing'.

The simplicity thesis guides Einstein throughout his scientific work and to destroy it seems like killing the goose that lays the golden eggs. It was found, after all, that the cosmological constant wasn't really needed. The field equations of general relativity (without ghe cosmological constant) are the simplest laws which a Riemannian metric can satisfy. We can further regard a Riemannian metric as more simple (because more general) than a Cartesian one. If, from general relativity, one derives a vector field then its simplest laws are those of Maxwell's field equations for empty space. Einstein gives a list of such examples[1]. These examples require a deeper analysis than there is room for here and also bring us to the problem of formulating the simplicity thesis correctly. I hope to consider these problems in a later article[2].

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[1] 'On the Method of Theoretical Physics' (1933), *IAO.*, pp. 272-4; *TWAISI*, pp. 134-6. (My italics).

[2] 'The Fundaments of Theoretical Physics' (1933), *IAO.*, p. 323; *OOMLY.*, p. 98.

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[1] 'On the Method of Theoretical Physics' (1933), *IAO.*, pp. 274-5; *TWAISI.*, p. 137.

[2] An early draft of this paper was read to a meeting of the Philosophical Society of Leicester University in October, 1969.

STUDIES OF ATMOSPHERIC POLLUTION FROM SPACE

The possibilities of air pollution investigations from the data of satellite measurements are discussed. In this connection the results of photography and spectrography of the day and twilight Earth's horizons from manned spacecraft « Soyuz » are considered. The possibilities are noted as to the use of the absorption spectroscopy method in the observations of the atmospheric absorption spectrum during the periods of sunrises and sunsets relative to the satellite (« occultation technique »). The feasibilities are considered in great detail of determining the air pollutant content from the data of spectral measurements of atmospheric thermal radiation into space using the method of selective modulation. A short review is made of the possibilities of applying the methods of laser sounding and Raman scattering observations. Emission infrared spectroscopy is shown to be the most promising method.

Despite the essential measures undertaken for reducing industrial atmospheric pollution which sometimes becomes dangerous (particularly in the vicinity of industrial centeres), this problem is becoming more and more important. For example, it has been established that the meteorological regime in many cities depends essentially on air pollution (see, for example, Berlyand [1] and Georgii [13]). The consequences of pollution can be of far more serious a character, particularly in the case of a number of West European countries.

To illustrate the validity of this conclusion, Fig. 1 reproduces the data (taken from Georgii's paper [13]) in relation to the Federal Republic of Germany, on the increase in the number of car engines and fuel and mineral oil consumption compared with the population growth whose linear increase is seen to be accompanied by the exponential increase in the factors determining air pollution.

A rapid development of all kinds of transport (primarily cars and aircraft) as well as intensive industrial development lead to an increase in the air pollution level, not only in the immediate proximity of large industrial centres, but also in more extensive areas. Barringer et al. [9] note that the pollution caused by high-altitude jet aircraft and satellite launches extends to the high atmosphere. All this imparts to the problem a global character necessitating the search for the feasibility of applying satellite methods for studying air pollution.

From the point of view of atmospheric turbidity at various altitudes, photographing and spectrophotometric observations of the Earth's twilight aureole from manned orbital stations may be important.

It should be mentioned, for example, in this context that the photometric analysis of the Earth's day horizon photographs from the « Soyuz-3 » spacecraft performed in the work by Sandomirsky et al. [7] permitted the determination of the vertical profiles of the optical atmospheric thickness and the scattering function of stratospheric air (for the height range of 15 - 52 km). These data demonstrated once again the possibility of tracking the planetary field of aerosol air pollution and reaffirmed, in particular, the existence of the earlier detected zone of « clean » atmosphere at altitudes of 30 - 35 km.

Kondratyev et al. [4] have analyzed the possibilities of using the Earth's twilight aureole spectra which were first obtained from the « Soyuz-5 » spacecraft and shown the importance of this kind of spectral data for the determination of the vertical aerosol profiles in the stratosphere. Satellites are certain to be used extensively for investigations of atmospheric aerosol pollution. Having mentioned some investigations in this field, we shall concentrate our attention only on the discussion of the problem in relation to gas air pollutants.

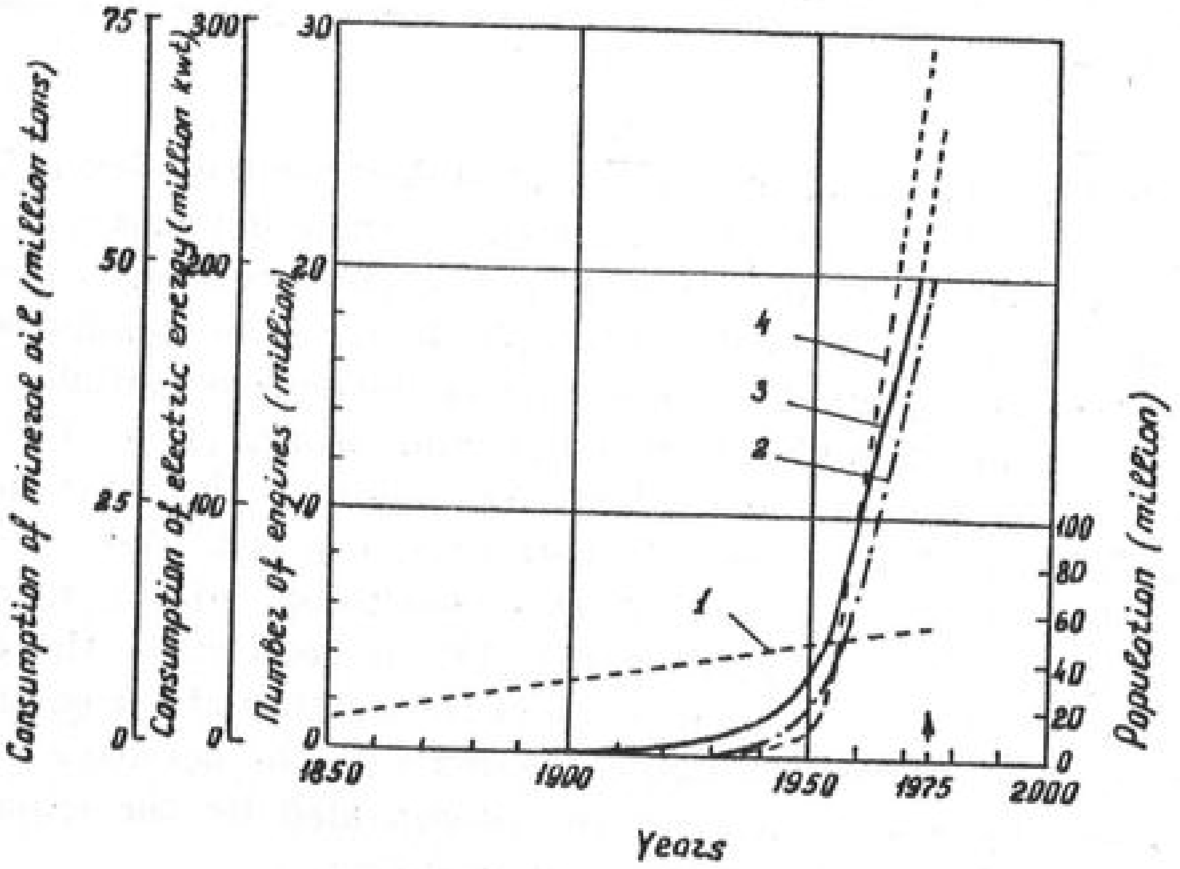

Fig. 1

Increase in fuel and mineral oil consumption in the Federal Republic of Germany compared with the growth in population.

1 - population; 2 - number of car engines; 3 - power consumption; 4 - mineral oil consumption.

Air pollution is associated with the presence in the atmosphere of both primary pollutants and secondary products resulting from various chemical and photochemical reactions. The main pollutants are the compounds of sulphur (mainly SO_2) ,nitrogen (NO, NO_2, NH_3), carbon (CO, CO_2), halogens, hydrocarbons, aldehydes and particles (solid aerosols). The most typical industrial pollutants are CO, SO_2, NO_2 and O_3. Table 1, prepared by Georgii [13], represents the comparative data characterizing approximate concentrations of pollutants for the case of « clean » and polluted atmospheres. In all cases, with the exception of aerosol pollution, the values for the mixing ratio are in ppm.

As will be seen, the concentration of carbon monoxide exceeds the total concentration of all other pollutants (except for carbon dioxide). Sulphur gas and hydrocarbons as pollutants rank second to it in importance. Large industrial centres are characterized by the presence of the pollution « dome » above them whose height may reach 1000 m and usually exceeds 500 m (see Georgii [13]).

As was noted by Ludwig [18], all the pollutants listed in Table 1 are of great importance in view of their global extent. For example, annually about 220 m tons of carbon monoxide, having a long life and of toxic nature, are passed into the atmosphere. The annual influx of sulphur dioxide is, approximately, 146 M tons; despite its relatively short life (four days) the sulphur dioxide concentration in the atmosphere increases steadily.

TABLE 1

Pollutant concentration in the conditions of « clean » and polluted atmospheres. According to Georgii [13].

Pollutant	*Clean atmosphere*	*Polluted atmosphere*
Sulphur dioxide	10^{-3} - 10^{-2}	0.02 - 2
Carbon monoxide	1	5 - 200
Carbon dioxide	310 - 330	350 - 700
Nitrogen oxides	10^{-3} - 10^{-2}	10^{-2} - 10^{-1}
Hydrocarbons	1	1 - 20
Aerosol pollution ($mg.m^{-3}$)	0.01 - 0.02	0.07 - 0.7

In principle, one can conceive various possibilities of detecting and determining air pollutant concentrations from satellites. For instance, use can be made of the absorption spectroscopy method which consists of recording the solar radiation absorption spectrum by the atmosphere during sunrises and sunsets relative to the satellite (occultation technique) when solar rays penetrate the atmospheric thickness.

A similar method has been proposed by Bolle [11], Kondratyev et al. [2] for determining the water-vapour content in the stratosphere and mesosphere. Bolle suggests using for this purpose the data of measurements in the wave number interval 202.56 - 202.97 cm^{-1}, for which the transmission function may be considered to be independent of temperature. Spatial resolution is 0.5 - 0.6 km, the accuracy of determining the water-vapour concentration is about $\pm 10\%$ with the value for the mixing ratio being of the order of 10^{-6} - 10^{-5} g/g (such an accuracy can be assured when the error of radiation measurements is not more than 1 - 2% with the equivalent width of spectral interval equal to 0.5 cm^{-1}). Kondratyev et al. [2] have discussed the possibilities of determining the stratospheric and mesospheric humidity from the data of measurements in the 2.7μm water-vapour band (the wave-number interval is 3795 - 3825 cm^{-1}). Numerical modelling for various humidity conditions showed that in the case of the dry stratosphere the mixing ratio could be determined up to the altitude of about 70 km. An even higher ceiling of sounding can be reached in the presence of the humid stratosphere.

It is obvious, however, that the method of « occultation » sounding cannot be used for the lower atmosphere where the attenuation of radiation is too large and the measured signal is therefore practically equal to zero. Yet, it is the investigation of the lower atmosphere that is of prime interest.

If the solar radiation reflected and scattered by the Earth is measured in the ultraviolet and visible spectral regions an attempt may be made to use the spectral peculiarities of outgoing shortwave radiation, due to the existence of absorption bands in a number of pollutants (this refers primarily to SO_2, NO_2 and O_3). The practical applicability of this method is greatly complicated by the fact that the spectral peculiarities mentioned are comparatively weakly expressed whereas, the factors hampering the interpretation of data, viz. reflection from the Earth's surface and scattering (as well as absorption) by solid aerosols are very variable and influence greatly the outgoing radiation field. This situation is aggravated by the fact that the gases enumerated have absorption bands, first of all, in the ultraviolet and visible spectral regions (300 - 655 nm) where the influence of the factors complicating the interpretation of the observational data is most strongly expressed.

The interpretation of measured data in the ultraviolet and visible spectral regions using the correlation technique proposed by Barringer [9, 10] may have certain possibilities. This technique implies the use of the correlation spectrometer in which the measured spectrum of outgoing radiation is modulated by the fixed comparison spectrum. The similarity of both spectra is detected from the amplitude of signal output. The amplitude of this signal serves as a quantitative characteristic of the atmospheric content of the pollutant being investigated.

Aircraft tests of a similar method for determining the sulphur dioxide content (from the data of measurements near the wavelength of 3150 Å) have provided promising results.

However, to find out the effectiveness of the technique Barringer et al. [10], while measuring above the ozone layer on September 3, 1969, carried out the balloon launch of the instrument. The balloon was launched in such a way that it flew over Chicago at a height of 35 km. Simultaneously ground-based measurements of the sulphur dioxide and nitrogen dioxide content were made. The results of balloon and ground-based measurements are in satisfactory agreement, suggesting a strong increase in the pollutant content within the city.

Barringer et al. [10] started the elaboration of the correlation spectroscopic technique which will permit the exclusion of the effect of the light scattered backward by the atmosphere, thus giving the possibility of measuring only the solar radiation which is reflected from the Earth's surface and which passes (twice) through the atmospheric thickness.

The development of satellite meteorology showed the use of the data on spectral measurements of outgoing thermal radiation to be most promising for the solution of problems connected with the study of the composition and structure of the atmosphere (see Kondratyev and Timofeev [3]). Employing some results of recent investigations, we shall demonstrate the validity of this conclusion for the solution of the problem for determining the air pollution content.

Ludwig et al. [17, 18] have carried out numerical modelling to estimate the influence on the outgoing radiation of the 300 m thick pollutant layer (near the Earth's surface) for the following models of atmospheric stratification: the standard ARDC - 1959 atmosphere, winter arctic and tropical atmospheres. Table 2 respresents the calculated results for the 4.6 μm CO band with the CO concentrations corresponding to the conditions of negligible (1 ppm), moderate (10 ppm) and very high pollution (100 ppm).

The relative emissivity of the Earth's surface is assumed to be 0.95. The calculations are made for different temperature values of the Earth's surface T_s (to allow for the influence of the diurnal temperature variation) and for the mean temperature of the pollution layer T_a.

It is natural that the « sensitivity » of outgoing radiation to the pollutant concentration depends essentially on the temperature contrast T_s - T_a. At small temperature contrasts the ΔI_∞ - values are within the limits of error of measurements (the instrumental feasibilities available allow accuracy of measurements not exceeding 0.5 - 1% (see, for example, Kondratyev [5]).

Yet, on the whole, the results of the calculation referring to one of the most important pollutants are promising.

Ludwig et al. [17] have also investigated the influence of the character of the vertical profile of pollutant concentration (its total content being unchangeable) on the value of outgoing radiation. Detailed calculations of this kind have been carried out for ammonia (the wavelength being 10.8 μm). These calculations revealed the essential dependence of outgoing radiation on the type of vertical pollutant distribution.

Fig. 2 reproduces the results of calculations of the ΔI (λ) - value spectral distribution (as obtained by Ludwig et al. [17]) depending on wavelength for eight pollutants separately, as well as the calculations of the total effect of all these pollutants (of course, the total effect is not additive) for the conditions of a normal atmosphere. The spectral resolving power is 0.1 µm. The values for the maximum gas concentration n_{max} shown in the figure correspond, approximately, to one-tenth of the concentration typical for the level of the Earth's surface in the case of moderate and high pollution. Most distinctive on the whole curve are the peaks corresponding to carbon dioxide and sulphur dioxide.

TABLE 2

Signal changes (ΔI_∞) *between « clean » and 300 m thick, CO-polluted atmospheres for various atmospheric temperature profiles* (*ARDC* - 1959) *as observed at 50 km and* $\lambda = 4.6$ *µm* (*surface emissivity* $= \varepsilon_s = 0.95$). *After Ludwig* [17].

Atmosphere	*Carbon monoxide concentration (ppm)*	ΔI_∞ (%)
Normal	1	0.05
$T_s = 288^\circ K$	10	0.26
$\bar{T}_a = 287^\circ$	100	0.80
Normal	1	1.85
$T_s = 298^\circ$	10	6.56
$\bar{T}_a = 287^\circ$	100	16.63
Normal	1	— 2.65
$T_s = 278^\circ$	10	— 9.20
$\bar{T}_a = 287^\circ$	100	—22.96
Tropical	1	0.02
$T_s = 305^\circ$	10	0.06
$\bar{T}_a = 304^\circ$	100	0.32
Arctic winter	1	— 1.74
$T_s = 247^\circ$	10	— 6.49
$\bar{T}_a = 251^\circ$	100	—16.98

The data in Fig. 2 reveal the existence of a complex problem of overlapping emissivity bands of individual gases. It is natural that the situation becomes more complicated if one allows for the influence of non-considered bands, particularly those belonging to water-vapour and carbon dioxide. It follows from this that the first condition necessary for a successful solution of the problem of the determination of the pollutant content from the data of outgoing radiation measurements is isolating such a spectral interval where the absorption spectra of this and other pollutants do not overlap. It is apparent that such a condition can be most reliably realized when sufficiently narrow spectral intervals are chosen.

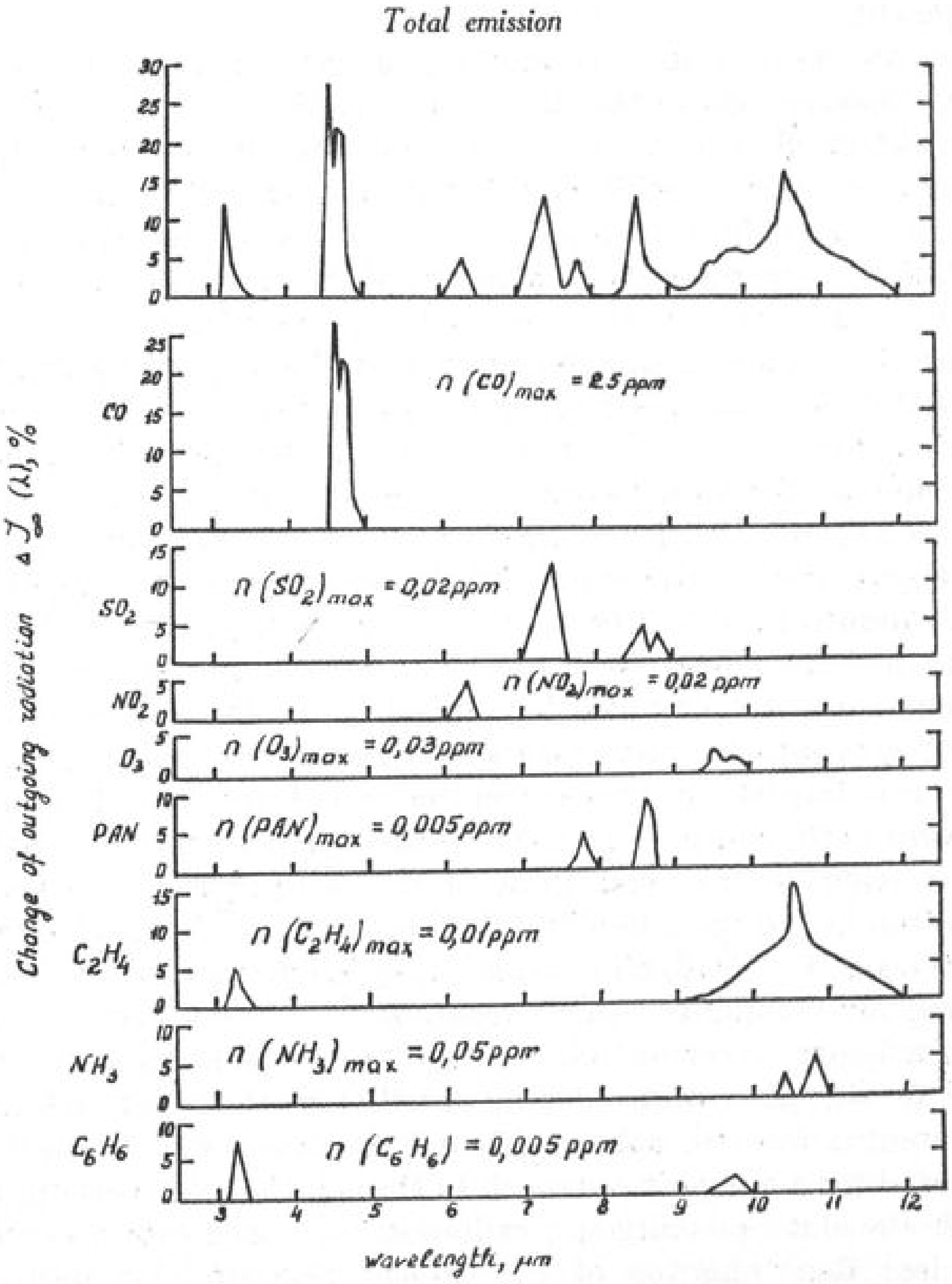

Fig. 2

Variations in outgoing rediation $\Delta I_{\infty}(\lambda)$ *for various pollutants depending on wavelength (PAN - peroxyl acetyl nitrate).*

Ludwig [18] notes that the resolution not worse than 0.1 cm^{-1} is necessary to achieve this. The only possibility of realizing such a high resolution is to use the selective modulation method (optical correlation) with the help of a gas cell containing the gas investigated. A similar method was earlier proposed by Houghton et al. [16] for the solution of the problem of thermal atmospheric sounding. Ludwig

[18] described the aircraft remote gas correlation analyzer elaborated for the determination of the carbon monoxide content from the data of thermal radiation measurements in the wavelength range of 4.58 - 4.78 μm. The same instrument is intended for measuring oxides of nitrogen and sulphur dioxide. The multi-channel gas pollutant correlation analyzer (a number of channels from eight to sixteen) is under development.

The above-mentioned computational results imply outgoing radiation measurements in the direction of nadir. It is natural that the influence of small atmospheric constituents on outgoing radiation must be strongly manifested while sighting in the inclined direction, particularly when the sight ray passes above the Earth's surface (as in the « occultation » experiments). Therefore, sighting in the inclined direction can greatly increase the sensitivity of the method.

Since the outgoing radiation is mainly determined by the temperature and vertical pollutant distributions (in the absence of band overlapping), it follows that the first step in the solution of the problem for determining the vertical profile of the pollutant concentration must be the determination of temperature variation with altitude, which will not give rise to particular difficulties because thermal sounding from a satellite may be considered as practically solved. Moreover, mathematical methods developed for the solution of the problem of thermal sounding may also be applied to the interpretation of data on outgoing radiation measurements to determine the pollutant content.

Of great importance in this connection is the essential dependence (as revealed in the solution of inverse problems of satellite meteorology) of the solution on the correctness of the integral equation kernel of the problem, i.e. on the reliability of determining the transmission function derivative. The available data on transmission functions of the above-considered air pollutants can be regarded as but tentative. Undoubtedly, insufficient spectroscopic study of these pollutants is a serious obstacle in the path of developing satellite methods for determining their content in the atmosphere. Though at present developments have been started for a complex approach to the solution of inverse problems in which the data on outgoing radiation measurements are used for independent determination of the transmission function derivatives, these cannot be considered as practically realizable.

Investigations carried out in connection with the solution of the problem of determining the vertical humidity profile have shown that one of the serious difficulties is caused, in this case, by « bad » weighting functions determining the vertical distribution to the relative contribution of separate atmospheric layers to the outgoing radiation (see Kondratyev and Timofeev [3]). This difficulty must apparently be also characteristic of the solution of the problem of noted pollutant content.

Finally, it should be that all noted the above-considered results of calculations refer to conditions of cloudless sky. This means that the use of the measured data referring to real conditions will cause the same problems about the influence of cloudiness as in the case of thermal atmospheric sounding. The situation is much more complicated since the determination of the pollutant concentration near the Earth's surface is of greatest interest.

As in the case of thermal soundings, the solution of the problem regarding cloud influence can be sought, in particular, through the use of the microwave spectral region. However, the essential overlapping of numerous lines of separate atmospheric constituents makes the use of this region difficult.

The use of lasers for optical sounding has aquired more extensive application in recent years [6, 8, 12]. One of the principal purposes of laser-sounding techniques being developed is the determination of vertical aerosol profiles (in this connection Novitsky [6] has noted that the use of golography may be of considerable interest for studying aerosols).

As shown by Hanst [14] interesting prospects are related to the use of lasers for determining pollutants. The lasers on carbon dioxide and iodine are most convenient for infrared spectroscopy. The first, apart from the most known 10.6 μm line, have dozens of other lines to be used. The iodine laser is suitable because of its high power and since its lines coincide with the absorption bands of three important pollutants: hydrocarbons, carbon monoxide and nitrogen oxide. The strongest line of the iodine laser is localized at 4.862 μm (this wavelength is within the principal carbon monoxide band). Hanst [14] has composed a summary illustrating the possibility of using different lines of laser sources for the solution of the problem on determining various pollutants. In practice, the solution of this problem is possible through the use of special or natural (under field conditions) reflectors as well as multiple pass cells. There are certain feasibilities of applying pulse lasers (in this case account is to be taken of the influence of scattering (Rayleigh, Mie, resonance, and Raman). The difficulty of using satellite laser sounding is that there is not a sufficiently developed interpretation technique for data from this kind of measurement.

Hirschfeld and Klainer [15] have discussed the feasibility of using remote Raman spectroscopy which becomes possible due to the availability of high - power lasers. In the case of Raman scattering, a part of the light scattered by molecules suffers a change in wavelength. The difference in wavelength between the irradiating and emitted light called the Raman displacement is a very sensitive characteristic of the molecules responsible for the Raman effect. The use of Raman scattering has a number of advantages; in particular, it allows the immediate determination of the molecular concentration of the gas pollutant investigated. The fact that intensity of Raman components is inversely proportional to the fourth power of the wavelength determines the expedience of using laser sources of the shortest wavelengths. The opposite factor has however also to be taken into account, viz. a strong atmospheric attenuation of shortwave radiation. Therefore, Hirschfeld and Klainer [15] expect that the choice of the laser at the wavelength of 3742 Å may be considered optimum (the double frequency ruby laser). Tests of the Raman technique using the nitrogen pulse laser (the wavelength is 3371 Å) and the double frequency argon laser (2572 Å) revealed distinct prospects for Raman spectroscopy in a study of air pollutants.

To summarize, the conclusion can be drawn that the use of data on spectral measurements of outgoing thermal radiation in the infrared spectral region is evidently most promising from the point of view of the development of satellite methods for determining gas air pollutants. The data available show that despite numerous difficulties efforts towards the development of this kind of technique must give fruitful results. Of great interest are also the possibilities of laser sounding (including the use of Raman spectroscopy). Yet, the prospects in this direction are not so distinct.

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SYMMETRIEN IN DEN PHYSIKALISCHEN NATURGESETZEN

Gemäss der grossen Entdeckung von E. Noether aus dem Jahre 1918 existiert ein enger innerer Zusammenhang zwischen den mathematischen Symmetrien der Naturgesetze und den Erhaltungssätzen. Beide Arten von Symmetrien, nämlich die kontinuierlichen und die diskreten, werden behandelt. Der physikalische Hintergrund dieser Symmetrien wird dargelegt. Schliesslich skizzieren wir einige aktuelle Erhaltungsprobleme der Physik der Elementarteilchen.

1. SYMMETRIE IN NATURWISSENSCHAFT UND PHILOSOPHIE. — In der heutigen Naturwissenschaft gebraucht man den Begriff der Symmetrie in verschiedenen Abstraktionsstufen, die man durch die Stichworte: Symmetrie von Körpern, Symmetrie von Eigenschaften, Symmetrie von Relationen (z. B. Naturgesetzen) charakterisieren kann. Der Ursprung dieses Begriffes (griech. Gleichmass, Ebenmass) geht wohl auf die Bezeichnung von Regelmässigkeiten im äusseren Erscheinungsbild materieller Körper zurück. Diese Regelmässigkeiten waren besonders durch die Beobachtung von Kristallen (Kristallsymmetrie) offensichtlich geworden. Davon ausgehend wurde dieser Begriff auf allgemeine Formaussagen über die Eigenschaften von Körpern verallgemeinert (Kugelsymmetrie, Zylindersymmetrie, Axialsymmetrie). Schliesslich abstrahierte man von materiellen Objekten überhaupt und übertrug diesen Begriff in rein gedankliche Sphären. Einer der ersten, der den Begriff der Symmetrie in dieser Hinsicht für die Mathematik nutzbar gemacht hat, war wohl Galois in seiner Fundierung der Algebra und Gruppentheorie. In die theoretische Physik wurde dieses Gedankengut durch die bekannte Arbeit von Emmy Noether[1] (1918) getragen. Infolge der sehr abstrakt-gruppentheoretischen Gestaltung dieser Abhandlung, die nur von einigen wenigen der damaligen Physikergeneration verstanden wurde, fand die Noethersche Theorie zwei Jahrzehnte keine rechte Resonanz. Es war insbesondere einer Arbeit von Bessel-Hagen[2] zu verdanken, der die Noetherschen Sätze auf konkrete physikalische Systeme anwandte und dadurch mit Leben erfüllte, so dass schliesslich der eigentliche Sinn dieser Erkenntnis ausgeschöpft werden konnte. Erst nach dem Zweiten Weltkrieg befassten sich in ungewöhnlich grosser Zahl Publikationen mit diesem Gegenstand[3], der Hauptinhalt des vorliegenden Artikels sein soll.

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Philosophisch gesehen, ist das Problem der Symmetrie schon von der griechischen Naturphilosophie (Pythagoreer, Platon) her bekannt. Dort gruppiert sich diese Thematik mehr um den Begriff Harmonie. Bekanntlich sagte die Lehre von der Harmonie der Sphären nach Pythagoras aus, dass durch den Umlauf der Weltkörper um das Zentralfeuer die harmonische Sphärenmusik entstehen soll, die allerdings für das menschliche Ohr nicht hörbar sei. Diese Idee wurde in Ciceros « Traum des Scipio » weiter ausgeführt. Danach sollen die Abstände der sieben Kreise der Weltkörper dem Zahlenverhältnis der sieben Töne eines Heptachords entsprechen. Sogar Kepler wurde nach vielen Jahrhunderten von diesem phythagoräisch-platonischen Gedanken noch tief berührt. Er wollte daraus ein Verständnis für die Planetenbewegung gewinnen. Nach der Vorstellung von Kepler u. a. war die Weltharmonie das geistig-göttliche Urprinzip der Welt überhaupt.

Durch die Naturwissenschaft wurden die Begriffe Symmetrie und Harmonie schliesslich ihres mystischen Elements entkleidet und auf eine höhere wissenschaftliche Erkenntnisstufe gestellt. Für die Wissenschaftsentwicklung ist das Studium der Symmetrien bzw. Asymmetrien in der materiellen Welt und in den mathematischen Formalismen zur Beschreibung dieser Welt von fundamentaler Bedeutung, sowohl in erkenntnistheoretisch-praktischer als auch in heuristischer Hinsicht. Das zeigt sich heute besonders deutlich in den Bemühungen um die Schaffung einer Theorie der Elementarteilchen. Das Studium der Symmetriemöglichkeiten, die mathematische Apparate zulassen, steht dabei bei vielen Forschern im Mittelpunkt. Es wird versucht, die empirisch gefundenen Erhaltungssätze der Elementarteilchenphysik mit mathematischen Symmetrien zu verknüpfen, um auf diese Weise Anhaltspunkte für die Formulierung der für den Elementarteilchenbereich noch zu entdeckenden Naturgesetze zu finden. Manche Forscher gehen sogar so weit, dass sie derartigen Symmetrieprinzipien die Rolle von Naturgesetzen zuschreiben, mithin eine Suche nach weiteren über die Symmetrieprinzipien hinausgehenden Naturgesetzen als Scheinproblem ansehen. Dem Einsteinschen Leitmotiv der Relativistischen Physik, nämlich dem Kovarianzprinzip (Forminvarianz) der Naturgesetze, wird heute häufig für den Elementarteilchenbereich ein Komplex von Symmetriepostulaten zur Seite gestellt, die als Leitlinien der Forschung fungieren.

2. Physikalische Grundgesetze. — Aussagen über Symmetrien der physikalischen Grundgesetze zu machen, setzt voraus, dass wir uns erst darüber klar werden, welche Gesetze zum Grundbestand der Physik gehören. Wir wollen darunter diejenigen Fundamentalgesetze verstehen, die das axiomatische Grundgerüst der Theoretischen Physik darstellen, aus dem die Unzahl von Einzelgesetzen zur Beschreibung

spezieller Phänomene deduziert werden kann. Um einen besseren Überblick zu erhalten, teilen wir die Physik wie folgt ein:

A. *Nichtrelativistische Physik.* — Diese erste wissenschaftliche Epoche der Physik gründet sich auf die fundamentalen Erkenntnisse von Galilei und Newton zur Mechanik, deren grundlegendes Bewegungsgesetz für mit kleinen Geschwindigkeiten bewegte Körper Newton 1687 publizierte:

Kraft = zeitliche Änderung des Impulses
= Masse mal Beschleunigung.

Als weiteres Grundgesetz tritt das ebenfalls von Newton im Jahre 1687 formulierte Gravitationsgesetz dazu, welches die Kraft zwischen zwei schweren Massen angibt.

Diese beiden Gesetze bilden die Basis der Newtonschen Mechanik. Die geometrische Grundlage für sie ist der ungekrümmte (Euklidische) 3-dimensionale Ortsraum, dem Newton Absolutheitscharakter zuschreibt. Die mechanischen Bewegungsabläufe in diesem Raum werden mittels einer Weltzeit, nach Newton ebenfalls von absolutem Charakter, parametrisiert. Sowohl die irdischen Körper als auch die Himmelskörper genügten im Rahmen der damaligen Messgenauigkeit glänzend diesen beiden Grundgesetzen. Über die statistische Mechanik hinweg reichten die Kompetenzen dieser Theorie bis in die Thermodynamik. Allerdings trat hier durch das Phänomen der Irreversibilität (II. Hauptsatz) ein völlig neuartiger Erscheinungskomplex hervor, durch den ein bestimmter Richtungssinn des Zeitablaufs ausgezeichnet wird. Gerade in den letzten Jahren wurde der enge Zusammenhang zwischen Irreversibilität, Symmetrie der Naturgesetze gegen Zeitumkehr und Kosmologie erkannt. Diese Problematik wird noch eifrig diskutiert. Doch haben die bisher vorgebrachten Lösungsvorschläge noch nicht allgemeine Zustimmung finden können.

Für den Bereich der Atomhülle versagte die Newtonsche Mechanik. Die Beschreibung der Bewegungsvorgänge von Teilchen mit kleinen Ruhmassen und kleinen Energien (nicht-relativistisches Gebiet) gelang Heisenberg (1925) durch die Schaffung der Matrizenmechanik und Schrödinger (1926) durch die Entwicklung der Wellenmechanik. Beide Theorien fallen in den Bereich der nichtrelativistischen Physik. Sie sind äquivalente Darstellungsformen der abstrakten Quantenmechanik, die eine mathematisch elegante Formulierung der Quantenmechanik darstellt und von Dirac begründet wurde. Auch die Paulische Erweiterung der Schrödinger-Gleichung zur Erfassung des Elektron-Spins gehört noch zur nichtrelativistischen Physik. Desweiteren ebenfalls die auf Heisenberg, Pauli, Jordan und Wigner zurückgehende Quantisierung des Schrödinger-Feldes, als deren Resultat sich die Existenz von Quantenteilchen ergibt.

B. *Relativistische Physik.* — Obwohl bereits 1864 endgültig von Maxwell formuliert, gehören doch die Grundgesetze des Elektromagnetismus, nämlich die Maxwell-Gleichungen, bereits zur Relativistischen Physik, die von Einstein mit der Entwicklung der Speziellen Relativitätstheorie 1905 und der Allgemeinen Relativitätstheorie 1915 begründet wurde. Während Maxwell selbst seine Gleichungen auf ein quasimechanisches Medium, den sog. Weltäther, bezog und auch Lorentz von dieser Idee noch gefesselt war, hat Einstein den qualitativ völlig neuartigen Charakter der elektromagnetischen Erscheinungen durchschaut. Er gab der Maxwell-Theorie das Primat gegenüber der Newtonschen Mechanik und konnte auf dieser Grundlage die speziell-relativistische Mechanik entwickeln. Das geometrische Fundament der Speziellen Relativitätstheorie ist die ungekrümmte (Euklidische) 4-dimensionale Raum-Zeit, der sog. Minkowski-Raum.

Der Versuch, eine speziell-relativistische Quantenmechanik aufzubauen, gelang 1928 Dirac durch die Aufstellung der sog. Dirac-Gleichung des Spin-Elektrons, die man als relativistische Verallgemeinerung der Schrödinger-Gleichung ansehen kann. Während die physikalischen Grundgesetze bis dahin auf dem Tensorkalkül basierten, erwies es sich als notwendig, zur Erfassung der Spineigenschaften der Materie neue mathematische Grössen, nämlich die Spinoren und Bispinoren zu benutzen.

Eine befriedigende speziell-relativistische Formulierung der Quantenfeldtheorie wurde am Ende des Zweiten Weltkrieges von Feynman, Schwinger und Tomonaga erzielt. Damit war also die speziell-relativistische Fassung der entscheidendsten physikalischen Naturgesetze erreicht:

Speziell-relativistische Mechanik,
Maxwellsche Elektromagnetik,
Diracsche Quantenmechanik des Elektrons,
Quantenfeldtheorie.

Die Absicht, auch die Newtonsche Gravitationstheorie speziell-relativistisch zu verallgemeinern, liess sich nicht sinnvoll in Übereinstimmung mit der Erfahrung verwirklichen. Der tiefere Grund dafür liegt darin, dass die Gravitation immanent mit der Krümmung der 4-dimensionalen Raum-Zeit verbunden ist, also in dem mit den speziell-relativistischen Erscheinungen verknüpften Minkowski-Raum nicht erfassbar ist. Vielmehr bedarf es zur Beschreibung des Gravitationsphänomens einer den Gesetzen der Riemannschen Geometrie gehorchenden gekrümmten 4-dimensionalen Raum-Zeit. DerAufbau einer derartigen Gravitationstheorie gelang Einstein 1915 im Rahmen der Allgemeinen Relativitätstheorie. Das Ergebnis war die *Einsteinsche Gravitationstheorie.*

Damit haben wir die wichtigsten, bisher bekannten Grundgesetze der Physik zusammengestellt, deren Symmetrien wir nun studieren wollen. Voraussichtlich wird die nächste Fundamentaltheorie der Physik die Theorie der Elementarteilchen sein, mit deren Hilfe es möglich sein wird, die innere Struktur der Elementarteilchen zu erfassen und Aussagen über Ladung, Masse, Spin, Isospin, Strangeness usw. machen zu können. Einer der bekanntesten Versuche wurde vor mehr als 10 Jahren in dieser Richtung von Heisenberg unternommen, doch ist der Erfolg dieser Theorie trotz einiger interessanter Voraussagen noch umstritten.

3. KONTINUIERLICHE SYMMETRIEN. — A. *Allgemeines.* — Die Konsequenzen der kontinuierlichen Symmetrien der Naturgesetze für die Erhaltungsgesetze der Physik hat, wie bereits einleitend erwähnt, Emmy Noether aufgedeckt. Es geht dabei um folgende Aufgabenstellung:

Wir betrachten ein physikalisches System, welches durch einen Satz von Feldfunktionen beschrieben werden möge, die von Raum- und Zeitkoordinaten abhängen. Die zentrale Funktion, welche das Verhalten des Systems wiedergibt, ist die Lagrangefunktion, die von den Feldfunktionen und von deren partiellen Ableitungen abhängt.

Man kann nun infinitesimale Koordinatentransformationen durchführen, also die Koordinaten infinitesimal abändern. Eine solche Operation induziert eine durch den geometrischen Charakter der Feldfunktionen bedingte Abänderung der Feldfunktionen selbst, die substantielle Variation heisst. Beispiele für solche Transformationen sind Translationen oder Drehungen des Koordinatensystems.

Unabhängig davon kann man die Feldfunktionen selber infinitesimal in ihrer Funktionsstruktur abändern, also daraus infinitesimal benachbarte Funktionsstrukturen erzeugen. Eine Abänderung dieser Art, die somit nichts mit einem Wechsel des Koordinatensystems zu tun hat, nennen wir Funktionalvariation. Diese ist also das Ergebnis einer Funktionaltransformation. Bekannte Beispiele dafür sind die Eich- und Phasentransformationen.

Unterwirft man die Lagrangefunktion eines physikalischen Systems diesen beiden Arten von Transformationen, so bekommt im Allgemeinfall die Lagrangefunktion eine neue Form in den transformierten Grössen. Von besonderer physikalischer Bedeutung sind nun gerade solche spezifischen Transformationen, die die Form der Lagrangefunktion erhalten, also deren Forminvarianz garantieren. Die Transformationen dieser Klasse heissen Symmetrietransformationen. Solche Symmetrietransformationen gewährleisten auch die Forminvarianz der physikalischen Grundgesetze, die meist mittels des Hamilton-Prinzips als sog. Lagrange-Gleichungen aus der Lagrangefunktion deduzierbar sind.

Das Noethersche Theorem beinhaltet nun die Feststellung, dass zu jeder unabhängigen Symmetrietransformation ein differentieller Erhaltungssatz gehört. Dabei versteht man unter einem differentiellen Erhaltungssatz eine auf einen Raum-Zeit-Punkt bezogene Differentialgleichung von der Form einer sog. Kontinuitätsgleichung, die die Bilanzierung des betrachteten Substrats ausdrückt, also die lokale zeitliche Änderung zum Zufluss und Abfluss in Beziehung setzt.

Durch Integration über den 3-dimensionalen Ortsraum versucht man dann, von einem differentiellen Erhaltungssatz zu einem integralen Erhaltungssatz überzugehen. Letzterer soll dabei so beschaffen sein, dass die in einem festen Volumen befindliche Menge eines gewissen Substrats, dessen Erhaltung untersucht werden soll, sich zeitlich nicht ändern darf, wenn sich Zufluss und Abfluss kompensieren. Dieses angestrebte Ziel gelingt aus mathematischen Gründen nur unter gewissen Voraussetzungen, mit denen wir uns im folgenden befassen wollen.

B. *Minkowski-Raum.* — Wie bereits oben erwähnt, hat die Spezielle Relativitätstheorie den ungekrümmten Minkowski-Raum als geometrische Grundlage. Dabei werden zur Beschreibung der physikalischen Objekte sog. Galilei-Koordinaten benutzt. Das sind vier Koordinaten, von denen drei räumliche rechtwinklige kartesische Koordinaten sind und die vierte Koordinate die Zeit ist. Auf dieser Basis bereitet der Übergang von einem differentiellen zu einem integralen Erhaltungssatz keine besonderen Schwierigkeiten. Welche kontinuierlichen Symmetrien weisen nun die physikalischen Grundgesetze im Minkowski-Raum auf?

Wir beginnen mit der Behandlung der Symmetrien bei Koordinatentransformationen.

Die physikalischen Grundgesetze enthalten keine Auszeichnung eines räumlichen Anfangspunktes (Ursprung) des Minkowski-Raumes. Gegenüber einer räumlichen Translation des Koordinatensystems sind sie forminvariant. Da eine derartige Translation in den drei unabhängigen räumlichen Richtungen erfolgen kann, gehen drei freie Parameter in die Rechnungen ein. Konsequenz dieser drei Symmetrietransformationen ist der Erhaltungssatz des Impulses, der vektorieller Natur ist und deshalb in drei Komponenten-Gleichungen zerfällt.

Die physikalischen Grundgesetze enthalten auch keine Auszeichnung eines zeitlichen Anfangspunktes des Minkowski-Raumes. Deshalb sind sie auch gegenüber einer zeitlichen Translation forminvariant. Die Verschiebung der Zeitskala hat also keinen Einfluss auf die Form dieser Grundgesetze. Folgerung dieser Eigenschaft ist der Erhaltungssatz der Energie.

Man benutzt für diese beiden Symmetrieeigenschaften der räumlichen und zeitlichen Translation den Begriff Homogenität des Minkowski-Raumes.

Die Grundgesetze der Physik enthalten darüber hinaus auch keine

Auszeichnung einer bestimmten Richtung des 3-dimensionalen Ortsraumes, d.h. sie behalten dieselbe Form, wenn man das räumliche Koordinatensystem dreht. In eine derartige Drehung gehen drei freie Parameter ein; denn die Drehrichtung selbst ist durch zwei Angaben festgelegt, während der Drehwinkel um diese Richtung durch einen Zahlenwert fixiert wird. Konsequenz dieser Symmetrietransformation ist der Erhaltungssatz des Drehimpulses, der sich wegen des Vektorcharakters des Drehimpulses aus drei Komponenten-Gleichungen aufbaut.

Wir kommen schliesslich noch zu einer weiteren Gruppe von Symmetrien, die für die Entwicklung der Physik von immenser Bedeutung waren. Bekanntlich ist das Newtonsche Bewegungsgesetz forminvariant, wenn man von einem Inertialsystem, für das es formuliert ist, zu einem anderen Inertialsystem übergeht, das sich gegenüber dem ersten geradlinig und gleichförmig bewegen möge. Man nennt eine solche Transformation Galilei-Transformation. Zu beachten ist dabei, dass in beiden Inertialsystemen mit derselben absoluten Weltzeit gerechnet wird. Da nun die Maxwell-Gleichungen des Elektromagnetismus gegen Galilei-Transformationen nicht forminvariant sind, stand Einstein vor einem grossen Dilemma: Hält man an dem Postulat der Einheit der Physik fest, so muss für die Mechanik und Elektromagnetik derselbe Transformationsapparat verbindlich sein. Deshalb hat man sich entweder für die Newtonsche Mechanik mit der Galilei-Transformation zu entscheiden und die Maxwellsche Elektromagnetik abzuändern, oder man gibt letzterer das Primat und muss die Newtonsche Mechanik aufgeben. Einstein schenkte der Maxwell-Theorie ein grösseres Vertrauen. Er gab gegenüber der Galilei-Transformation der den Maxwell-Gleichungen zugeordneten Lorentz-Transformation den Vorzug und musste deshalb die Newtonsche Mechanik so abändern, dass eine neue, die sog. speziell-relativistische Mechanik, entstand, die der Lorentz-Transformation entsprach. Dieses Werk vollbrachte Einstein in seiner Speziellen Relativitätstheorie im Jahre 1905. Dabei ist die Lorentz-Transformation eine für grosse Geschwindigkeiten (bis annähernd Lichtgeschwindigkeit) gültige Verallgemeinerung der Galilei-Transformation. Sie geht für kleine Geschwindigkeiten stetig in die Galilei-Transformation über.

Es zeigte sich nun gemäss der Anlage des Speziellen Relativitätsprinzips, dass nicht nur die speziell-relativistische Mechanik und die Maxwellsche Elektrodynamik gegen die Lorentz-Transformation forminvariant sind, sondern auch die übrigen oben behandelten Grundgesetze der Physik. Konsequenz dieses Tatbestandes ist der Erhaltungssatz des Schwerpunkts. Da die Translationsgeschwindigkeit zwischen den beiden Inertialsystemen drei Komponenten besitzt, so müssen drei Erhaltungssätze folgen. Das stimmt auch gut damit überein, dass die Lage des Schwerpunktes ebenfalls durch drei Komponenten beschrieben werden muss.

Die Lorentz-Transformation lässt sich 4-dimensional als eine Art räumlich-zeitliche Drehung interpretieren. Deshalb kennzeichnet man diese beiden Symmetrieeigenschaften der räumlichen Drehinvarianz und der räumlich-zeitlichen Drehinvarianz (« Lorentz-Drehung ») mit dem Begriff Isotropie des Minkowski-Raumes.

Wir fassen die aus Koordinatentransformationen fliessenden 10 Erhaltungssätze für die dynamischen Grössen noch einmal zusammen:

räumliche Translation	→	Impuls-Erhaltung (3)	Homogenität des Minkowski-Raumes
zeitliche Translation	→	Energie-Erhaltung (1)	
räumliche Drehung	→	Drehimpuls (3)	Isotropie des Minkowski-Raumes
räuml.-zeitl. Drehung	→	Schwerpunkt (3)	

Historisch bemerkenswert ist noch, dass bis zur Schaffung der Relativitätstheorie in den Naturwissenschaften ein Erhaltungssatz der Masse anerkannt war, wobei die Masse als ein Mass des Stoffes angesehen wurde. Deshalb gewann gerade dieser Erhaltungssatz eine besondere Bedeutung in der Chemie. Durch die relativistische Erkenntnis der Äquivalenz von Masse und Energie ging dieser Erhaltungssatz der Masse in dem Erhaltungssatz der Energie auf.

Neben die bisher behandelten 10 Erhaltungssätze, deren Existenz geometrischen Aspekten der Raum-Zeit zuzuschreiben ist, treten noch die durch Funktionaltransformationen bedingten Erhaltungssätze. Das bekannteste Beispiel davon ist der Erhaltungssatz der elektrischen Ladung. Die ihn verursachende Symmetrie ist die Eich-Phasen-Invarianz derjenigen physikalischen Theorien, in deren Bereich der Erhaltungssatz für die elektrische Ladung gilt. Das sind insbesondere die aus Maxwell-Feld und Dirac-Feld sowie die aus Maxwell-Feld und Klein-Gordon-Feld bestehenden kombinierten Feldsysteme.

Dabei versteht man unter Eichinvarianz den Tatbestand, dass eine Theorie durch eine Umeichung der elektromagnetischen Potentiale nicht beeinflusst wird. Das liegt daran, dass die Potentiale selber nicht messbar sind, sondern nur Feldstärken gemessen werden können.

Von Phaseninvarianz einer Theorie spricht man, wenn eine Verschiebung der Phase der Wahrscheinlichkeitswelle eines Teilchens die betrachtete Theorie forminvariant lässt, die Theorie mithin gegen Phasenverschiebungen unempfindlich ist.

Die beiden oben angesprochenen Feldsysteme (Maxwell-Dirac-Feld und Maxwell-Klein-Gordon-Feld) haben nun die interessante Eigenschaft, dass die in einer ganz bestimmten Kombination ausgeführte Eichtransformation und Phasentransformation (Eich-Phasen-Transformation) diese Theorien invariant lassen. Als Folge davon gilt der Erhaltungssatz der elektrischen Ladung:

Eich-Phasen-Transformation → elektrische Ladungserhaltung (1).

Weitere Erhaltungssätze für ladungsartige Grössen sind im Zusammenhang mit der Elementarteilchen-Physik entdeckt worden. Insbesondere handelt es sich dabei um die Erhaltung der baryonischen und leptonischen Ladung. Obwohl viele Physiker die Erhaltung der baryonischen Ladung mit der Touschek-Symmetrie in Verbindung bringen, gehen die Meinungen darüber und auch über andere Symmetrien noch weit auseinander. Erst mit der Schaffung einer allgemein anerkannten Elementarteilchen-Theorie ist eine diesbezügliche Aufklärung der komplizierten Situation zu erwarten.

C. *Riemann-Raum.* — In der gekrümmten Raum-Zeit, deren Gesetzmässigkeiten durch Einstein 1915 im Rahmen seiner Allgemeinen Relativitätstheorie und Gravitationstheorie aufgedeckt wurden und an deren Existenz insbesondere auf Grund der Nutzbarmachung des Mössbauer-Effekts nicht mehr ernsthaft gezweifelt werden kann, ist die Situation um die oben behandelten Erhaltungssätze wesentlich komplizierter. Summarisch kann folgendes gesagt werden:

Alle Erhaltungssätze, die in der Kontinuitätsgleichung einen Tensor 1. Stufe, etwa eine Viererstromdichte, zur Grundlage haben, gelten wie im Minkowski-Raum uneingeschränkt. Das betrifft insbesondere alle ladungsartigen Erhaltungssätze. Wir können deshalb feststellen, dass insbesondere die elektrische Ladung auch im gekrümmten Riemann-Raum eine strenge Erhaltungsgrösse ist, sie also ein umfassendes invariantes Attribut der Materie ausdrückt.

Grundsätzlich anders ist die Situation mit den 10 Erhaltungssätzen für die dynamischen Grössen. Bei der Darstellung dieser Problematik schliessen wir uns dem Standpunkt an, der zum Teil von Einstein bereits 1915 ausgesprochen wurde und zum Teil logische Konsequenz seiner Konzeption ist. Alle Versuche, diese im folgenden dargelegte Auffassung während des letzten halben Jahrhunderts zu revidieren, blieben bisher unfruchtbar, so dass voraussichtlich damit ein Schlusspunkt unter diese Problematik gesetzt wurde.

Es zeigt sich, dass für die Energie und den Impuls aus dem Noether-Theorem ein differentieller Erhaltungssatz in Form einer Kontinuitätsgleichung folgt. Das ist jedoch nicht mehr für den Drehimpuls und den Schwerpunkt der Fall. Für diese beiden Grössen versagt also bereits eine sinnvolle Formulierung differentieller Erhaltungsgesetze. Im Grunde genommen ist diese Situation verständlich, denn die Koordinaten haben einerseits in der Allgemeinen Relativitätstheorie nicht mehr den Charakter von Tensoren, andererseits sind aber die Koordinaten gerade die Konstruktionselemente für Drehimpulsgrössen. Da jedoch die Koordinatendifferentiale Tensorcharakter besitzen, lassen sich für infinitesimale Betrachtungen einige Begriffsbildungen retten. Doch ändert das nichts an der grundsätzlichen Schwierigkeit der Prägung brauchbarer Begriffe für Drehimpuls und Schwerpunkt.

Trotz der Existenz differentieller Erhaltungssätze für Energie und Impuls gelingt es nur unter sehr einschneidenden Voraussetzungen, durch Integration über den 3-dimensionalen Ortsraum zu integralen Erhaltungsgesetzen überzugehen. Im folgenden Abschnitt werden wir diese Situation am Beispiel der Energieerhaltung darlegen. Sinngemäss gilt für die Impulserhaltung ähnliches.

Zusammenfassend können wir somit feststellen, dass im Unterschied zu dem ebenen, ungekrümmten Minkowski-Raum der gekrümmte Riemann-Raum die beiden wichtigen Symmetrien der Homogenität und Isotropie nicht mehr aufweist. Das ist physikalisch auch gut verständlich, denn auf Grund der Einsteinschen Feldgleichungen der Gravitation ist die Krümmung der Raum-Zeit von Punkt zu Punkt unterschiedlich, und zwar in dem Masse, wie die Verteilung des Energie-Impuls-Tensors der Materie beschaffen ist. Der Verlust der Homogenität und Isotropie muss aber notwendigerweise Konsequenzen für die Erhaltungsgesetze der dynamischen Erhaltungsgrössen haben.

D. *Energieerhaltung.* - Bekanntlich findet man die Prägung des Begriffes « Energie » um 1620 erstmals bei Kepler. Allerdings war der Inhalt dieses Begriffes noch reichlich verschwommen. Auf Leibniz geht die Bezeichnung « vis viva » (lebendige Kraft) für die kinetische Energie und « vis mortua » (tote Kraft) für die potentielle Energie zurück. Da aber in der Literatur jener Zeiten die Begriffe Energie, Kraft, lebendige Kraft usw. synonym gebraucht wurden, kam es zu einer ständigen Begriffsverwirrung, die erst mit der Mathematisierung der Mechanik beendet werden konnte. Die eindeutige Fixierung des Begriffes Energie (gemeint war die kinetische Energie) ist Th. Young (1807) zuzuschreiben, während Rankine die Festlegung der Begriffe « kinetische Energie » und « potentielle Energie » in der bis heute bewährten Form zu verdanken ist. W. Thomson erkannte die Bedeutung des Energiebegriffs für die Wärmelehre. Schliesslich wurde ein relativ umfassendes Verständnis der Energie im Rahmen der nichtrelativistischen Physik durch den Erhaltungs- und Umwandlungssatz der Energie (Energieprinzip) erzielt, der 1842 und mit allgemeiner Aussagekraft 1845 von Robert Mayer entdeckt wurde. Unabhängig davon bezog 1843 Joule auch die chemische Energie in die Energiebilanz ein, wobei ihm die in einem galvanischen Element gespeicherte chemische Energie, die sich bei Stromfluss in Joulesche Wärme umwandeln kann, als Anhaltspunkt diente. Vier Jahre später (1847) formulierte Helmholtz in Unkenntnis der Arbeiten von Robert Mayer das Energieprinzip als universelles Naturprinzip für alle Gebiete der Naturwissenschaften. Aber erst um 1860 fand es in Physikerkreisen allgemeine Anerkennung.

Als Vorläufer von Robert Mayer sollen auch Galilei (1638), Huygens (1673), Leibniz, Johann Bernoulli (1748) und Euler genannt werden, denen die Grundzüge der mechanischen Energieerhaltung bei konservativen Systemen offensichtlich schon bekannt waren. Gedanken allgemeiner Art über die Erhaltung der « Materie », die philosophisch bis in die Antike zurückreichen, finden sich auch bei Lomonossow (1748).

Über die Entdeckung des Energieprinzips existiert eine umfangreiche Bibliographie. Deshalb soll diese Thematik hier nicht weitergeführt werden. Es ist zu gut bekannt, welche immense Bedeutung dieses Prinzip für den Fortschritt der Physik und damit für die Entwicklung der Technik hatte. Das gesamte Zeitalter der industriellen Revolution basiert auf seinem Fundament. Auch die philosophischen Auswirkungen dieser Entdeckung waren vielfältig und sogar sehr extrem gelagert. Einerseits war die Verabsolutierung des Energieprinzips eine der Hauptstützen des mechanischen Materialismus; andererseits führte diese Verabsolutierung direkt zur Ostwaldschen Energetik, einer vollkommenen Überschätzung des Energiebegriffs. Alles in der Welt sollte nur noch die Ursubstanz Energie sein.

Zu welcher Einschätzung des Energiebegriffs führen nun die von Einstein initiierten Gedanken?

Wir haben oben festgestellt, dass im ebenen Minkowski-Raum der Speziellen Relativitätstheorie die Energieerhaltung ebenso uneingeschränkt gewährleistet ist wie in der vorrelativistischen Physik. Im gekrümmten Riemann-Raum, der ein genaueres Abbild unserer Welt ist, lässt sich ein differentieller Erhaltungssatz in Form einer Kontinuitätsgleichung formulieren, doch gelingt der Übergang zu einem integralen Erhaltungssatz durch Integration über den 3-dimensionalen Ortsraum nur bedingt. Voraussetzung ist die Annahme einer inselartigen Verteilung des Energie-Impuls-Tensors der Materie. Unter diesen Umständen ist es möglich, solche Koordinaten einzuführen, die, vom Unendlichen kommend (im Unendlichen liegt ein Minkowski-Raum vor), durch die inselartige Verteilung laufen und wieder im Unendlichen auslaufen. Wir wollen derartige Koordinaten im Unterschied zu polarartigen Koordinaten Längenkoordinaten nennen. Unter Bezugnahme auf Längenkoordinaten gelingt dann die beabsichtigte Integration und damit die Prägung eines brauchbaren Energiebegriffes und damit auch die Formulierung eines Energieerhaltungssatzes.

Von einem universellen Energieprinzip kann also keine Rede mehr sein. Die Definition des Begriffes der Energie ist also an die sehr einschneidende Voraussetzung der Inselartigkeit der Verteilung des Energie-Impuls-Tensors und an die Benutzung von Längenkoordinaten gebunden. Ein allgemein gültiger und insbesondere in beliebigen Koordinatensystemen verwendbarer Energiebegriff existiert demnach nicht mehr.

Da die Krümmung von Raum und Zeit in unseren kosmischen Bereichen ein sehr kleiner Effekt ist, wirkt sich die Einschränkung des Energie-Erhaltungssatzes bis heute praktisch nicht aus. Dennoch handelt es sich um eine grundsätzliche Erkenntnis und ist deshalb physikalisch, aber auch erkenntnistheoretisch von prinzipieller Bedeutung.

Weil nach aller bisherigen wissenschaftlichen Erfahrung unser Universum kaum von einer inselartigen Struktur sein dürfte, wird die Anwendung des Einsteinschen Energiebegriffs auf das ganze Universum unzulässig, so dass Aussagen der Art: « Die Gesamtenergie der Welt bleibt bei allen Umwandlungsprozessen erhalten » ihren wissenschaftlichen Wert verlieren müssten.

Die obige Beschränkung auf Längenkoordinaten hängt noch mit einer anderen schwerwiegenden physikalischen, aber auch philosophischen Konsequenz zusammen: Die dem dargelegten Energiebegriff zugeordnete Energiedichte in einem Raumpunkt transformiert sich bei rein räumlichen Koordinatentransformationen, also bei Austausch der Namen der Raumpunkte, nicht wie eine Invariante. Folge davon ist, dass sich einem Raumpunkt nicht mehr eindeutig eine bestimmte Energiedichte zuschreiben lässt, mithin die Feststellung, dass in einem festen Volumen ein fester Energiebetrag existiert, sinnlos wird. Die Lokalisierung der Energie muss demnach fallen gelassen werden. Da die Lokalisierung aber Voraussetzung für den Begriff der Substanzialisierung einer bestimmten Grösse ist, so scheidet damit auch die Substanzialisierung der Energie aus. Vielmehr handelt es sich also bei der Energie um eine sehr abstrakte physikalische Grösse zur Beschreibung gewisser dynamischer Eigenschaften der Materie. Ihre Definition ist nur unter besonderen topologischen Gegebenheiten und unter Beachtung besonderer geometrischer Konfigurationen sinnvoll möglich.

Diese harte Konsequenz, auf die die Einsteinsche Energiekonzeption führt, ist anfangs sowohl bei den Physikern als auch bei den Philosophen auf allgemeinen Widerstand gestossen, zumal erst ungefähr 70 Jahre seit Aufstellung und Kanonisierung des Energieprinzips, das sich nach wie vor in der ungekrümmten Raum-Zeit bestens bewährt, vergangen waren. Deshalb hat es bis heute nicht an Revisionsversuchen gefehlt, die sich jedoch bisher alle als unfruchtbar erwiesen haben. Fast jeder dieser Versuche hat eine eigene Wissenschaftsgeschichte. Die wissenschaftliche Prognose für eine Revision der Einsteinschen Energiekonzeption dürfte deshalb mit grosser Wahrscheinlichkeit negativ ausfallen, so dass wir uns mit dieser eigenartigen Situation vermutlich für immer abfinden müssen. Sollte sich diese vielschichtig begründete Voraussage bestätigen, so bedeutet diese Degradierung des Energiebegriffs und Relativierung des Energieprinzips keineswegs ein Abgehen von einem soliden Fundament der Physik, sondern vielmehr ein tieferes Eindringen in neue Sphären unserer Welt. Frühere Erkenntnisse werden relativiert. Neue Erkenntnisse treten uns, insbesondere in der Gestalt der neuartigen Erhaltungssätze der Kernphysik, entgegen.

4. Diskrete Symmetrien. — A. *Allgemeines.* — Die Untersuchung der diskreten Symmetrien beschränkt sich bis heute im wesentlichen auf den Minkowski-Raum der Speziellen Relativitätstheorie unter Benutzung Galileischer Koordinaten. Auch hier wollen wir uns auf diesen Raum einschränken. Ihre eigentliche Bedeutung erlangten die diskreten Symmetrien in dem Bereich der Quantenmechanik und Quantenfeldtheorie. Das liegt daran, dass ihre mathematische Beschreibung aus dem Rahmen des Noether-Theorems, das sich ja gerade mit den kontinuierlichen Symmetrien befasst, herausfällt und des quantentheoretischen Operatorkalküls bedarf.

Die diskreten Transformationen, aus denen sich die diskreten Symmetrien herleiten, falls die Lagrange-Funktion bei ihrer Ausführung forminvariant bleibt, umfassen zwei verschiedene Typen von Koordinatentransformationen, nämlich die Raumspiegelung P und die Zeitumkehr T, die als uneigentliche Lorentz-Transformationen bekannt sind, und die sog. Ladungskonjugation C (Teilchen-Antiteilchen-Konjugation). Bei letzterer Transformation handelt es sich um eine in einer ganz bestimmten Weise ausgeführte diskrete (unstetige) Abänderung der Feldgrössen der betrachteten Theorie, ohne dass dabei die Koordinaten ausgewechselt werden.

B. *Raumspiegelung P.* — Ist ein physikalisches Naturgeschehen gegenüber der geometrischen Operation der Raumspiegelung (Ersetzung der rechtwinkligen kartesischen Koordinaten durch deren negative Werte) invariant, was sich in der Forminvarianz der für dieses Naturgeschehen zuständigen Lagrange-Funktion ausdrückt, so heisst das, dass zwei Physiker, von denen der eine von einem Linkssystem aus (Linkshänder) und der andere von einem Rechtssystem aus (Rechtshänder) dieses Naturgeschehen beschreiben, dieselbe Form der zugeordneten Naturgesetze finden. Anders ausgedrückt: Das in einem Spiegel sichtbare Naturgeschehen ist auch in der Natur selber möglich.

Die der Raumspiegelungs-Symmetrie korrespondierende Erhaltungsgrösse ist die Raumparität (oft kurz Parität genannt), die die beiden Zahlenwerte ± 1 besitzen kann. Alle oben im Rahmen der Speziellen Relativitätstheorie aufgezählten Naturgesetze besitzen diese Symmetrie, so dass diese früher als eine Naturnotwendigkeit angesehen wurde und keiner eine Verletzung für möglich gehalten hat. Schlagartig anders wurde die Situation, als im Jahre 1956 Lee und Yang entdeckten, dass die Erhaltung der Parität bei den schwachen Wechselwirkungen zwischen den Elementarteilchen nicht gewährleistet ist. Durch ein interessantes Experiment von Frau Wu wurde diese These empirisch bestätigt. Damit war klar, dass die Rechts-Links-Symmetrie der Naturgesetze in gewissen Bereichen der Natur keine Trivialität ist, also insbesondere bei den schwachen Wechselwirkungen ein Schraubensinn ausgezeichnet wird. Eine derartige Situation liegt vor, wenn bei einem bestimmten Prozess Neutrinos im Spiele sind. Ein typisches Beispiel dafür ist der radioaktive β-Zerfall.

C. *Zeitumkehr T.* — Wenn ein physikalisches Naturgeschehen gegenüber der Zeitumkehr (Ersetzung der Zeitkoordinate durch deren negativen Wert) invariant ist, also die Lagrangefunktion dieses Naturgeschehens bei Ausführung dieser geometrischen Operation forminvariant bleibt, so bedeutet das bei Heranziehung der Transformationseigenschaften für die Zustände, dass bei Vorgabe des zeitumgekehrten Endzustandes als Anfangszustand der zeitumgekehrte ursprüngliche Anfangszustand als Endzustand bei einem erneuten Ablauf des Prozesses erscheinen muss. Kinotechnisch ausgedrückt, heisst das, dass ein physikalisches Naturgeschehen gegenüber der Zeitumkehr invariant ist, wenn beim Rückwärtsablauf des das Naturgeschehen festhaltenden Films ein Prozess gezeigt wird, der im Rahmen dieses Naturgeschehens auch möglich ist. Die Zeitumkehr-Invarianz ist aber offensichtlich zerstört, wenn irreversible Prozesse, z.B. Reibungseffekte, mitspielen.

Im Unterschied zur Raumspiegelung ist die Zeitumkehr eine sog. antilineare mathematische Operation. Daran liegt es, dass dieser Symmetrie keine Erhaltungsgrösse korrespondiert, also von einer Zeitparität als Erhaltungsgrösse nicht gesprochen werden kann.

D. *Ladungskonjugation C* (*Teilchen-Antiteilchen-Konjugation*). — Es handelt sich dabei um eine Ersetzung der Feldgrössen durch derartige andere Feldgrössen, so dass die ein Elementarteilchen charakterisierenden Ladungsgrössen (elektrische, baryonische, leptonische) im Vorzeichen umgekehrt werden, also vom Teilchen zum entsprechenden Antiteilchen übergegangen wird. Beispiele solcher Übergänge sind:

Elektron → Positron,
Neutrino → Antineutrino,
Proton → Antiproton,
Neutron → Antineutron usw.

Auf dieser Operation beruht die Begriffsbildung der Antimaterie, bei der es sich also um Materie mit gespiegelten Ladungen (nicht Massen!) handelt. Die der Symmetrieoperation Ladungskonjugation zugeordnete Erhaltungsgrösse ist die Ladungsparität, die aber nur für ein System mit der Gesamtladung null definierbar ist und dann die beiden Zahlenwerte ± 1 annehmen kann. Als interessantes Beispiel für ein solches System erwähnen wir das Positronium.

E. *PTC-Theorem von Pauli und Lüders.* — Von Pauli und Lüders wurde nach Zuarbeit anderer Forscher der Zusammenhang der drei erwähnten diskreten Symmetrien P, T und C mit den kontinuierlichen Symmetrien der Lorentz-Drehungen aufgedeckt und erkannt, dass eine

Quantenfeldtheorie, die gegen Lorentz-Drehungen invariant ist und einige hier nicht näher zu diskutierende, aber im übrigen naheliegende Eigenschaften hat, dann auch gegenüber der kombinierten Operation PTC invariant ist.

Der Wert dieses Theorems ist enorm, weil daraus viele interessante Schlüsse für die Elementarteilchen-Physik gezogen werden können. So muss also eine Theorie, deren P-Symmetrie gestört ist, auch eine Störung der TC-Symmetrie aufweisen, wobei sich beide Störungen gegenseitig kompensieren müssen.

Nach dem Sturz der Erhaltung der Parität hoffte man, dass zumindest die kombinierte Symmetrie PC in der Natur garantiert sei, so dass die These richtig sein sollte, dass im Spiegel ein Prozess mit Antiteilchen beobachtbar sein müsse. Durch den Zerfall des neutralen K_2-Mesons in zwei Pionen, mit dem sich 1964 die Forscher Christensen, Cronin, Fitch und Turlay beschäftigten, sind ernsthafte Zweifel an dieser These aufgetaucht. Die Störung dieser PC-Symmetrie zieht dann infolge des PTC-Theorems sofort die Störung der T-Symmetrie bei diesen Prozessen nach sich.

Man erkennt an diesem Beispiel, zu welch tiefgründigen Aussagen heute das Studium der Symmetrien der Naturvorgänge befähigt. Ein völlig neuer Zugang zu den Gesetzmässigkeiten der Physik im Mikrokosmos und Makrokosmos wurde offenbar.

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TRANSLATION: J. B. SYKES · STEVENTON

SYMMETRIES IN THE PHYSICAL LAWS OF NATURE

According to the great discovery by E. Noether in 1918 there exists an intrinsic connection between the mathematical symmetries of the laws of nature and the conservation laws. The two kinds of symmetries, namely the continuous and the discrete ones, are discussed. The physical background of these symmetries is illustrated. Finally, we sketch some topical conservation problems in elementary particle physics.

1. SYMMETRY IN SCIENCE AND PHILOSOPHY. — In present-day science, the concept of symmetry is used in various stages of abstractness, which may be described by the terms symmetry of bodies, symmetry of properties, symmetry of relations (e.g. laws of nature). The origin of the concept of symmetry (the Greek word denoted equal measure) is probably to be found in the identification of regularities in the external world of phenomena involving material bodies. These regularities were particularly evident from the observation of crystals (crystal symmetry). From this starting point, the concept was generalised to statements about the shapes of bodies (spherical symmetry, cylindrical symmetry, axial symmetry). Finally, material objects were disregarded and the concept was applied in the abstract to the sphere of pure thought. Galois, in his foundations of algebra and group theory, was probably one of the first to utilise the concept of symmetry in this way for the benefit of mathematics. The same ideas were applied to theoretical physics as a result of the classical paper by Emmy Noether[1] (1918). Because of the fact that her paper was written in terms of abstract group theory and understood by only a few of that generation of physicists, Noether's theory met with little response for twenty years. A paper by Bessel-Hagen[2] in particular applied the Noether theorems to specific physical systems and thus gave them life, so that the true significance of the discovery was finally realised. Only after the second world war did many papers appear dealing with this subject[3], which will be the main theme of the present article.

In philosophical terms, the problem of symmetry is to be found in the writings of the Greeks (Pythagoreans and Plato), where the emphasis is more on the aspect of harmony. Pythagoras' doctrine of the harmony of the spheres asserted that the celestial bodies revolving around the central fire generate the harmonious music of the spheres, inaudible to the human ear. This idea was further developed in Cicero's « Dream of Scipio », according to which the seven circles of the bodies correspond to the numerical ratio of the seven notes of a heptachord. Even Kepler, many centuries later, was deeply affected by this

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[1] NOETHER E., Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse, 235, 1918.

[2] BESSEL-HAGEN E., Mathematische Annalen *84*, 258, 1921.

[3] See the references given by E. SCHMUTZER, Relativistische Physik, Teubner, Leipzig 1968.

Pythagorean-Platonic concept, and sought to use it for an explanation of the motion of the planets. In Kepler's view, the harmony of the universe constituted the fundamental spiritual and divine principle of the universe.

Science finally divested the concepts of symmetry and harmony of their mystical aspect and raised them to a higher plane of knowledge. The study of symmetries and asymmetries in the material world and in the mathematical forms used to describe it is of fundamental significance, both epistemological and heuristic, in the evolution of science. This is particularly evident today in efforts to construct a theory of elementary particles. Many investigators regard the possible symmetries allowed by mathematical formalisms as the centre of interest here. Attempts are being made to relate the empirical conservation laws in the physics of elementary particles to mathematical symmetries, so as to obtain a guide to the formulation of the laws of nature yet to be discovered for elementary particles. Some scientists even take these symmetry principles to *be* laws of nature, and thus regard it as meaningless to look for further such laws beyond the symmetry principles. The basis of Einstein's theory of relativity, the principle of covariance (invariant form) of the laws of nature, is now often set alongside a group of symmetry postulates for the elementary-particle range, which act as guidelines for research.

2. THE BASIC LAWS OF PHYSICS. — Statements concerning symmetries of the basic laws of physics presuppose that we are agreed which are these basic laws. Here the term will be applied to the fundamental laws which represent the axiomatic framework of theoretical physics, and from which the countless specific laws describing particular phenomena can be deduced. In order to clarify the situation, we shall discuss in turn non-relativistic and relativistic physics.

A. *Non-relativistic physics.* — This first epoch of physics as a science rests on the fundamental discoveries of Galileo and Newton in mechanics, whose basic law of motion for bodies moving at low speeds was announced by Newton in 1687:

force = rate of change of momentum with time
= mass times acceleration.

A further basic law is that of gravitation, also stated by Newton in 1687, which gives the force between two masses.

These two laws form the foundation of Newtonian mechanics. Their geometrical basis is three-dimensional Euclidean (zero curvature) coordinate space, regarded by Newton as absolute. Mechanical motions in that space are expressed in terms of a world-time parameter, which, according to Newton, is likewise absolute. Both terrestrial and celestial bodies obeyed these two basic laws splendidly, within the accuracy of measurement then available. The validity of the theory extended beyond statistical mechanics into thermodynamics. But here the irreversibility implied by the second law of thermodynamics led to a totally novel group of phenomena, distinguishing a direction of the passage of time. In recent years a close relationship has been recognised between irreversibility, symmetry of the laws of nature under time reversal, and cosmology, and the problem is still being actively discussed, but the solutions so far proposed have not found general acceptance.

Newtonian mechanics failed when applied to the outer layers of the atom. Heisenberg (1925) by the creation of matrix mechanics and Schrödinger (1926) by the development of wave mechanics were able to describe the motion of particles having small rest-masses and small energies (non-relativistic range). Both theories form part of non-relativistic physics. They are equivalent representations of abstract quantum mechanics, which is a mathematically elegant formulation of quantum mechanics, due originally to Dirac. Pauli's extension of the Schrödinger equation to take account of electron spin is still non-relativistic physics; likewise the quantisation of the Schrödinger field due to Heisenberg, Pauli, Jordan and Wigner, which leads to the existence of quantum particles.

B. *Relativistic physics.* — Although the formulation of the basic laws of electromagnetism, namely Maxwell's equations, was completed by Maxwell in 1864, they belong to the relativistic physics founded by Einstein with his development of the special theory of relativity in 1905 and the general theory of relativity in 1915. Whereas Maxwell related his equations to a quasi-mechanical medium, the universal ether, and Lorentz too could not escape from this idea, Einstein saw that electromagnetic phenomena were qualitatively different. He established the primacy of Maxwell's theory over Newton's mechanics, and was able in this way to develop the mechanics of special relativity. The geometrical basis of special relativity theory is four-dimensional Euclidean (zero curvature) space-time, called Minkowski space.

In 1928 Dirac was able to construct a quantum mechanics of special relativity by deriving the Dirac equation for an electron having spin; this may be regarded as a relativistic generalisation of the Schrödinger equation. Whereas the basic laws of physics had previously been derived in terms of tensor calculus, it was now found to be necessary to use new mathematical quantities, the spinors and bispinors, in order to take account of the spin properties of matter.

A seemingly adequate formulation of quantum field theory in terms of special relativity was achieved by Feynman, Schwinger and Tomonaga at the end of the second world war. Thus the principal physical laws of nature had been put in a form appropriate to special relativity:

Special-relativity mechanics
Maxwell's electromagnetic theory
Dirac's quantum mechanics of the electron
Quantum field theory.

The idea of generalising Newton's gravitational theory to special relativity could not be carried out in a way that was in reasonable agreement with experience. The real cause of this difficulty is that gravitation is intrinsically connected with the curvature of four-dimensional space-time, and therefore cannot be described in the Minkowski space appropriate to special relativity. Instead, to describe gravitation, we need a curved four-dimensional space-time, obeying the laws of Riemannian geometry. In 1915 Einstein was able to construct such a theory of gravitation as part of the general theory of relativity. Thus, with *Einstein's theory of gravitation,* we complete the list of the principal basic laws of physics as known at present, and now propose to examine their symmetries. It is likely that the next fundamental theory in physics will be that of the elementary particles, whereby the internal structure of these particles will be described and deductions regarding charge, mass, spin, isospin, strangeness etc. be obtained. One of the best known attempts at this was made by Heisenberg over ten years ago, but despite some interesting predictions the success of this theory is still under debate.

3. CONTINUOUS SYMMETRIES. — A. *General.* — As has been mentioned in section 1, it was Emmy Noether who discovered the consequences of the continuous symmetries of the laws of nature with regard to the conservation laws of physics. The problem was as follows. Consider a physical system described by a set of field functions of space and time coordinates. The central function specifying the behaviour of the system is the Lagrangian function, which depends on the field functions and on their partial derivatives.

It is possible to carry out infinitesimal coordinate transformations, i.e. to modify the coordinates infinitesimally. Such an operation brings about a modification of the field functions, called a substantive variation, as a result of the geometrical nature of these functions. Translations or rotations of the coordinate system are transformations of this kind.

Independently of this, the field functions themselves can undergo an infinitesimal modification of their functional structure, i.e. can be changed into functions with infinitesimally different structures. A modification of this kind, unconnected with any change of the coordinates, is called a functional variation, the result of a functional transformation. Gauge transformations and phase transformations are familiar examples.

If the Lagrangian of a physical system is subjected to these two kinds of transformation, it will in general have a different form in the transformed quantities. Particular physical interest attaches to those specific transformations which preserve the form of the Lagrangian, i.e. which ensure invariance of the form. Such transformations are called symmetry transformations; they also ensure invariance of the form of the basic laws of physics, which can usually be deduced from the Lagrangian as « Lagrange equations », using Hamilton's principle.

Noether's theory asserts that there is a differential conservation law for every independent symmetry transformation. A « differential conservation law » is a differential equation relating to a point in space-time and having the form of a continuity equation, expressing the balance-sheet of a given substrate by relating the local variation with time to the quantities added and removed.

Next, an integral conservation law is sought by integration of a differential one over three-dimensional position space. The integral law is to be such that the quantity of a certain substrate (whose conservation is in question) in a fixed volume does not vary with time if the quantities added and removed are equal. This aim can, for mathematical reasons, be attained only under certain conditions which will now be discussed.

B. *Minkowski space.* — As has already been mentioned, the geometrical basis of special relativity theory is zero-curvature Minkowski space, using Galilean coordinates to describe physical objects. There are four such coordinates: three rectangular Cartesian spatial coordinates and the time. The change from a differential to an integral conservation law then offers no particular difficulties. We now ask what are the continuous symmetries of the basic laws of physics in Minkowski space, and first discuss the symmetries under coordinate transformations.

The basic laws of physics give no indication of any preferred spatial origin in Minkowski space. They are form-invariant under a spatial translation of the coordinate system. Since such a translation can take place in the three independent directions in space, three free parameters are involved in the calculation. These three symmetry transformations lead to the law of conservation of momentum, a vector law having three component equations.

The basic laws of physics also give no indication of any preferred origin of time in Minkowski space. They are therefore also form-invariant under a time translation. A shift of the time scale does not affect the form of these laws. This property leads to the law of conservation of energy.

The concept of the homogeneity of Minkowski space is used to describe these two symmetry properties under space and time translation.

The basic laws of physics further give no indication of any preferred direction in three-dimensional position space; they retain the same form when the spatial coordinate system is rotated. Such a rotation involves three free parameters: the direction about which the rotation takes place is specified by two parameters, and the angle of rotation by one parameter. This symmetry transformation leads to the law of conservation of angular momentum, with three component equations.

Finally, there is a further group of symmetries which have been extremely important in the evolution of physics. Newton's law of motion is known to be form-invariant between one inertial frame of reference and another inertial frame moving relative to the first in a straight line at uniform speed. This is called a Galilean transformation. The absolute world time used is the same in both frames. Since Maxwell's electromagnetic equations are not form-invariant under Galilean transformations, Einstein was faced with a serious dilemma. If the postulate of the unity of physics is maintained, mechanics and electromagnetism must use the same transformations. Thus the choice has to be made between using Newtonian mechanics with the Galilean transformation while changing Maxwell's electromagnetism, or giving preference to the latter and abandoning Newtonian mechanics. Einstein had greater confidence in Maxwell's theory, giving preference to the Lorentz transformation which corresponds to this theory rather than to the Galilean transformation; he was therefore obliged to modify Newtonian mechanics into a new mechanics of special relativity agreeing with the Lorentz transformation. Einstein completed this task in his special theory of relativity (1905), the Lorentz transformation being a generalisation of the Galilean transformation valid at high velocities (up to almost the speed of light) and having the Galilean transformation as a limit to which it tends in a continuous manner at low velocities.

It was found that, according to the principle of special relativity, all the basic laws of physics discussed above are form-invariant under the Lorentz transformation, and not only the mechanics of special relativity and the Maxwell electrodynamics. This leads to the law of conservation of centre of mass. Since the relative translation velocity of the two inertial frames has three components, there must be three conservation laws, in agreement with the fact that the position of the centre of mass is also specified by three components.

The Lorentz transformation may be interpreted in four dimensions as a type of space-time rotation. These two symmetry properties of spatial rotation invariance and space-time rotation invariance (« Lorentz rotation ») are therefore described by the concept of the isotropy of Minkowski space.

The ten conservation laws of dynamic quantities which arise from transformations of coordinates may be tabulated as follows.

Space translation	→ conservation of momentum	(3)	homogeneity of Minkowski space
Time translation	→ conservation of energy	(1)	
Space rotation	→ cons. of angular momentum	(3)	isotropy of Minkowski space
Space-time rotation	→ cons. of centre of mass	(3)	

Historically, it is noteworthy that, before the development of the theory of relativity, science recognised a law of conservation of mass, mass being regarded as a measure of quantity of matter. This particular conservation law therefore became especially important in chemistry. The recognition by relativity theory of the equivalence of mass and energy meant that this conservation law became part of the law of conservation of energy.

In addition to the ten conservation laws so far discussed, whose existence is due to geometrical properties of space-time, there are also conservation laws arising from functional transformations. The best known example of this is the law of conservation of electric charge. The symmetry responsible is the gauge-phase invariance of those physical theories in which the conservation law is valid, in particular the combined field systems consisting of the Maxwell and Dirac fields and of the Maxwell and Klein-Gordon fields.

Gauge invariance means that a theory is unaffected by a change of gauge of the electromagnetic potentials, because only field strengths and not the potentials themselves can be measured.

A theory is said to be phase-invariant if it is form-invariant under a change in the phase of the probability wave of a particle, and is therefore unaffected by phase shifts.

The two field systems mentioned above (Maxwell-Dirac and Maxwell-Klein-Gordon) have the interesting property that these theories are invariant under one particular combination of gauge transformation and phase transformation, called a gauge-phase transformation. In consequence, we have the law of conservation of electric charge:

Gauge-phase transformation → cons. of electric charge (1).

Further conservation laws for charge-like quantities have been discovered in studies of the physics of elementary particles, in particular the conservation of baryon and lepton charge. Although many physicists relate the conservation of baryon charge to Touschek symmetry, opinions are very divided on this and other symmetries. The situation is likely to be clarified only by the development of a generally accepted theory of elementary particles.

C. *Riemann space.* — In the curved space-time whose properties were discovered by Einstein in 1915 in connection with his general theory of relativity and gravitation theory, and whose existence can no longer be seriously doubted (in particular, as a result of studies of the Mössbauer effect), there is a considerably more complex situation as regards the conservation laws discussed above. The following is a brief summary of the position.

All the conservation laws which are based on a tensor of rank one in the continuity equation, for example a 4-current density, are unrestrictedly valid, as in Minkowski space. This applies, in particular, to all conservation laws for charge-like quantities. We can therefore say that the electrical charge in particular is rigorously conserved even in curved Riemann space, and thus expresses a universal invariant attribute of matter.

The situation is quite different as regards the ten conservation laws for the dynamical quantities. In discussing this problem we shall adhere to the viewpoint in part expressed by Einstein himself in 1915, and in part forming a logical consequence of his ideas. All attempts during the last fifty years to modify this view, which is described below, have proved fruitless, and it therefore probably represents the final answer to the problem.

It is found that Noether's theorem gives for energy and momentum a differential conservation law in the form of a continuity equation, but this does not occur for angular momentum or centre of mass. Thus, for these two quantities, there is no meaningful formulation of differential conservation laws. This situation is essentially understandable, since the coordinates, which are the basis for the construction of angular momenta, are no longer tensors in the general theory of relativity. But since the coordinate differentials *are* tensors, some concepts can be retained in respect of infinitesimal quantities. This, however, does not affect the fundamental difficulty of arriving at valid concepts of angular momentum and centre of mass.

Despite the existence of differential conservation laws for energy and momentum, integral laws can be derived by integration over three-dimensional position space only under very restrictive conditions. The situation will be described in the next section as regards the conservation of energy; corresponding remarks apply to the conservation of momentum.

To summarise, therefore, the curved Riemann space differs from the flat (zero curvature) Minkowski space by not possessing the two important symmetries of homogeneity and isotropy. This is physically reasonable, since according to Einstein's field equations of gravitation the curvature of space-time varies from point to point, in the same manner as the distribution of the energy-momentum tensor in matter. The loss of homogeneity and isotropy must necessarily affect the conservation laws for the dynamical quantities.

D. *Energy conservation.* — It is well known that the concept of « energy » was originated by Kepler in about 1620, but it was extremely vague at that time. Leibniz coined the terms *vis viva* (living force) for kinetic energy and *vis mortua* (dead force) for potential energy. Since, however, the literature of that period made no distinction between energy, force, living force and so on, there was continual confusion, terminated only by the application of mathematics to mechanics. The concept of energy (meaning kinetic energy) was properly defined by Young (1807); Rankine defined « kinetic energy » and « potential energy » in the form in which these concepts are still used today. Thomson saw the significance of the energy concept in thermodynamics. Finally, a fairly complete understanding of energy in non-relativistic physics was achieved by means of the law of conservation and conversion of energy (the energy principle), discovered in 1842 and in its most general form in 1845 by Mayer. Independently, Joule in 1843 included chemical energy in the balance sheet, on the basis of chemical energy stored in a galvanic cell, which can change into Joule heat when a current flows. Four years later, in 1847, Helmholtz, unaware of the work of Mayer, formulated the energy principle as a universal one for all fields of science, but it was generally accepted by physicists only towards 1860.

Among the precursors of Mayer were Galileo (1638), Huygens (1673), Leibniz, Johann Bernoulli (1748) and Euler, who were apparently acquainted with the principles of conservation of mechanical energy in conservative systems. Lomonosov (1748) also advanced general ideas concerning the conservation of « matter », whose philosophical history goes back to ancient times.

There is an extensive literature on the discovery of the energy principle, and this topic will therefore not be further pursued here. Everyone knows the great part played by this principle in the progress of physics and hence the development of technology. It forms the basis of the whole age of the industrial revolution. Its philosophical consequences too were manifold and sometimes extreme. The establishment of an absolute energy principle was one of the main pillars of mechanical materialism; it also led directly to Ostwald's energetism, a gross overvaluation of the concept of energy, which regarded the whole universe as being made up of energy alone.

Turning to the question of the energy concept as it appears on the basis of Einstein's theory, we have seen above that in the flat Minkowski space of special relativity the conservation of energy applies without restriction, as in pre-relativistic physics. In the curved Riemann space which is a more exact representation of our universe, a differential conservation law can be formulated as a continuity equation, but there are limitations on the derivation of an integral conservation law by integration over three-dimensional position space. We have to assume an island-like distribution of the energy-momentum tensor of matter. It is then possible to define coordinates which come from infinity (where there is a Minkowski space), pass through the island and return to infinity. Such coordinates will be called length coordinates, in contradistinction to polar coordinates. Then the desired integration can be carried out with respect to length coordinates, thus leading to a valid concept of energy and hence the formulation of an energy conservation law.

There is therefore no longer a universal energy principle. The definition of the energy concept is dependent on the very restrictive assumption of an island-like distribution of the energy-momentum tensor and on the use of length coordinates. There is no longer a concept of energy which is generally valid and, in particular, usable in all coordinate systems.

Since in our immediate neighbourhood the curvature of space and time is a very small effect, the restriction on the law of conservation of energy is as yet without practical consequences, but the result is a fundamental one having both physical and epistemological significance.

According to all scientific experience hitherto, our universe is unlikely to have an island-like structure; the application of Einstein's concept of energy to the entire universe is therefore incorrect, and statements such as « The total energy in the universe remains constant in any transformation process » must cease to have any scientific value.

The limitation to length coordinates involves another serious physical and philosophical consequence. The energy density at a point in space, corresponding to the concept of energy as described above, does not behave as an invariant under purely spatial transformations of the coordinates, i.e. when the names of the points in space are interchanged. Consequently, it is no longer possible to assign a definite energy density to a point in space, and there is no meaning in the assertion that a fixed amount of energy exists in a given volume. The localisation of energy must therefore be abandoned, and since localisation is necessary for a quantity to be substantialised, energy cannot be substantialised, but is instead a highly abstract physical quantity used to describe certain dynamic properties of matter. It can be meaningfully defined only under special topological circumstances and with reference to special geometrical configurations.

This severe consequence of the Einstein conception of energy met at first with general resistance from both physicists and philosophers, especially as only some seventy years had elapsed since the energy principle, which had proved (and is still) extremely adequate in flat space-time, had been formulated and accepted. There has therefore been no lack of attempted modifications, but so far all have proved to be in vain. Almost every such attempt has its own scientific history. It may thus be predicted that there is practically certain to be no change from the Einstein conception of energy, and we must come to terms permanently with this peculiar situation. If such a repeatedly indicated prediction turns out to be correct, the degradation of the energy concept and the relativisation of the energy principle does not imply an abandonment of the firm foundations of physics, but rather a deeper penetration into new spheres of knowledge of the universe. Earlier knowledge becomes relative, and new knowledge is revealed, especially in the framing of the novel conservation laws of nuclear physics.

4. DISCRETE SYMMETRIES. — A. *General.* — The study of discrete symmetries has so far been restricted essentially to the Minkowski space of special relativity theory using Galilean coordinates, and this space only will be considered here also. The true significance of the discrete symmetries was attained with the discovery of quantum mechanics and quantum field theory. This is because their mathematical description lies outside the scope of Noether's theorem (which deals with the continuous symmetries) and demands the operator calculus of the quantum theory.

The discrete transformations, from which the discrete symmetries are derived if the Lagrangian is form-invariant under the transformations, comprise coordinate transformations of two types: the « improper Lorentz transformations » spatial inversion P and time reversal T, together with charge conjugation C (particle-antiparticle interchange). The latter transformation is a specific discrete (discontinuous) modification of the field quantities in the theory concerned, without interchange of the coordinates.

B. *Spatial inversion P.* — If a physical process is invariant under the geometrical operation of spatial inversion (which changes the signs of the rectangular Cartesian coordinates), and the Lagrangian of the process is thus form-invariant, it follows that two physicists of whom one uses a left-handed system and the other a right-handed system to describe the process will express the corresponding laws of nature in the same form. In other words, a process as viewed in a mirror is itself a possible process in nature.

The conserved quantity which relates to symmetry under spatial inversion is spatial parity, often briefly called parity, which can have the two values ± 1. This symmetry occurs for all the laws of nature listed above in relation to the special theory of relativity, and was therefore at one time regarded as an inviolable property of nature. The situation changed abruptly in 1956, when Lee and Yang discovered that parity is not conserved in weak interactions between elementary particles. This theory was empirically confirmed by Wu in an interesting experiment. It thus became clear that the right-left symmetry of the laws of nature is sometimes not trivial, and that in particular one screw direction is preferred in weak interactions. Such a situation occurs if neutrinos are involved in a process, a typical example being radioactive β-decay.

C. *Time reversal T.* — If a physical process is invariant under time reversal (which changes the sign of the time coordinate), and the Lagrangian of the process is thus form-invariant under this geometrical operation, it follows, using the transformation properties of the states, that when the same physical process is repeated with the time-reversed final state as the new initial state, the time-reversed original initial state will appear as the new final state. In cinematographic terms, a physical process is invariant under time reversal if, when the film of the process is run backwards, the resulting process is also physically possible. Time-reversal invariance obviously does not occur if irreversible processes such as friction are involved.

Unlike spatial inversion, time reversal is an « antilinear » mathematical operation, and therefore does not correspond to any conserved quantity. There is no such conserved quantity as a « time parity ».

D. *Charge conjugation C (particle-antiparticle interchange).* — This involves the replacement of field quantities by other field quantities such that the charges (electric, baryon and lepton) pertaining to an elementary particle change sign, and therefore each particle is replaced by the corresponding antiparticle, for example:

electron ⟶ positron
neutrino ⟶ antineutrino
proton ⟶ antiproton
neutron ⟶ antineutron

This operation is the basis of the concept of antimatter, which is thus matter with reversed charges (not masses). The conserved quantity which relates to the symmetry operation of charge conjugation is charge parity, but it can be defined only for a system whose total charge is zero; in that case, it can have the two values ± 1. Positronium is an interesting example of such a system.

E. *Pauli and Lüders' PTC theorem.* — Following contributions by other authors, Pauli and Lüders discovered the relationship between the three discrete symmetries P, T and C discussed above and the continuous symmetries of the Lorentz rotations, and found that, if a quantum field theory is invariant under Lorentz rotations and possesses certain other properties (which are likely, but will not be further described here), then it is also invariant under the combined operation PTC.

This theorem is exceedingly valuable, since it leads to many interesting results in elementary-particle physics. Thus a theory in which P symmetry is violated must also be one in which TC symmetry is violated in a compensatory manner.

After the abandonment of the conservation of parity, it was hoped that at least the combined PC symmetry would be preserved in nature, so that an antiparticle process would be seen as the result of inversion. This idea became open to serious doubt when the decay of the neutral K_2 meson into two pions was demonstrated in 1964 by Christenson, Cronin, Fitch and Turlay. The violation of PC symmetry, together with the PTC theorem, would necessarily imply the violation of T symmetry in these processes.

This example shows the deep results which can now be derived from studies of the symmetries of natural processes, opening an entirely new approach to the laws of physics in the microscopic and macroscopic universe.

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SUPERNOVAE E STELLE DI NEUTRONI

Vengono esposte le considerazioni (basate essenzialmente sulla stabilità dell'equilibrio di sfere fluide) che hanno indotto gli astrofisici a considerare come possibile l'esistenza di un particolare tipo di stelle dette stelle di neutroni. Successivamente si mostra come si è arrivati ad identificare le stelle di neutroni con residui di quel particolare fenomeno di esplosione stellare che va sotto il nome di Supernova.

1. STELLE DI NEUTRONI — Diciamo subito che la scoperta delle stelle di neutroni, anzichè attraverso un telescopio, avvenne mediante un computer e per un certo tempo rimase un'idea degli astrofisici teorici, i quali erano arrivati a calcolare un modello possibile di stella con caratteristiche assai particolari e non corrispondenti ad alcuno degli oggetti celesti osservati, appunto una stella di neutroni. Per apprezzare il valore di una tale scoperta è necessario avere ben chiaro che cosa si intende per modello stellare e soprattutto le condizioni che si devono imporre ad un tale modello perchè esso risulti accettabile. Cominciamo perciò col dire che una stella altro non è che una specie di macchina la quale produce energia nel suo interno e la irradia attraverso la sua superficie. Quello che si può osservare di una stella è molto poco. Nelle migliori condizioni si può conoscerne la luminosità, la distanza, la massa, il raggio, la rotazione e si possono avere indicazioni sulla composizione chimica e sul campo magnetico negli strati più esterni. Da questi scarsi elementi gli astrofisici teorici devono risalire alla struttura interna della stella. Essi, partendo da una sfera fluida cercano di calcolare una distribuzione di pressione, temperatura e densità all'interno della sfera tale che il « modello » ottenuto corrisponda il più possibile alle caratteristiche osservate nella stella che si sta studiando. Elementi fondamentali per il calcolo di un modello sono il tipo di reazioni nucleari che producono la energia, la temperatura, la pressione e la densità centrali, la composizione chimica e l'equazione di stato della materia costituente la stella. Il trasporto della energia nell'interno della stella avviene generalmente per radiazione, ma possono esistere regioni in cui il trasporto radiativo non è sufficiente a mantenere un gradiente di temperatura troppo ripido ed allora la materia collabora nel trasporto di energia mettendosi in moto e l'equilibrio da radiativo diviene convettivo.

Modelli corrispondenti assai bene alle stelle realmente esistenti sono stati calcolati in questo modo e la conoscenza della struttura interna stellare è oggi assai sviluppata. Tuttavia il problema non si esaurisce qui e quando si è costruito un modello bisogna assicurarsi della sua « stabilità ».

Le stelle si trovano in uno stato quasi stazionario. Variazioni della loro struttura si producono inevitabilmente in tempi dell'ordine del milione di anni e più, quando la stella, avendo esaurito un certo combustibile nucleare, ricava la energia necessaria alla sua esistenza da una momentanea contrazione gravitazionale e dalla successiva combustione di altro materiale nucleare resa possibile nelle nuove condizioni. Questi combiamenti, che fanno passare la stella da una condizione di equilibrio ad un'altra pure di equilibrio, costituiscono il fenomeno della evoluzione stellare. Durante la sua evoluzione la stella esaurisce gran parte del suo combustibile nucleare e della sua energia gravitazionale, riducendosi ad uno stadio di stella « vecchia » e cioè a quel tipo stellare che va sotto il nome di « Nana Bianca ».

Ma nelle stelle, oltre a questi lentissimi cambiamenti, possono avvenire fenomeni violenti che si compiono in un tempo molto breve e che possono alterare seriamente la condizione di equilibrio. In un tal caso si possono verificare due circostanze: o si generano forze di richiamo che riportano la stella verso la configurazione di equilibrio che viene raggiunta dopo una serie di oscillazioni smorzate (per una stella variabile le oscillazioni possono essere anche non smorzate); oppure la stella si allontana sempre più dalla configurazione di equilibrio esplodendo o contraendosi o compiendo oscillazioni di ampiezza crescente.

I modelli stellari devono corrispondere al primo caso e cioè essere stabili; un modello corrispondente al secondo caso è instabile e non può corrispondere ad una stella vera e propria, ma tutt'al più ad un fenomeno catastrofico di esplosione e di collasso. Quando si parla di stelle di neutroni non si creda che gli astronomi siano partiti d'emblée a calcolare modelli di stelle costituite da queste particelle elementari. Prima di tutto non ci sarebbe alcuna ragione che si fosse pensato ai neutroni piuttosto che ad altre particelle; in secondo luogo il fatto che il risultato dei calcoli dava luogo ad oggetti non solo strani, ma inosservati e difficilmente osservabili, avrebbe tolto ogni valore ad una tale ricerca. Le stelle di neutroni si sono invece presentate sulla scena della ricerca cosmologica, per così dire di prepotenza, reclamando un loro diritto alla esistenza, diritto che non si è potuto contestare proprio in forza della loro stabilità. Una delle circostanze più curiose e paradossali è che gli elementi essenziali relativi alle stelle di neutroni furono scoperti da Landau nel 1932 prima che nello stesso anno Chadwick rendesse pubblica la sua scoperta del neutrone.

La scoperta delle stelle di neutroni è connessa alla instabilità che può sorgere in stelle di bassa temperatura e densità centrale molto elevata. Per densità centrali di 10^8g/cm^3 (100 tonnellate per centimetro cubo) e più, la instabilità nasce dal fatto che i modelli devono essere calcolati facendo uso delle equazioni della relatività generale di Einstein.

Ora questa teoria prevede un fenomeno che va sotto il nome di « regenerative multiplication of pressure » e che consiste in sostanza nel fatto che la pressione, la quale è un'energia per unità di volume e come tale corrisponde relativisticamente ad una massa per unità di volume ($E = mc^2$!), ha un proprio peso. Se quindi per assicurare contro il collasso una stella i cui strati sono molto densi e quindi molto pesanti si aumenta la pressione, aumenta in tal modo anche il peso di questi strati e si arriva ad una configurazione critica oltre la quale ogni aumento della densità centrale conduce inevitabilmente al collasso, perchè nessuna distribuzione di pressione può equilibrare « stabilmente » il peso. Se le cose fossero schematizzabili in questo modo si avrebbe una sola densità centrale critica con configurazioni instabili per densità centrali maggiori e stabili per densità centrali minori. Nella realtà le cose sono più complicate. La pressione è funzione della densità attraverso la equazione di stato e questa equazione viene determinata dal comportamento microscopico della materia. Ora alle basse temperature ed alle densità di 10^8g/cm^3 caratteristiche del tipo di stelle ricordato sopra, le Nane Bianche, il gas di elettroni si deve considerare degenere e le sue proprietà sono determinate dalla statistica di Fermi-Dirac. Questo significa, detto in maniera grossolana, che non più di due elettroni possono possedere la medesima velocità. Per quello che riguarda il nostro problema, la proprietà più importante è che un gas degenere esercita una pressione assai superiore a quella di un gas ordinario e, siccome in una Nana Bianca solo il gas di elettroni è degenere, mentre i gas costituiti dalle altre particelle non lo sono, ne segue che la pressione è prodotta praticamente dai soli elettroni.

Se ora si aumentano la densità e la pressione al centro della stella, si arriva, per una densità centrale compresa fra 10^8 e 10^9g/cm^3, ad un fatto nuovo. Gli elettroni non possono più sostenere queste elevatissime compressioni, o, per meglio dire, il bilancio energetico è tale che gli elettroni preferiscono combinarsi con i protoni dei nuclei dando luogo a neutroni. Avviene cioè la reazione (inverse β-decay): elettrone + protone legato ad un nucleo ⟶ neutrone libero o legato + neutrino. In simboli:

$$e^- + p \longrightarrow n + \nu.$$

In questa situazione gli elettroni si vanno schiacciando contro i protoni e non esercitano più la pressione necessaria per sostenere gli strati sovrastanti. La stella diviene così instabile nei riguardi del collasso.

Se la densità centrale aumenta ancora la materia stellare si trasforma in un gas di neutroni ed ha luogo un ritorno alla stabilità per una densità centrale compresa fra 3.10^{13}g/cm^3 e 6.10^{15}g/cm^3. Oltre questo limite non si conoscono configurazioni stabili.

Questa scoperta di un intervallo di stabilità per modelli di stelle costituite da neutroni, oltre il limite di stabilità delle Nane Bianche ed in mezzo a due intervalli di instabilità (v. fig. 1) ha introdotto nella Astrofisica l'idea della possibile esistenza di stelle di neutroni.

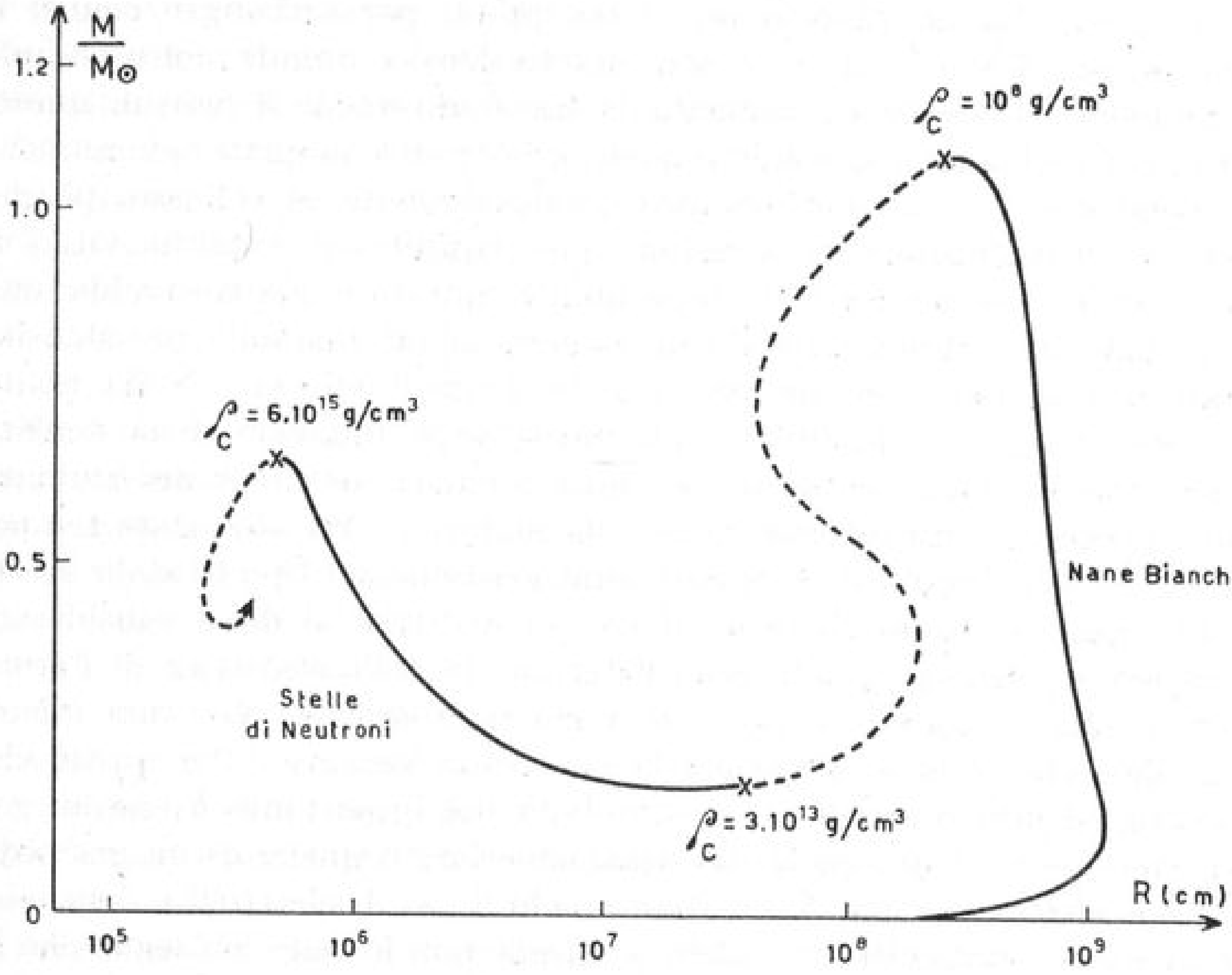

Fig. 1

I tratti punteggiati corrispondono a regioni di instabilità. La zona di esistenza delle stelle di neutroni confina con due ranges di instabilità.

The dotted lines correspond to regions of instability. The zone of existence of the neutron stars lies within two instability ranges.

Storicamente sono stati Oppenheimer, Serber e Volkoff ad introdurre l'idea della possibile formazione di neutroni mediante la inverse β-decay reaction a pressioni elevatissime (1938) e ad usare la trattazione relativistica (1939) per una stella costituita da un gas degenere di neutroni. Una trattazione sistematica dell'argomento è stata fatta da Harrison, Wakano e Wheeler. Landau potè arrivare al suo sorprendente risultato nel 1932 prima della scoperta del neutrone per questa ragione: quando la energia del gas degenere di Fermi diviene relativistica, l'energia intrinseca (massa) delle particelle diviene trascurabile ed energia e pressione cessano di dipendere dal tipo di particelle, mentre dipendono solo dalla dimensione del volume a cui la particella è confinata.

Non possiamo evidentemente fare il nome di tutti gli scienziati che hanno partecipato a questo interessante campo di ricerca e ricorderemo soltanto, oltre ai precedenti, Chandrasekhar, Misner, Zapolsky e Thorne.

Abbiamo detto che il dominio di esistenza e di stabilità delle stelle di neutroni è isolato. Se infatti si procede verso stelle con densità centrali minori (v. fig. 1) si incontra l'intervallo di instabilità che separa le stelle di neutroni dalle Nane Bianche. Il comportamento di stelle con densità centrali maggiori di quelle relative alle stelle di neutroni non è stato definitivamente studiato. Non si può escludere che esistano in questo dominio modelli di stelle stabili. Si può però asserire con certezza due cose: se le densità centrali crescono oltre ogni limite i modelli finiscono per diventare instabili; inoltre esiste certamente un dominio di instabilità contiguo a quello di esistenza delle stelle di neutroni dalla parte delle densità centrali maggiori.

È opportuno a questo punto precisare che la sequenza della figura 1 non rappresenta una serie di configurazioni che una stella possa successivamente assumere durante la sua evoluzione. I modelli sono stati calcolati partendo dalla densità centrale e ciascuno corrisponde ad una diversa massa-energia e ad un diverso numero di barioni. Da questo segue in base alle leggi di conservazione che un modello, evolvendosi, non può trasformarsi nel successivo. Una sequenza di modelli che rappresenti la evoluzione di una data stella deve corrispondere ad un determinato numero di barioni uguale per tutti i modelli. Tali sequenze non sono facili a costruire e presentano un aspetto diverso da quello della curva di fig. 1.

In ogni caso però, se si vuole ammettere la ipotesi molto ragionevole che le stelle di neutroni costituiscono uno stadio, seppur singolare, della evoluzione stellare, che, cioè, altre stelle si siano trasformate in stelle di neutroni, dobbiamo ammettere che queste stelle hanno compiuto tale trasformazione superando un intervallo di instabilità e cioè in maniera catastrofica.

In conclusione una stella di neutroni è un oggetto celeste formatosi discontinuamente e violentemente da una stella che ha subito un collasso della sua parte centrale e forse anche una esplosione degli strati periferici. Il risultato della catastrofe è quindi un nucleo centrale collassato circondato da uno strato di materia che si allontana nello spazio per effetto della velocità comunicatagli dalla esplosione.

Il nucleo è un oggetto celeste che dalle dimensioni di una stella nana come il Sole con raggio di 695.700 km si è ridotto al raggio di circa 10 km caratteristico di una stella di neutroni (v. fig. 1). Questa contrazione ha prodotto risultati formidabili. Per effetto della conservazione del momento angolare la sua velocità di rotazione, anche se inizialmente modesta (per il Sole un giro in circa un mese), deve assumere valori tali che la conseguente forza centrifuga riduce la iniziale forma sferica a quella di disco. Il campo magnetico, che alla superficie del Sole ammonta a frazioni di gauss, deve, per la conservazione del flusso di induzione magnetica, salire a valori dell'ordine di 10^{14} gauss.

Ed infine in una rapida contrazione come quella dovuta al collasso, in cui si raggiunge la velocità di caduta libera, la temperatura deve aumentare notevolmente anche se l'aumento è controbilanciato dalla emissione di neutrini e dal fatto che la inverse β-dacy reaction diviene endotermica alle elevatissime pressioni che regnano nell'interno di una stella di neutroni.

Come ben si comprende la stella originaria non arriva tranquillamente al suo nuovo stato di equilibrio. La maniera catastrofica con cui questa nuova configurazione è raggiunta fa sì che la stella di neutroni deve essere sede di violente oscillazioni e si deve pensarla come un oggetto estremamente eccitato che, per raggiungere lo stato di equilibrio, deve smorzare le sue oscillazioni ed eliminare ogni eccesso di energia. La energia così emessa raggiunge lo strato sferico e la materia interstellare che circonda la stella ed è la causa dei vari tipi di emissione che provengono da questi strati.

2. EMISSIONE TERMICA ED EMISSIONE DI SINCROTRONE. — Le considerazioni svolte fin qui hanno carattere puramente teorico. Per capire come si sia arrivati ad identificare le stelle di neutroni con certi oggetti celesti è necessario chiarire le linee generali del metodo mediante il quale gli astronomi interpretano le loro osservazioni.

Le stelle emettono varie specie di radiazioni fra le quali, per ragioni varie, la più cospicua è la luce. Finchè lo studio degli astri si limitò all'astronomia sferica ed alla meccanica celeste la luce emessa o diffusa da un astro serviva solo per segnalare la sua posizione. Ma col sorgere dell'astrofisica si cercò di scomporre questa luce nei propri colori e di ricavare le caratteristiche fisiche delle stelle dagli spettri così ottenuti.

In prima approssimazione si suppone che esistano nelle stelle regioni assai vaste che possono considerarsi in equilibrio termodinamico. In tale condizione, per ogni temperatura, esiste una distribuzione di velocità che si mantiene stazionaria. Le molecole subiscono collisioni e si scambiano energia, ma la distribuzione rimane statisticamente invariata. Quando però queste collisioni sono inelastiche, si produce emissione di radiazione per parte delle molecole, atomi, ioni ed elettroni liberi e, siccome la energia irradiata è fornita dal moto termico, questo tipo di emissione si chiama termica. Naturalmente se la energia viene irradiata in quantità sensibile, l'equilibrio termodinamico sussisterà solo se esiste una sorgente di energia per compensare la perdita subita per irradiazione e mantenere costante la temperatura. Ad ogni temperatura corrisponde una determinata energia cinetica media del moto termico, quindi una determinata intensità di eccitazione e di emissione.

Per un corpo capace di assorbire qualunque tipo di radiazione incidente sulla sua superficie, o, come si dice, per il corpo nero, la energia emessa alle varie frequenze è stata misurata da Lummer e Pringsheim e calcolata da Planck con la sua famosa teoria dei quanti. In fig. 2 sono tracciate diverse curve corrispondenti a diverse temperature.

Come si vede il massimo della emissione si sposta verso il violetto al crescere della temperatura, ma in misura maggiore o minore le sostanze condensate emettono in tutte le frequenze dalle onde hertziane ai raggi X.

Ogni gas invece emette un suo caratteristico spettro discontinuo costituito da serie di righe e da bande molecolari, e quando viene attraversato da una radiazione assorbe da essa proprio in corrispondenza di quelle righe che esso è capace di emettere, se portato a temperatura sufficientemente elevata. Così lo spettro continuo del Sole è attraversato dalle righe scure di Fraunhofer prodotte dall'assorbimento della propria atmosfera. L'osservazione in uno spettro stellare di una riga che per osservazioni di laboratorio si sa corrispondere ad una data sostanza, indica la presenza nella stella di quella sostanza. Inoltre l'intensità e la larghezza di quella riga, gli eventuali effetti Doppler e Zeeman, danno preziose informazioni sulle condizioni fisiche, chimiche,

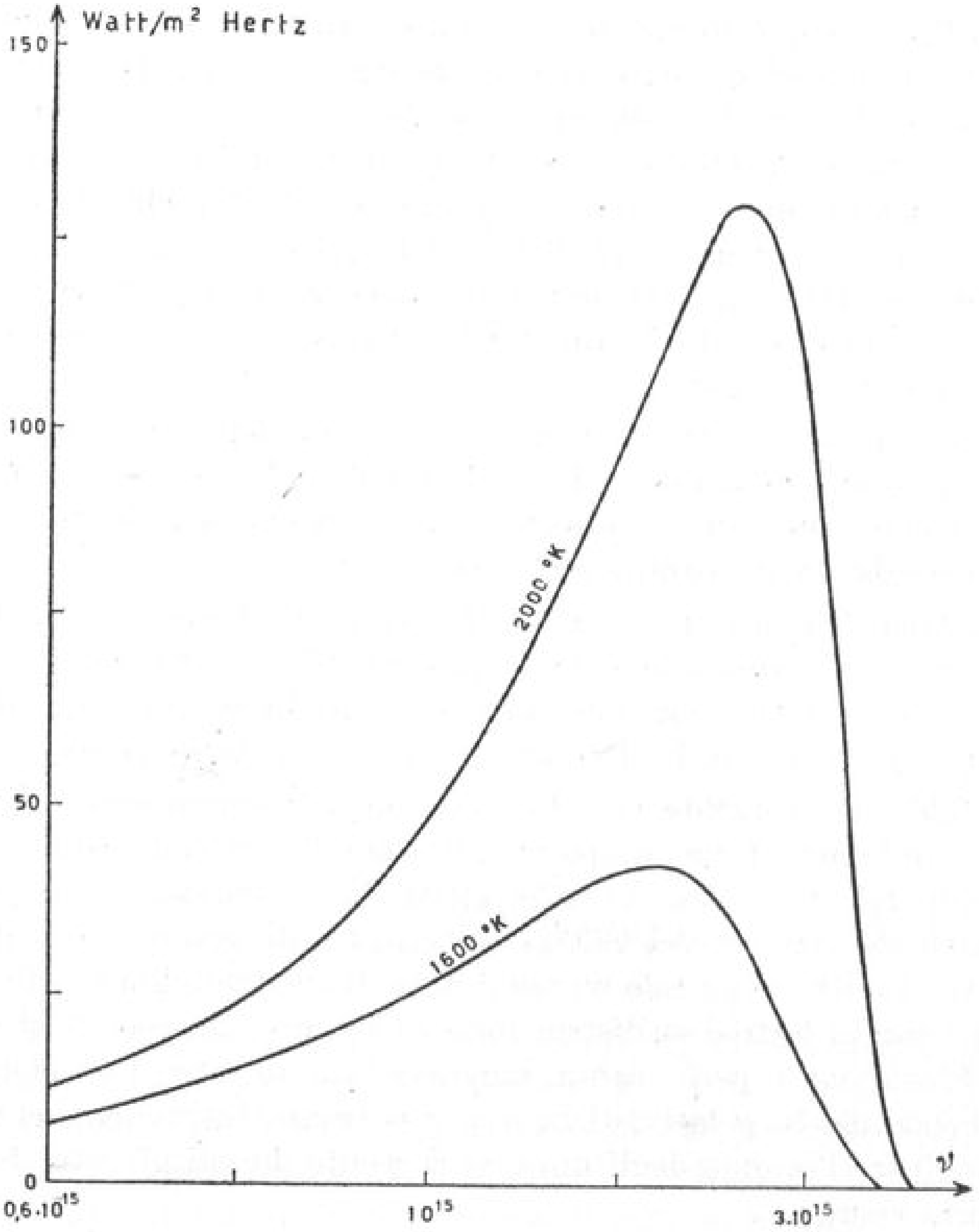

Fig. 2
Spettro del corpo nero.
Spectrum of the black body.

cinematiche e magnetiche della regione stellare in cui l'emissione ha avuto luogo. Queste informazioni però devono essere ottenute a spese di un abile e paziente lavoro. I rapporti fra le varie righe ed in generale l'interpretazione di un intero spettro pongono agli astronomi dei veri e propri indovinelli. In certi casi le difficoltà di spiegazione sono tali che si deve abbandonare l'ipotesi della emissione termica e supporre un tipo di emissione non termica.

Un fatto di questo genere si è avuto dopo la scoperta delle radiosorgenti, di oggetti celesti cioè che emettono nel dominio delle onde hertziane con intensità eccezionalmente elevata. L'interpretazione delle osservazioni mediante emissione termica portava alla conclusione che la sorgente doveva avere una temperatura assai elevata ed ad un elevato rapporto tra intensità ottica ed hertziana. Ora, anche quando si poterono scoprire oggetti celesti visibili in corrispondenza della regione del cielo in cui era stata localizzata la radiosorgente, un rapporto così elevato non venne osservato.

Nelle emissioni non termiche la intensità della radiazione non è legata alla temperatura dalla legge di Planck. La sorgente può essere fredda, come nelle usuali lampade al neon in cui la radiazione deriva la sua energia da un campo elettrico.

Un tipo di emissione non termica con cui si riesce a spiegare la emissione delle radiostelle è la radiazione di sincrotrone, così detta perchè essa ha luogo con meccanismo in parte simile a quello della macchina acceleratrice di elettroni che porta lo stesso nome.

Perchè una sorgente emetta radiazione di sincrotrone si richiede in essa la presenza di un campo magnetico e di particelle cariche dotate di energie relativistiche. Cariche elettriche si trovano ovunque nell'Universo che per più del 999‰ è costituito di plasma e cioè di fluidi ionizzati. Inoltre in un tale mezzo dotato di conducibilità praticamente infinita i campi lettrici si distruggono ed i campi magnetici si conservano. Ovviamente però campi magnetici molto intensi ed elettroni e protoni con velocità relativistiche non si potranno introdurre nei modelli senza indicare l'origine degli uni e la sorgente da cui gli altri derivano la propria energia.

Il meccanismo della radiazione di sincrotrone è il seguente. Le cariche elettriche nel campo magnetico subiscono, per effetto di questo, un'accelerazione in conseguenza della quale si muovono su spirali che si avvolgono ciascuna attorno ad una data linea di forza del campo magnetico. Ora una carica elettrica si porta dietro il suo campo elettromagnetico finchè si muove di moto uniforme; ma quando acquista un'accelerazione perde una parte del suo campo e questa parte perduta si allontana con la sua energia propagandosi sotto forma di onda elettromagnetica con la velocità della luce. Questa emissione di radiazione, dovuta in generale al fatto che la sorgente possiede un'accelerazione, ha il nome tedesco di Bremsstrahlung; se in particolare l'accelerazione è quella centripeta prodotta da un campo magnetico, la radiazione viene detta di sincrotrone.

L'energia emessa per radiazione di sincrotrone dipende dall'intensità del campo magnetico e dalla velocità delle cariche; la sua distribuzione spettrale è rappresentata da una curva del tipo di quella disegnata in fig. 3; come si vede, pur esistendo un massimo, la pendenza è molto minore di quella delle curve di fig. 2 che rappresentano la legge di Planck.

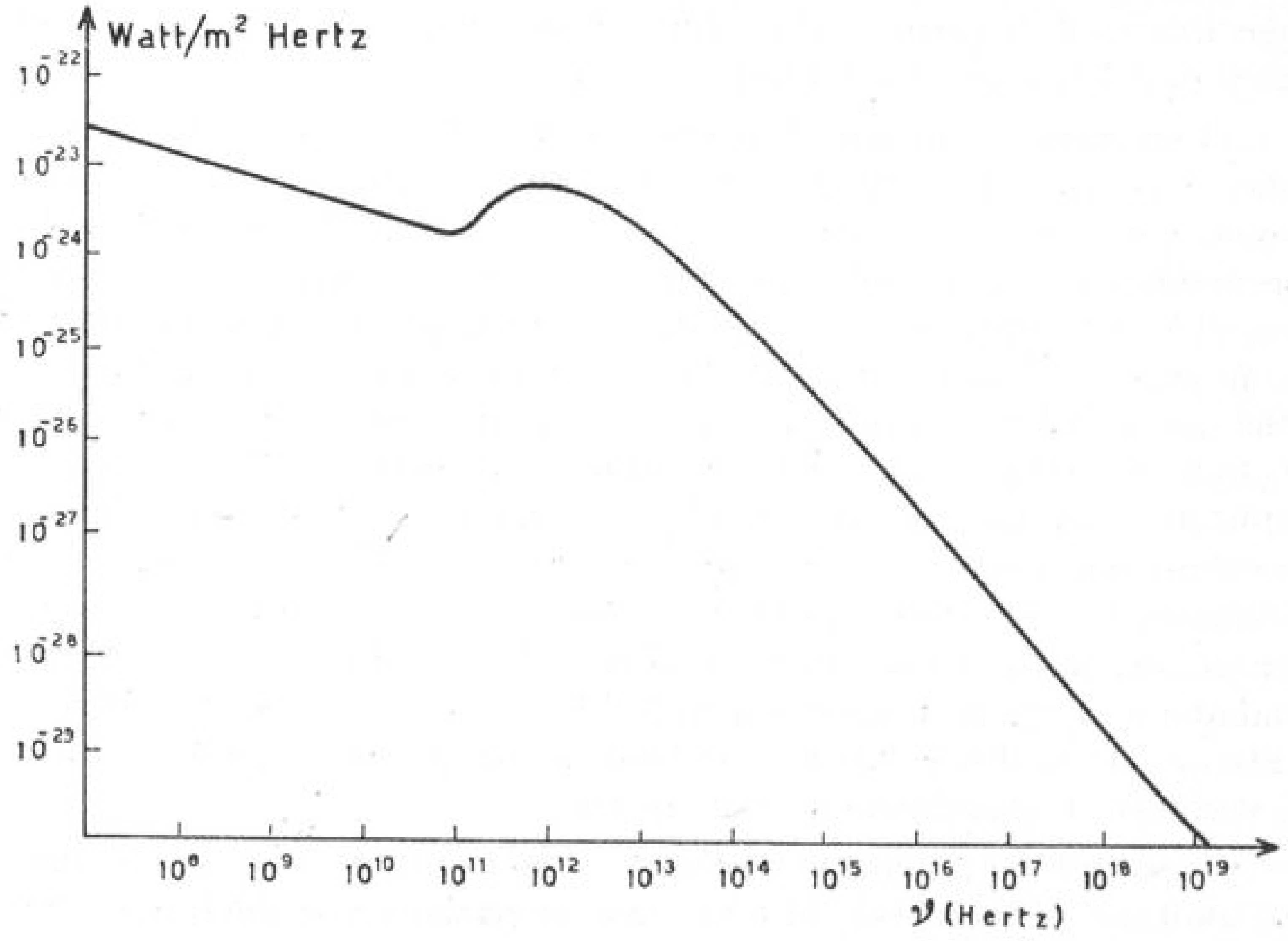

Fig. 3

Spettro della radiazione di sincrotrone emesso dalla Crab Nebula.
Spectrum of the synchrotron radiation emitted by the Crab Nebula.

Con la emissione di sincrotrone si spiegano assai bene gli spettri osservati per le radio sorgenti, il che è una prima conferma della correttezza delle ipotesi fatta. Un'altra conferma proviene dal fatto che, a differenza della radiazione termica, la radiazione di sincrotrone è polarizzata e questa polarizzazione si è potuta osservare.

Per campi magnetici molto intensi e particelle con energie estremamente elevate l'emissione si sincrotrone può essere notevole anche nell'ultravioletto e nella regione di raggi X.

La X-astronomia è ancora più recente della radioastronomia e si è sviluppata dal 1948, quando l'osservazione del cielo si è potuta compiere mediante veicoli spaziali. Gli osservatori terrestri sono completamente ciechi per i raggi X che vengono assorbiti dall'atmosfera e solo dei satelliti e razzi che si muovono al di fuori di essa possono eseguire osservazioni di X-astronomia che poi vengono radiotrasmesse a terra. In questa maniera non si è tardato a scoprire che talune regioni del cielo sono intense sorgenti di raggi X.

3. Supernovae. — Il primo tentativo di identificare un oggetto celeste con una stella di neutroni fu fatto proprio per spiegare la natura delle X-sorgenti celesti. Si pensava che una stella di neutroni formatasi in seguito a collasso potesse essere un oggetto avente una temperatura superficiale molto elevata di circa 10^7°K e quindi un'intensa sorgente termica di raggi X. Entrate nell'Astrofisica per questa via le stelle di neutroni furono ben presto associate ad un tipo di fenomeno celeste che riuscì ad attrarre sempre più l'attenzione su di esse; intendiamo parlare del fenomeno delle Supernovae.

L'apparizione di una Supernova è uno di quei fenomeni naturali che, oltre a destare stupore con le loro caratteristiche apparenti, inducono, quando se ne è compresa la vera essenza, ad una meditazione profonda sulla grandiosità prodigiosa con cui si compiono i fenomeni cosmici. Una indagine statistica mostra che in una determinata galassia il fenomeno di apparizione di una Supernova avviene in media ogni 360 anni. L'osservazione di una Supernova nella nostra galassia è quindi per l'astronomo, per così dire, una questione di fortuna e la difficoltà è aumentata dal fatto che le regioni del cielo, dove il fenomeno avviene, non sono contraddistinte da nessuna particolare caratteristica. Tuttavia con una paziente indagine in cui l'astronomo trasformandosi in storico, orientalista e filologo ha consultato antiche cronache cinesi, indiane e giapponesi, si sono potute identificare varie regioni del cielo interne alla nostra galassia nelle quali in tempi più o meno lontani si è verificata l'apparizione di Supernovae.

I residui del fenomeno originario sono costituiti da tenui filamenti ed ammassi nebulari che, in molti casi, si rivelano solo dopo accurate e faticose osservazioni fatte con strumenti molto potenti. Tre Supernovae galattiche sono famose. La Supernova osservata dagli astronomi cinesi e giapponesi nel 1054 d.C. apparve nella regione del cielo dove oggi si trova la Crab Nebula ed è uno dei più brillanti risultati della moderna astronomia quello di avere identificato la nebulosa con il residuo della Supernova. Questa nebulosa ha avuto un'importanza fondamentale in astronomia e ci occorrerà di riparlarne. Le altre due Supernovae sono state osservate, una dal famoso astronomo danese Tycho Brahe nel 1572, l'altra dal grande Keplero nel 1604. La descrizione che Tycho fa nel suo libro "De Nova Stella,, dell'apparizione della Nova Cassiopeiae è soprattutto permeata dal profondo stupore di osservare un cambiamento in quel cielo delle stelle fisse che per tutta l'antichità era stato ritenuto immutabile. Egli invoca testimoni oculari nel momento della scoperta, perchè crede ad un'allucinazione dei propri sensi e fa confronti col miracolo di Giosuè e con l'oscuramento che, secondo il Vangelo, avvenne dopo la crocefissione di Cristo. Conclude con un reverente omaggio alla divina onnipotenza del Creatore.

Dal punto di vista scientifico l'apparizione della Stella di Tycho rappresenta il momento in cui la scienza occidentale si trova per la prima volta davanti ad un fenomeno che contraddice la creduta invariabilità ed eternità delle stelle.

Gli aspetti generici del fenomeno delle Supernovae si possono riassumere nella maniera seguente. In una regione del cielo, che come abbiamo detto è assolutamente priva di caratteristiche particolari, appare una stella di luminosità rapidissimamente crescente, la cui grandezza apparente raggiunge e talvolta supera quella di Venere nel giro di pochi giorni. Talune Supernovae erano visibili in pieno giorno, mentre la loro luminosità durante la notte era paragonabile a quella della Luna tanto che si sono potute osservare ombre di oggetti prodotte dai loro raggi. Il massimo di luminosità è raggiunto dopo una variazione di 20 grandezze circa ed in seguito la diminuzione avviene più o meno regolarmente, finchè dopo un tempo dell'ordine di 200 o 300 giorni la luminosità ormai ridotta ad un valore molto modesto finisce col rimanere costante. L'oggetto dal quale la Supernova si sviluppa e le primissime fasi del fenomeno non sono state ancora osservate con certezza e qualche astronomo dubita che tale oggetto sia una stella, ma piuttosto un ammasso nebulare.

Fino al 1885 non si comprese la reale essenza del fenomeno e non si distingueva fra Supernovae e Novae, stelle queste ultime che aumentano improvvisamente la loro luminosità soltanto di una diecina di grandezze in conseguenza di un'esplosione che espelle uno strato di materia pari a 10^{-5} circa masse solari.

Nel 1885 fu scoperta la prima Supernova non galactica, la Nova Andromedae, seguita nel 1895 dalla Z Centauri ed in seguito da molte altre. La distanza della nebulosa di Andromeda, valutata nel 1919 da Lundmark a 650.000 anni luce, dava per la grandezza assoluta della Nova Andromedae il valore —15. Un tale valore non aveva precedenti ed un'idea della sua enormità si può avere dalle conseguenze che se ne traevano. Nel 1920 la questione della interpretazione di nebulose del tipo di Andromeda come « island universes », cioè come galassie simili, ma distinte dalla nostra, era ancora in discussione, nonostante che una tale idea risalisse all'epoca di Lambert nel secolo XVIII. Shapley, contrario ad una tale concezione ed in polemica con Curtis, considerava una « reductio ad absurdum » il fatto che essa conducesse a grandezze stellari, secondo lui impossibili, come quella della Nova Andromedae.

Se lo sviluppo della Astronomia si è svolto nel senso previsto da Curtis, non si può però non comprendere la perplessità di Shapley, quando si pensi che la luminosità di alcune Supernovae al loro massimo è risultata superiore a quella della intera galassia di cui esse fanno parte.

La scoperta delle Supernovae extragalactiche fece quindi capire che ci si trovava davanti ad un fenomeno avente caratteristiche sostanzialmente diverse da quello delle ordinarie Novae e nel 1934 Zwicky e Baade introdussero la denominazione di Supernovae.

Nonostante i molti caratteri comuni a tutte le Supernovae, esistono anche differenze che portano a distinguere due tipi: il tipo I ed il tipo II.

Oltre a differire per la caratteristiche fotometriche e spettrali, i due tipi sono diversamente distribuiti nello spazio. Il tipo II si manifesta nei bracci delle nebulose spirali dove è abbondante la materia interstellare. Le stelle ivi contenute sono ancora immerse nel mezzo da cui hanno avuto origine e si debbono considerare giovani. Esse debbono anche avere una massa notevole perchè durante il loro breve ciclo evolutivo non hanno avuto la possibilità di consumare molta materia.

Mentre non si hanno esempi di Supernovae di tipo II, apparse in nebulose di tipo ellittico, le Supernovae di tipo I appaiono, sia nelle nebulose di tipo ellittico, che nei nuclei delle nebulose spirali, tanto le une che gli altri poveri di materia e contenenti stelle vecchie e di piccola massa.

Le supernovae di tipi I sono quindi da associare a stelle vecchie, mentre il tipo II è legato a stelle giovani.

Ma quale può essere questo tipo di legame? Hoyle e Fowler hanno supposto che il fenomeno di Supernova consista in una catastrofe stellare in cui il nucleo della stella ha subito il collasso, mentre gli strati più esterni sono stati espulsi improvvisamente a velocità assai elevate.

Questa ipotesi è confermata dal fatto che gli spettri delle Supernovae di tipo II si spiegano come dovuti ad una grande quantità di materia, non inferiore ad una massa solare, ma probabilmente molto maggiore, che si espande con velocità valutata a 6000 km/sec.

Per le supernovae di tipo I invece la massa in espansione è molto minore.

Il tipo I di Supernovae deriverebbe quindi dalla esplosione di una stella vecchia e di piccola massa, mentre per il tipo II l'esplosione avrebbe luogo in una stella giovane e di grande massa.

I residui delle Supernovae galactiche sono spesso identificabili con uno strato nebulare che si espande più o meno simmetricamente divergendo da un punto centrale. Le prime osservazioni verso questo centro non hanno portato, in un primo tempo, a nessun oggetto degno di considerazione. Successivamente invece questa indagine ha avuto risultati sensazionali.

4. CRAB NEBULA. — La relazione fra Supernovae e stelle di neutroni verrà stabilita nel seguito di questo articolo per un determinato residuo di Supernova, avvertendo che considerazioni analoghe sono state ripetute in altri casi.

L'oggetto celeste a cui ci riferiremo è senza dubbio la nebulosa più famosa e la più studiata nell'Astronomia moderna e cioè la Crab Nebula.

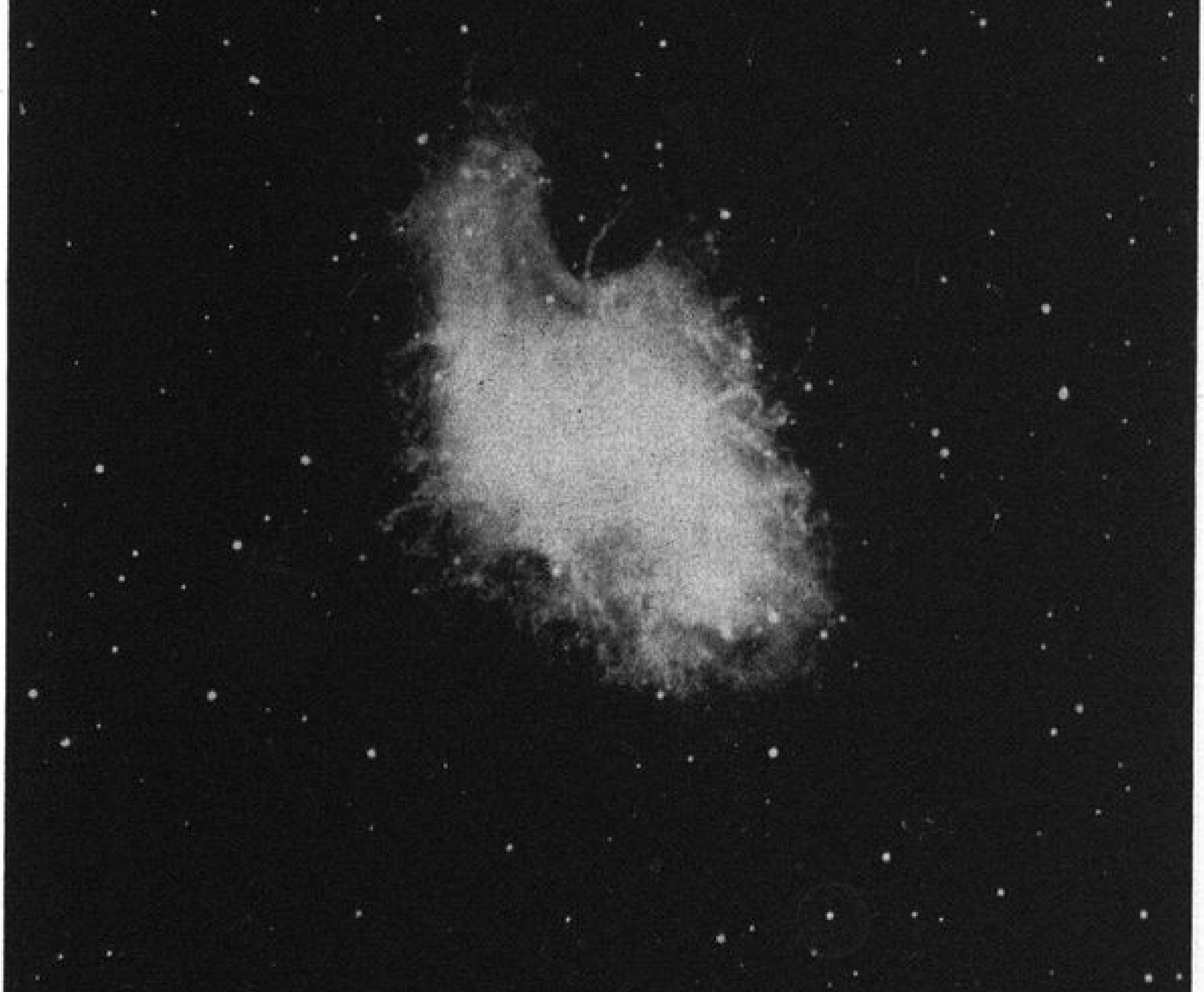

Fig. 4 - Nebulosa del Granchio nel Toro (The Crab Nebula in Taurus)

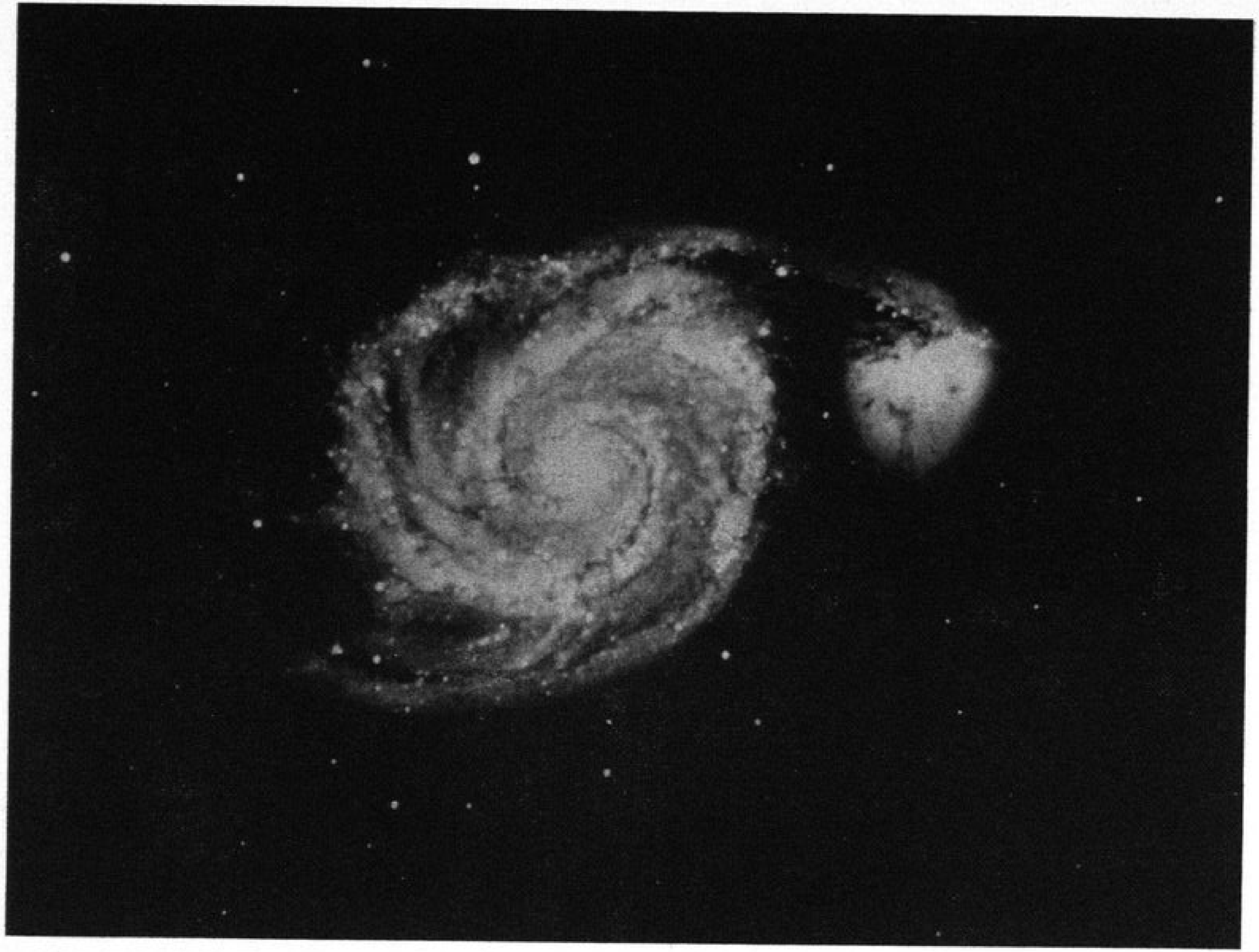

Fig. 5 - Nebulosa a spirale nei Canes Venatici (Spiral nebula in Canes Venatici)

La Crab Nebula nel catalogo di nebulose di Messier è indicata M1, mentre nel New General Catalogue di Dreyer ha il simbolo N.G.C. 1952. Scoperta da John Bevis nel 1731, fu il primo oggetto galattico identificato con una radio sorgente nel 1949 e con una X-sorgente nel 1963; fu anche la prima sorgente cosmica la cui emissione si rivelò non termica e conseguentemente polarizzata ed infine fu il primo residuo di supernova identificato con un oggetto celeste da Hubble (1928) e da Lundmark (1938). La velocità di espansione della Crab Nebula è stata valutata da Baade a 789 km/sec e tale velocità coincide con quella di un oggetto che ha raggiunto le dimensioni attuali 900 anni dopo la sua esplosione. Come abbiamo già detto un'origine termica per la radiazione della Crab Nebula appare insostenibile, mentre il suo spettro si accorda assai bene con quello di una radiazione di sincrotrone in tutto l'intervallo di emissione, raggi X, ultravioletto, visibile, infrarosso ed onde hertziane (v. fig. 3). Ma una tale interpretazione richiede la presenza nella materia nebulare di particelle relativistiche e si deve quindi indicare il meccanismo che accelera elettroni e protoni fino a far loro raggiungere tali elevatissime energie.

Nell'astrofisica è assai comune il caso di un ammasso nebulare di per se oscuro la cui luminosità viene eccitata dalla intensa emissione di una stella vicina. Un caso tipico è quello delle nebulose planetarie. Ma una tale spiegazione non si può invocare, non solo perchè la emissione che ne deriva è termica, ma anche e soprattutto perchè nessuna stella luminosä è presente.

Nella ipotesi di Hoyle e Fowler si deve però ammettere che nell'interno della Crab Nebula, considerata come residuo della Supernova del 1054, esiste il nucleo che ha subito il collasso. Se si suppone che questo oggetto collassato sia una stella di neutroni si spiega il fatto che esso non è visibile e, d'altra parte, che esso sia in uno stato così eccitato da poter essere invocato come sorgente dell'energia necessaria per spiegare l'emissione di sincrotrone.

In questo modo si arriva al modello di residuo di una Supernova costituita da una stella di neutroni circondata da ammassi nebulari emittenti radiazione di sincrotrone.

Rimangono però ancora due questioni da risolvere.

La prima riguarda ancora la stabilità. Una semplice occhiata al diagramma in figura 1 mostra che la massa di una stella di neutroni in condizioni statiche non può superare 0.7 masse solari (mentre per le Nane Bianche questo limite è 1.2 masse solari, cioè di poco superiore). Se si supera questi limiti le stelle, non incontrando più nessuna configurazione di equilibrio, subiscono un collasso illimitato; raggiungono ed oltrepassano una dimensione critica (raggio di Schwarzschild) oltre la quale la curvatura dello spazio, prodotta dalle masse secondo la teoria generale della relatività, raggiunge valori tali che la stella si chiude in sè stessa, non può più emettere radiazione e manifesta la sua presenza solo per mezzo del suo intenso campo gravitazionale.

La condizione $M < 0.7$ *masse solari* è molto severa ed in contrasto con alcune proprietà delle Supernovae e perciò Hoyle, Narlikar e Wheeler (1964) hanno cercato una via di uscita a questa difficoltà, osservando che la rotazione esercita un'azione stabilizzatrice opponendosi al collasso mediante la forza centrifuga e che in conseguenza per una massa ruotante il limite di stabilità può considerarsi notevolmente superiore.

La seconda questione consiste nello spiegare come la stella di neutroni trasmette la sua energia agli strati nebulari circostanti.

In una stella di neutroni esiste una percentuale di elettroni e protoni che assicura al materiale stellare una conducibilità elettrica assai elevata, tale da legare indissolubilmente le cariche elettriche alle linee di forza magnetiche e dare a queste ultime (che secondo il modello elettromagnetico di Faraday sono da « pensarsi » come elastici in tensione) una esistenza concreta. I violenti movimenti di cui è sede una stella di neutroni eccitano in tal modo nelle linee magnetiche oscillazioni che si propagano lungo di esse come lungo una corda vibrante. Secondo i tre autori nominati sopra ad una certa distanza dalla stella si verificano condizioni per cui le suddette onde, che sono chiamate onde magnetoidrodinamiche, si trasformano in ordinarie onde elettromagnetiche. Queste onde, dopo aver attraversato il vuoto creato dal fortissimo campo gravitazionale della stella nelle sue vicinanze, pervengono agli strati nebulari e sono da questi riflesse in parte. Come conseguenza della riflessione viene esercitata una compressione ed è questa compressione degli strati nebulari che fornisce il meccanismo microscopico per la accelerazione delle particelle fino alle velocità relativistiche.

Ma questo modello è insostenibile. Infatti nel quadro della relatività generale in cui si deve trattare il problema delle stelle di neutroni non si possono trascurare le onde gravitazionali e cioè l'analogo delle delle onde elettromagnetiche per il campo gravitazionale. Ora Zee e Wheeler, e Meltzer e Thorne hanno mostrato che la emissione di tali onde prodotte dalle oscillazioni di una stella di neutroni ruotante eliminerebbe tutta l'energia disponibile nel giro di pochi secondi o, nella ipotesi più favorevole, in un tempo che si aggira sull'ordine di un giorno. Il meccanismo di Hoyle e collaboratori è quindi assolutamente inadatto a spiegare un fenomeno che nella Crab Nebula dura da mille anni e non accenna a cessare.

Un'alternativa è stata proposta da Pacini (1967) col suo modello di rotatore obliquo. Pacini suppone che dopo avere eliminato l'eccesso di energia termica mediante la emissione di neutrini ed avere smorzato le sue vibrazioni con l'emissione delle onde gravitazionali, la stella si riduce ad un corpo ruotante attorno ad un asse diverso dall'asse di simmetria del campo magnetico. In queste condizioni essa emette direttamente (cioè senza onda magnetoidrodinamica intermediaria) un'onda elettromagnetica monocromatica con pulsazione uguale alla velocità angolare. Questa onda poi agisce sugli strati nebulari, da cui viene riflessa, con lo stesso meccanismo previsto da Hoyle, Narlikar e Wheeler.

Nello stesso 1967 il gruppo di ricercatori del Mullard Radio Astronomy Observatory dell'Università di Cambridge (Inghilterra) con a capo Hewish e di cui facevano parte Bell, Pilkington, Scott e Collins scoprì la prima Pulsar. Come d'abitudine nella scienza, si tentò dapprima di spiegare il nuovo fenomeno senza fare ipotesi ad hoc e senza introdurre nuovi modelli. Ma l'ipotesi che i segnali inviati dalle Pulsar fossero prodotti da oscillazioni nelle Nane Bianche ed altre ipotesi si mostrarono ben presto insostenibili. Un modello eccellente in accordo coi fatti osservati si rivelò invece quello costituito da una stella di neutroni in rapida rotazione.

A questo punto la storia della Crab Nebula registra la scoperta di una pulsar che con i suoi segnali rivela la presenza di quella stella di neutroni che fino ad allora era stata una semplice ipotesi. Al momento attuale le stelle di neutroni sembrano indissolubilmente legate alle Pulsars e la loro storia si identifica con quella delle Pulsars stesse che saranno oggetto di un futuro articolo.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai e G. Righini

TRANSLATION: P. MOULE - FLORENCE

SUPERNOVAE AND NEUTRON STARS

The considerations are set out (based essentially on the stability of the equilibrium of fluid spheres) that have caused astrophysicists to consider possible the existence of a special type of star called neutron stars. Later it is shown how neutron stars came to be identified with residues of that particular phenomenon that goes under the name of Supernova.

1. NEUTRON STARS. — We must say at once that the discovery of neutron stars, instead of by means of the telescope, was made by a computer and for some time remained an idea of theoretical astrophysicists, who succeeded in calculating a possible star model with very special characteristics and not corresponding to any observed celestial body, in fact a neutron star. To appreciate the value of such a discovery it is necessary to be clear about what is meant by a stellar model and above all the conditions that will make the model acceptable. We shall begin by saying that a star is nothing but a kind of machine that produces energy in its interior and radiates it through its surface. Very little can be observed of a star. Under the best conditions we can know its luminosity, distance, mass, radius, rotation and we can obtain information about its chemical composition and its magnetic field in the outer layers. From these few elements theoretical astrophysicists have to draw conclusions about the internal structure of the star. Starting from a fluid sphere they attempt to calculate a pressure, temperature and density distribution inside the sphere so that the « model » obtained corresponds as closely as possible to the observed characteristics of the star being studied. Fundamental elements for the calculation of a model are the type of nuclear reaction producing the energy, the temperature, the central pressure and density, the chemical composition and the state equation of the material forming the star. The transfer of energy in the interior of the star is usually due to radiation, but there may be regions where radiative transfer is not sufficient to maintain too steep a temperature gradient and then the material collaborates in the energy transfer starting to move and then the previous radiative equilibrium becomes convective.

Models corresponding very closely to real stars have been calculated in this way and today knowledge of interstellar structure is highly developed. However the problem does not end here and when we have constructed a model we must assure ourselves of its « stability ».

Stars are found in a quasi stationary state. Variations in their structure inevitably occur in periods of time to the order of millions of years and more, when the star, having exhausted a certain nuclear fuel, obtains the energy necessary for its existence from a momentary gravitational contraction and then from combustion of other nuclear material made possible in the new conditions. These changes, which make the star pass from one state of equilibrium to another, constitute the phenomenon of stellar evolution. During its evolution the star uses up a large part of its nuclear fuel and its gravitational energy, reaching the stage of an « old star », that is to say the type of star known as a « White Dwarf ».

But in the stars, besides these very slow changes, there may be violent phenomena taking place in a very short time and that may considerably change the equilibrium conditions. In such a case two things may occur: either forces are generated that bring the star back towards the equilibrium configuration that is reached after a series of damped oscillations (for a variable a star the oscillations may be undamped); or the star gets further and further from the equilibrium configuration exploding or contracting or developing oscillations of increasing amplitude.

The stellar models must be of the first type, that is they must be stable; a model of the second type is unstable and cannot represent a real star, but at the most a catastrophic phenomenon and collapse. When we speak of neutron stars it must not be thought that astronomers began arbitrarily to calculate star models formed from these elementary particles. First of all there would be no reason to think of neutrons rather than other particles; in the second place the fact that the calculation results produced not only strange bodies, but also ones which were unobserved and almost impossible to observe, would have made the research worthless. On the contrary neutron stars have presented themselves on the scene of cosmological research owing to their right to exist, a right that just because of their stability it is impossible to deny them. One of the most strange and paradoxical circumstances is that the essential elements concerning neutron stars were discovered by Landau in 1932 before Chadwick, in the same year, announced his discovery of the neutron.

The discovery of neutron stars is connected with the instability that may arise in stars of low temperature and very high central density. For central densities of 10^8g/cm^3 (100 tons per cubic centimetre) and more, the instability arises from the fact that the models must be calculated by using Einstein's general theory of relativity. Now this theory predicts a phenomenon that goes under the name of « regenerative multiplication of pressure » and that basically consists of the fact that the pressure, which is energy per unit of volume and as such corresponds relativistically to mass per unit of volume ($E = mc^2$), has its own weight. If then to prevent the collapse of a star, the layers of which are very dense and thus very heavy, the pressure is increased, also in this way the weight of the layers increases and a critical configuration is reached beyond which any increase in the central density leads inevitably to collapse, because no pressure distribution can « stably » balance the weight. If the situation were schematizable in this way there would be a single critical density with unstable configurations for higher central densities and stable for lower ones. Actually things are more complicated. The pressure is a function of the density through the state equation and this equation is determined by the microscopic behaviour of the material. Now at the low temperatures and the high densities of 10^8g/cm^3 characteristic of the type of stars mentioned above, the White Dwarfs, the electron gas must be considered degenerate and its properties are determined by the Fermi-Dirac statistic. This means, roughly speaking, that not more than two electrons can have the same velocity. As regards our problem, the most important property is that a degenerate gas exerts a much higher pressure than an ordinary gas and, since in White Dwarfs only the electron gas is degenerate, while the gases formed by the other particles are not, it follows that the pressure is produced practically by the electrons alone.

If now the pressure and the density at the centre of the star increase, we arrive, for a central density of between 10^8 and 10^9g/cm^3, at a new fact. The electrons can no longer sustain these very high compressions, or rather, the energy balance is such that the electrons tend to combine with the protons of the nuclei forming neutrons. That is we have the reaction (inverse β-decay): electron + proton bound to a nucleaus → free or bound neutron + neutrino. In symbols:

$$e^- + p \longrightarrow n + \nu.$$

In this situation the electrons are crushed against the protons and no longer exert the pressure necessary to support the layers above. The star thus becomes unstable as regards to collapse.

If the central density increases still further the stellar material is transformed into a neutron gas and there is a return to stability for a central density of between 3.10^{13}g/cm^3 and 6.10^{15}g/cm^3. Beyond this limit stable configurations are not known.

This discovery of a stability interval for models of stars consisting of neutrons, beyond the stability limit of White Dwarfs and between two instability intervals (see fig. 1) introduced into astrophysics the idea of the possible existence of neutron stars.

Historically Oppenheimer, Serber and Volkoff introduced the idea of the possible formation of neutrons by means of the inverse β decay reaction at very high pressures (1938) and used the relativity treatment (1939) for a star consisting of a degenerate gas of neutrons. The question was treated systematically by Harrison, Wakano and Wheeler. Landau was able to arrive at his surprising result in 1932 before the discovery of neutrons for this reason: when the energy of Fermi's degenerate gas becomes relativistic the intrinsic energy (mass) of the particle becomes negligible and energy and pressure cease to depend on the type of particle, while they depend only on the dimension of the volume within which the particle is confined.

Obviously we cannot name all the scientists who have taken part in this interesting field of research and we shall only mention, besides the preceding, Chadrasekhar, Misner, Zapolsky and Thorne.

We have said that the range of existence and stability of neutron stars is isolated. If in fact we proceed towards stars having lower densities (see fig. 1) we find the instability interval that separates neutron stars from White Dwarfs. The behaviour of stars with central densities higher than those of neutron stars has not been completely studied. It cannot be excluded that in this range there exist models of stable stars. However two things can be stated with certainty: if the density increases beyond all limits the models become unstable; also there is certainly an instability zone contiguous to that of the existence of neutron stars on the side of the higher central density.

We must here point out that the sequence in figure 1 does not represent a series of configurations that a star can successively assume during its evolution. The models were calculated starting from the central density and each corresponds to a different mass-energy and to a different number of baryons. From this it follows on a basis of the law of conservation that as one model evolves it cannot be transformed into the following one. A sequence of models representing the evolution of a certain star must correspond to a determined number of baryons equal for all models. These sequences are not easy to construct and have a different appearance to that of the curve in fig. 1.

However in any case, if one admits the very reasonable hypothesis that the neutron stars constitute a stage, though particular, in stellar development, that is to say that other stars are changed into neutron stars, we must admit that the stars have undergone this change overcoming an instability interval, that is to say catastrophically.

In conclusion a neutron star is a celestial body formed discontinuously and violently from a star that has undergone a collapse of its central part and perhaps also an explosion of the peripheric layers. The result of the catastrophe is therefore a collapsed central nucleus surrounded by a layer of material that is dispersed in space owing to the velocity conveyed to it by the explosion.

The nucleus is a celestial body that from the size of a dwarf star like the sun with a radius of 695,700 km has been reduced to a radius of about 10 km, characteristic of a neutron star (see fig. 1). This contraction has produced extraordinary results. Owing to the conservation of the angular moment its rotating speed, even though initially modest (for the sun one revolution in about a month), must assume values such that the consequent centrifugal force reduces the original spherical shape to that of a disk. The magnetic field, which on the sun's surface amounts to a fraction of a gauss, for the conservation of the magnetic induction flow, rises to values of the order of 10^{14} gauss.

And finally in a rapid contraction like that due to the collapse, in which the velocity of free fall is reached, the temperature must rise considerably even if the increase is counterbalanced by the emission of neutrinos and the fact that the inverse β-decay reaction becomes endothermic at the very high pressures existing in the neutron star.

As can be easily understood the original star does not arrive quietly at its new state of equilibrium. The catastrophic way in which this new configuration is reached causes the neutron star to be the site of violent oscillations and it must be thought of as an extremely excited body that, to reach a state of equilibrium, has to damp its oscillations and eliminate all excess energy. The energy thus emitted reaches the spherical layer and the interstellar material surrounding the star and is the cause of various types of emission from these layers.

2. THERMAL EMISSION AND SYNCHROTRON EMISSION. — So far the arguments have had a purely theoretical character. To understand how it has been possible to identify neutron stars with certain celestial bodies it is necessary to make clear the general lines of the method by which astronomers interpret their observations.

Stars emit various types of radiation among which, for several reasons, the most conspicuous is light. As long as the study of stars was limited to spherical astronomy and celestial mechanics the light emitted or diffused by a heavenly body served only to indicate its position. But with the development of astrophysics it was attempted to resolve this light into its different colours and to discover the physical characteristics of the star from the spectra thus obtained.

As first approximation it is assumed that there exist in the stars very large regions that can be considered as in thermodynamic equilibrium. In this condition, for each temperature, there is a velocity distribution that remains stationary. The molecules collide and exchange energy but the distribution remains statistically unvaried. When however these collisions are inelastic, radiations are produced by the molecules, atoms, ions and free electrons and, since the radiated energy is supplied by thermal motion, this type of emission is called thermal. Of course if the energy is radiated in considerable quantity, the thermal equilibrium will exist only if there is a source of energy to compensate for the loss due to radiation and to maintain the temperature constant. To each temperature corresponds a certain average kinetic energy of the thermal motion and therefore a certain intensity of excitation and emission.

For a body capable of absorbing any type of radiation falling on its surface, or as it is called the « black body », the energy emitted at different frequencies has been measured by Lummer and Pringsheim and calculated by Planck with his famous quantum theory. In fig. 2 several curves corresponding to various temperatures are traced.

As is seen the maximum emission is displaced towards violet with the increase in temperature, but to a greater or lesser degree the condensed substances transmit on all frequencies from hertzian waves to X-rays.

Each gas on the contrary emits its characteristic discontinuous spectrum consisting of series of lines and molecular bands, and when crossed by a radiation it absorbs from it just in correspondence with those lines that it is capable of emitting, if brought to a sufficiently high temperature. Thus the continuous spectrum of the sun is crossed by the dark Fraunhofer lines produced by the absorption of its atmosphere. The observation in a stellar spectrum of a line that from laboratory observations is known to correspond to a given substance, indicates the presence in the star of that substance. Besides the intensity and the width of that line, the Doppler and Zeeman effects, give valuable information on the physical, chemical, kinematic and magnetic conditions of the stellar regions in which the emission took place. This information has however to be obtained by means of skillful and patient work. The relationships between the various lines and the interpretation of a whole spectrum pose real puzzles for astronomers. In certain cases the difficulties of interpretation are such that the hypothesis of thermal emission has to be abandoned and a type of non-thermal emission must be assumed.

A fact of this kind occurred after the discovery of radiosources, that is of celestial bodies which emit in the range of hertzian waves with exceptionally high intensity. The interpretation of the observations by means of thermal emission led to the conclusion that the source must have a very high temperature and a high ratio between optic and hertzian intensity. Now even in the case where a celestial object was discovered in the part of the sky in which a radiosource has been observed, such a high ratio was not observed.

In non-thermal emissions the intensity of radiation is not connected with the temperature by Planck's law. The source may be cold, as in the common neon tube in which the radiation derives its energy from an electric field.

A type of non-thermal emission by which it is possible to explain the emission of radio stars is synchrotron radiation, so called because it takes place with a mechanism similar to that of the electron accelerator of the same name.

For a source to be able to emit synchrotron radiation the presence in it of a magnetic field and charged particles having relativistic energy is required. Electric charges are found everywhere in the universe that for more than 999‰ consists of plasma, that is of ionized fluids. Besides, in such a medium having almost infinite conductivity the electric fields are destroyed and the magnetic fields are conserved. Obviously however very intense magnetic fields and electrons and protons with relativistic velocities cannot be introduced into the models without indicating the origin of the former and the source from which the latter derive their energy.

The synchrotron radiation mechanism is as follows. The electric charges in the magnetic field undergo by its effect, an acceleration in consequence of which they move in spirals each turning round a given line of force of the magnetic field. Now an electric charge is followed by its electromagnetic field as long as it has uniform motion; but when it acquires an acceleration it loses part of its field and this lost part is dispersed with its energy, propagating itself in the form of electromagnetic waves having the speed of light. This radiation emission, usually due to the fact that the source possesses an acceleration, is called by the German name of bremsstrahlung; if in particular the acceleration is a centripetal one and is produced by a magnetic field, the radiation is called synchrotron radiation.

The energy emitted by synchrotron radiation depends on the intensity of the magnetic field and on the velocity of the charges; its spectral distribution is represented by a curve of the type shown in fig. 3; as is seen, though there is a maximum, the slope is much less than that of the curve in fig. 2 which represents Planck's law.

Synchrotron emission gives a good explanation of the spectra observed from radio sources, which is a first confirmation of the correctness of the hypothesis formed. Another confirmation is derived from the fact that, differently from thermal radiation, synchrotron radiation is polarized and this polarization has been observed.

For very intense magnetic fields and particles with extremely high energy synchrotron emission can be considered even in the ultraviolet and in the X-ray ranges.

X-astronomy is even more recent that radio-astronomy and has developed since 1948 when the observation of the sky by means of space vehicles became possible. Terrestrial observers are completely blind to X-rays, which are absorbed by the atmosphere, and only satelites and rockets moving outside it can carry out X-astronomy observations which are then transmitted to earth. In this way there was no delay in discovering some regions of the sky to be intense sources of X-rays.

3. SUPERNOVAE. — The first attempt to identify a celestial body with a neutron star was made just for the purpose of explaining the nature of celestial X-sources. It was thought that a neutron star formed after collapse might be a body having a very high surface temperature of about 10^7 °K and thus be an intense thermal source of X-rays. Having entered astrophysics in this way neutron stars were very soon associated with a type of celestial phenomenon that succeeded in attracting more and more attention to itself; that is to say the phenomenon of the supernovae.

The appearance of a supernova is one of those natural phenomena that, besides arousing astonishment by their visible characteristics, lead, when their true essence is understood, to profound meditation of the extraordinary grandeur with which cosmic phenomena occur. A statistical investigation shows that in any particular galaxy the appearance of a supernova occurs on the average every 360 years. The observation of a supernova in our galaxy is thus for the astronomer, so to say, a matter of luck and the difficulty is increased by the fact that the regions of the sky where the phenomenon occurs, are not marked by any special characteristics. However, by patient investigation in which the astronomer, transformed into historian, orientalist and philologist, consulting ancient Chinese, Indian and Japanese chronicals, it has been possible to identify various regions of the sky within our galaxy where in more or less distant times a supernova has been observed.

The residues of the original phenomenon consist of nebular filaments and masses that, in many cases, reveal themselves only after careful and fatiguing observations with very powerful instruments. There are three famous galatic supernovae. The supernova observed by Chinese and Japanese astronomers in 1054 A.D. appeared in that part of the sky where we now find the Crab Nebula and the identification of the nebula with the residue of the supernova is one of the most brilliant achievements of modern astronomy. This nebula has a fundamental importance in astronomy and we shall need to speak of it again. Of the other two supernovae, one was observed by the famous Danish astronomer Tycho Brahe in 1572 and the other by the great Kepler in 1604. Tycho's description in his book « De Nova Stella » of the appearance of the Nova Cassiopeiae is above all full of great astonishment at observing a change in that celestial sphere of the fixed stars that for all antiquity had been considered unchangeable. He invokes eye witnesses at the moment of the discovery, because he believes in an hallucination of his own senses and makes a comparison with the miracle of Joshua and with the darkness that, according to the Gospels, occurred after the crucifixion of Christ. He concludes with a reverant homage to the devine omnipotence of the Creator.

From the scientific point of view the appearance of Tycho's star represents the moment in which western science finds itself for the first time before a phenomenon that contradicts the belief in the invariability and eternity of the stars.

The general aspect of the phenomenon of the supernovae can be summed up as follows. In a region of the sky, which as we have said is completely without special characteristics, there appears a star of rapidly growing luminosity, the apparent magnitude of which equals and sometimes exceeds that of Venus in the space of a few days. Some supernovae were visible in full daylight, while their luminosity was comparable to that of the moon so that it was possible to see the shadows of objects produced by their rays. Maximum luminosity is reached after a variation of about 20 magnitudes and then a decrease occurs more or less regularly, until after a period to the order of 200 to 300 days the luminosity, now reduced to a very modest value remains constant. The body from which the supernova develops and the very earliest stages of the phenomenon have not yet been observed with certainty and some astronomers think that this body is not a star but a nebular mass.

Until 1885 the essential nature of the phenomenon was not understood and no distinction was made between supernovae and novae, these latter being stars that suddenly increase their luminosity by only ten magnitudes in consequence of an explosion that expells a layer of matter equal to about 10^{-5} of the solar masses.

In 1885 the first non-galactic supernova was discovered, the Nova Andromedae, followed in 1895 by the Z Centauri and then by many others. The distance of the Andromeda nebula calculated in 1919 by Lundmark at 650,000 light years, gave the value —15 as the absolute magnitude of the Nova Andromedae. Such a value had no precedents and an idea of its enormity can be obtained from the consequences that followed. In 1920 the question of the interpretation of nebulae of the Andromeda kind as « island universes », that is as galaxies similar but distinct from our own was still being discussed, in spite of the fact that the idea went back to the time of Lambert in the 18th century. Shapley, opposed to this concept and in controversy with Curtis, considered as a « reductio ad absurdum » the fact that it led to stellar magnitudes, according to him impossible, such as that of the Nova Andromedae.

If the development of astronomy has followed the direction foreseen by Curtis, we cannot fail however to understand Shapley's perplexity, when we remember that the luminosity of some supernovae at their maximum is greater than that of the whole galaxy of which they form part.

Thus the discovery of extra-galactic supernovae made it clear that we are confronted with a phenomenon having a substantially different character from that of ordinary novae and in 1934 Zwicky and Baade introduced the name supernovae.

Notwithstanding the many features common to all supernovae, there are also differences that lead to the distinguishing of two types: type I and type II.

Besides differing in their photometric and spectral characteristics, the two types are distributed differently in space. Type II appears in the arms of the spiral galaxies where there is abundant stellar matter. The stars here contained are still immersed in the medium from which they drew their origin and are to be considered young. They must also have a considerable mass because during their short evolutive life they have not been able to consume much matter.

While there are no examples of type II supernovae appearing in elliptical type galaxies, type I supernovae appear both in the elliptical type galaxies and in the nuclei of the spiral nebula, both being poor in matter and containing old stars of low mass.

Type II supernovae are thus to be associated with old stars, while type II are connected with young stars.

But what can this type of connection be? Hoyle and Fowler supposed that the supernova phenomenon consisted of a stellar catastrophe in which the nucleus of the star underwent collapse, while the outer layers were expelled suddenly at very high velocities.

This hypothesis is confirmed by the fact that the type II supernova spectra are explained as being due to a large quantity of matter, not less than the solar mass, and probably much greater, that expands with a velocity calculated at 6000 km/sec.

For type II supernovae instead the expanding mass is much less.

Thus type I supernovae would originate from the explosion of a low mass old star, while for type II the explosion would take place in a high young star.

The residue of galactic supernovae is often identifiable with a nebular layer that expands more or less symmetrically diverging from a central point. The first observations towards this centre did not lead, at the beginning, to the discovery of any body worthy of consideration. Later however this investigation had sensational results.

4. Crab Nebula. — The relationship between supernovae and neutron stars will be established later in this article for a determined supernova residue, noting that similar considerations have been repeated in other cases.

The celestial body to which we shall refer is undoubtedly the most famous and most studied stellar nebula in modern astronomy, the Crab Nebula.

In the nebulous catalogue of Messier the Crab Nebula is indicated by M1, while in the New General Catalogue of Dreyer it has the symbol N.G.C. 1952. Discovered by John Bevis in 1731, it was the first galactic body identified with a radio source in 1949 and with an X-source in 1963; it was also the first cosmic source the emission of which was found to be non-thermal and consequently polarized and finally it was the first supernova residue identified with a celestial body by Hubble (1928) and by Lundmark (1938). The expansion velocity of the Crab Nebula has been calculated by Baade at 789 km/sec and this velocity coincided with that of a body that has reached its presents size 900 years after its explosion. As we have already said a thermal origin for the Crab Nebula seems unacceptable, while its spectrum agrees quite well with a synchrotron radiation in the whole emission interval, X-ray, ultraviolet, visible, infrared and hertzian wave (see fig. 3). But this interpretation requires the presence in the nebula material of relativistic particles and it is therefore necessary to indicate the mechanism that accelerates electron and protons until they reach such high energies.

In astrophysics the case is very common of a nebular mass, in itself dark, the luminosity of which is excited by the intense emission of a nearby star. A typical case is that of the planetary nebulae. But such an explanation is not acceptable, not only because the emission deriving from it is thermal, but also and above all because no luminous star is present.

On Holye and Fowler's hypothesis it must be admitted however that inside the Crab Nebula, considered as residue of the 1054 Supernova, there exists the nucleus that has undergone collapse. If we assume that this collapsed body is a neutron star this explains the fact that it is invisible and on the other hand, that it is in such an excited state as to be considered as a source of the energy necessary to explain the synchrotron emission.

In this way we arrive at the model of a supernova remnant consisting of a neutron star surrounded by nebular masses emitting synchrotron radiation.

However two questions still remain to be solved.

The first again concerns the stability. A glance at the diagram in fig. 1 shows that the mass of a neutron star under static conditions cannot exceed 0.7 solar mass (while for the White Dwarf this limit is 1.2 solar mass, that is a little higher). If this limit is exceeded the stars, no longer encountering any equilibrium configuration, undergo unlimited collapse; reaching and exceeding a critical size (Schwarzschild's radius) beyond which the curvature of space, produced by the mass according to the general theory of relativity, reaches values such that the star is closed in itself and can no longer emit radiation and shows its presence only by means of its intense gravitational field.

The condition $M < 0.7$ *solar mass* is very severe and in contradiction with some other properties of supernovae, and so Hoyle, Narlikar and Wheeler (1964) sought a way out of this difficulty, observing that rotation exercises a stabilizing action opposing collapse by means of the centrifugal force and that in consequence for a rotating mass the stability limit can be considered as notably higher.

The second question is to explain how the neutron star transmits its energy to the surrounding nebular layers.

In a neutron star there is a percentage of electrons and protons that ensures a very high electric conductivity to the stellar material, such as to bind the electric charge indissolubly to the magnetic lines of force and give the latter (which according to Faraday's electromagnetic model are to be « thought of » as elastic under tension) a concrete existence. The violent movements to which a neutron star is subject excite magnetic oscillations in the lines in such a way that they are propagated along them as along a vibrating cord. According to the three above mentioned authors, at a certain distance from the star there occur conditions such that the above waves, which are called magnetohydrodynamic, are transformed into ordinary electromagnetic waves. These waves after crossing the vacuum created by the very strong gravitational field of the star in their vicinity, arrive at the nebular layers and are partly reflected from these. As a consequence of the reflexion, compression is exerted and it is this compression of the nebular layers that supplies the microscopic model for the acceleration of the particles up to relativistic velocities.

But this model is unacceptable. In fact in the general field of relativity within which the problem of neutron stars must be treated we cannot ignore the gravitational waves, that is the analogy of electromagnetic waves for the gravitational field. Now Zee and Wheeler, and Meltzer and Thorne have shown that the emission of these waves produced by the oscillations of a neutron star would eliminate all the available energy in the course of a few seconds or, on the most favourable hypothesis, in a period of about a day. The mechanism of Hoyle and co-workers is thus absolutely unsuitable for explaining a phenomenon that has lasted for thousands of years and shows no sign of coming to an end.

An alternative was proposed by Pacini (1967) with his oblique rotator model. Pacini assumes that after eliminating the excess thermal energy by means of emission of neutrinos and damping its vibrations by the emission of gravitational waves, the star is reduced to a body rotating round an axis different from the axis of symmetry of the magnetic field. In these conditions it emits a monochromatic electromagnetic wave directly (that is to say without intermediary magnetohydrodynamic wave) with pulsation equal to the angular velocity. This wave may act on the nebular layers by which it is reflected, with the same mechanism foreseen by Hoyle, Harlikar and Wheeler.

Also in 1967 the research team of the Mullard Astronomy Observatory of Cambridge University (England) headed by Hewish and including Bell, Pilkington, Scott and Collins, discovered the first Pulsar. As is usual in science, first one attempts to explain the phenomenon without making ad hoc hypotheses and without introducing new models. But the hypothesis that Pulsars were produced by oscillations in White Dwarfs and other hypotheses soon proved to be unacceptable. An excellent model agreeing with the observed facts was found to be that consisting of a neutron star in rapid rotation.

At this point the history of the Crab Nebula records the discovery of a Pulsar that with its signals reveals the presence of that neutron star which up till then had always been a pure hypothesis. At the present time neutron stars seem to be indissolubly linked with pulsars and their history is associated with that of Pulsars themselves which will be the subject of a future article.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai and G. Righini

Cent'anni dalla nascita di Federigo Enriques

Ricorre nel 1971 il centenario della nascita di Federigo Enriques, che per decenni occupò un posto preminente nella cultura italiana, non soltanto per i suoi studi di geometria, ma anche per la sua opera di filosofo e per la sua battaglia intesa ad affermare l'unità del sapere e del pensiero contro le tendenze specialistiche invalse largamente tra i culturi di discipline scientifiche.

L'Accademia Nazionale dei Lincei e l'Istituto Matematico dell'Università di Milano si sono assunti il compito di promuovere una serie di convegni nel corso dei quali il pensiero e l'opera dell'Enriques verranno riproposti all'attenzione degli uomini di studio attraverso un ampio riesame.

Il primo dei convegni, che avrà carattere internazionale e sarà dedicato alla Geometria algebrica, è stato indetto dall'Istituto Matematico milanese sotto gli auspici dell'Accademia dei Lincei. I lavori si svolgeranno a Milano dal 31 Maggio al 3 Giugno e verranno aperti da un discorso inaugurale del Professor Beniamino Segre, Presidente dell'Accademia dei Lincei, sul tema: « L'opera di Federigo Enriques ». Lo stesso Professor Segre terrà quindi la prima relazione dal titolo « On algebraic varieties as intersections of primals in a projective or affine space ».

Le conferenze sulle quali successivamente si articoleranno i lavori del Convegno sono le seguenti: « Superfici canoniche e pluricanoniche », di E. Bombieri; « Teoremi di massimo nelle Algebre », di E. Vesentini; « Points infiniment voisins », di B. L. Van Der Waerden; « Aritmetica dei corpi ellittici », di E. Kahler; « L'équivalence rationnelle et le moving lemma », di P. Samuel; « The hyperplane sections of Arithmetically Normal Varieties », di A. Seidenberg; « A topological approach to branch and doubles curves of correspondences between algebraic surfaces », di B. Scott; « Sopra la risoluzione delle singolarità delle curve e delle superfici algebriche. Punti infinitamente vicini », di G. Galbura; « Alcuni problemi sulle classi caratteristiche dei fibrati vettoriali algebrici », di A. Lascu; « Le plan de Cremona », di P. Libois; « Sur les surfaces privées de courbe canonique possédant un système canonique irréductible », di L. Godeaux. Altre conferenze verranno tenute dai Professori P. Dubreil, P. Dolbeault, D. Mumford, P. Burniat, I. Safarevic, I. I. Manin.

Fra il 7 e il 12 Ottobre avrà luogo un convegno indetto dall'Accademia dei Lincei e dedicato agli aspetti storici, pedagogici e filosofici dell'illustre studioso. La seduta inaugurale si svolgerà a Pisa, e ad essa interverrà probabilmente il figlio del compianto Maestro, Ing. Giovanni Enriques. I lavori saranno aperti da un discorso del Professor Beniamino Segre, al quale seguiranno le relazioni dei Professori Faedo, Frajese, Szabò e Bulmer-Thomas.

I lavori del convegno proseguiranno a Bologna nei giorni 8 e 9. Nelle due giornate gli intervenuti ascolteranno le relazioni dei Professori Libois, Freudenthal, Campedelli, Dedò, Checcucci, Pescarini, Emma Castelnuovo, Lina Mancini Proia e Villa.

L'ampio riesame dell'opera di Federigo Enriques si concluderà a Roma dove, nei giorni 11 e 12, si svolgeranno le sedute dedicate agli aspetti del pensiero dell'insigne studioso più specificamente riguardanti i rapporti tra filosofia e scienza. Nel corso delle due giornate sono previsti interventi e relazioni dei Professori Lombardo Radice, Somenzi, Spirito, Bernardini, Filiasi Carcano, Kahler e Szabò.

« Scientia », della quale Federigo Enriques fu, nel 1907, tra i fondatori, e poi per decenni l'animatore e la guida, riferirà ampiamente, nei prossimi fascicoli, sullo svolgimento dei convegni. Il pensiero e la figura di Enriques, inoltre, verranno rievocati sulle pagine della Rivista da autorevoli scritti, il primo dei quali, di Lucio Lombardo Radice, apparirà nel prossimo fascicolo. « Scientia » ritiene di assolvere, in tal modo, ad un preciso obbligo nei confronti del filosofo cui fu legata da così stretti vincoli, ed anche di offrire ai suoi lettori un ampio esame di problemi la cui attualità si va limpidamente manifestando ogni giorno.

A Hundred Years after the Birth of Federigo Enriques

The year 1971 marks the centenary of the birth of Federigo Enriques, who for several decades occupied a pre-eminent position in Italian culture, not only on account of his studies in geometry but also on account of his work as a philosopher and his fight to uphold the unity of knowledge and thought against the specialistic tendencies prevailing widely among the cultured exponents of scientific disciplines.

The National Academy of the Lincei and the Mathematical Institute of Milan University have undertaken to promote a series of meetings, in the course of which Enriques' work and ideas will be presented once more to the attention of scholars through a full reassessment.

The first of the meetings, which will be of an international nature and will be devoted to algebraic geometry, has been organised by the Mathematical Institute in Milan under the auspices of the Lincei Academy. The meeting

will be held in Milan from May 31st to June 3rd and will open with an inaugural speech by Prof. Beniamino Segre, President of the Lincei Academy, on the theme: « The work of Federigo Enriques ». Prof. Segre will then read the first paper, entitled « On algebraic varieties as intersections of primals in a projective or affine space ».

The other papers to be read at the Meeting are as follows: « Superfici canoniche e pluricanoniche » (Canonic and pluri-canonic surfaces) by E. Bombieri; « Teoremi di massimo nelle Algebre » (Maximum theories in Algebra) by E. Vesentini; « Points infiniment voisins » (Infinitely close points) by B. L. Van Der Waerden; « Aritmetica dei corpi ellittici » (The Arithmetic of elliptical bodies) by E. Kahler; «L'équivalence rationnelle et le moving lemma» (Rational equivalence and the moving lemma) by P. Samuel; « The hyperplane sections of Arithmetically Normal Varieties », by A. Seidenberg; « A topological approach to branch and doubles curves of correspondences between algebraic surfaces », by B. Scott; « Sopra la risoluzione delle singolarità delle curve e delle superfici algebriche. Punti infinitamente vicini » (On the resolution of the singularities of curves and algebraic surfaces. Infinitely close points) by G. Galbura; « Alcuni problemi sulle classi caratteristiche dei fibrati vettoriali algebrici » (Some problems on the characteristic classes of algebraic vectorial fibrates) by A. Lascu; « Le plan de Cremona » (The Cremona plan) by P. Libois; « Sur les surfaces privées de courbe canonique possédant un système canonique irréductible » (On surfaces lacking a canonic curve possessing an irreducible canonic system) by L. Godeaux. Papers will also be read by Profs. P. Dubreil, P. Dolbeault, D. Mumford, P. Burniat, I. Safarevic and I. I. Manin.

Another Meeting, to be held on October 7th-12th and organised by the Lincei Academy, will be devoted to the historical, educational and philosophical aspects of the work of the eminent scholar. The inaugural session will take place in Pisa and will probably be attended by the son of the late Master, Ing. Giovanni Enriques. The works will be preceded by an opening speech by Prof. Beniamino Segre, which will be followed by papers given by Profs. Faedo, Frajese, Szabò and Bulmer-Thomas.

The activities of the Meeting will be continued in Bologna on October 8th and 9th. On those two days, those present will hear papers read by Profs. Libois, Freudenthal, Campedelli, Dedò, Checcucci, Pescarini, Emma Castelnuovo, Lina Mancini Proia and Villa.

This full survey of the work of Federigo Enriques will be concluded in Rome, where, on October 11th and 12th, the sessions will be devoted to those aspects of the thought of the eminent scholar that more specifically concern the relationships between philosophy and science. In the course of the two days, it is expected that contributions and reports will be presented by Profs. Lombardo Radice, Somenzi, Spirito, Bernardini, Filiasi Carcano, Kahler and Szabò.

Federigo Enriques was, in 1907, one of the founders of « Scientia » and later, for some decades, was its guide and leading spirit. The Review will therefore give full reports on the activities of the meetings, in the forthcoming numbers. In addition, the thought and personality of Enriques will be recalled in the pages of the Review in authoritative articles, the first of which, by Lucio Lombardo Radice, will appear in the next number. In this way, « Scientia » aims to fulfil a definite obligation towards the philosopher to whom it was bound by such close ties, and also to offer its readers a full survey of problems that are daily assuming greater current interest and importance.

Utilità e pericoli della volgarizzazione

Gli ultimi decenni hanno visto aumentare, con rapidità crescente, la complessità delle conoscenze scientifiche, mentre - grazie anche ai progressi della tecnologia - i risultati della ricerca andavano esercitando una influenza sempre maggiore sulla vita della società, sulla stessa esistenza degli uomini. Se questa influenza si è esplicata talvolta come forza liberatrice (dal bisogno, dalla fame, dalla malattia), in qualche caso è parsa assumere un carattere minaccioso: valga l'esempio delle armi nucleari o degli strumenti bellici chimici o batteriologici. Un solco sempre più profondo è andato così dividendo gli operatori della ricerca scientifica, depositari di un patrimonio di nozioni precluso ai più, ma anche di ampie capacità operative, cioè di potere, e il grande pubblico, la gente comune. Di qui l'esigenza di diffondere le conoscenze scientifiche, con il massimo rigore possibile, fra le grandi masse di una società che dalla scienza è in così larga misura condizionata.

Non può perciò non essere considerata sommamente opportuna l'iniziativa assunta recentemente dal Consiglio d'Europa, che ha promosso un incontro di esperti per l'esame dei problemi connessi alla volgarizzazione scientifica (Strasburgo, 19-21 Aprile). Volgarizzazione è, ancor oggi, parola che può indurre a sospettare intenti paternalistici, e che spesso è servita a mascherare grossolane approssimazioni non sempre disinteressate. Non sembra che queste accuse possano essere rivolte ai promotori ed ai partecipanti al convegno.

« La volgarizzazione scientifica - scrivono nella loro relazione due di questi ultimi, Renaud Dulong e Werner Ackermann, del laboratorio di psicologia sociale di Parigi - sembra poter offrire all'uomo, qualunque sia il suo livello socio-culturale, strumenti che gli consentano di giudicare consapevolmente ciò che accade intorno a lui. Volgarizzare significherebbe dunque mettere a disposizione di tutti ciò che è stato concepito ed elaborato da pochi. Il problema è, in realtà, più complesso: la verità rivelata dal volgarizzatore colto non è la verità scientifica obiettiva, ma una verità d'opinione. Confrontando la volgarizzazione all'insegnamento, si potrebbe dire che quest'ultimo è caratterizzato da un movimento dell'uomo verso la scienza, da uno sforzo di oggettività, mentre la volgarizzazione si presenta come un movimento della scienza verso l'uomo, con tutto ciò che questo comporta di soggettività. I temi che costituiscono argomento di volgarizzazione sono problemi preesistenti nello spirito dei lettori, ed ai quali la scienza potrebbe porgere soluzioni. Si tratta dei rapporti dell'uomo con le sue origini e con la natura, dell'origine dell'universo, dei rapporti dell'uomo con i suoi simili e con il suo ambiente culturale, ed infine dei suoi rapporti con la morte e con la fine dell'universo. Sono proprio questi rapporti che vengono continuamente rimessi in discussione dalle grandi scoperte scientifiche e che già furono definiti in un modo relativamente stabile dai miti e dalle religioni. Al giorno d'oggi le soluzioni mitiche o religiose si trovano spesso in conflitto con quelle scientifiche. Poste queste premesse, diventa illusorio credere che la volgarizzazione possa trasmettere conoscenze obiettive. Per il suo lettore, essa deve contribuire alla soluzione di problemi che egli si pone a livello della sua vita quotidiana più immediata, ossia di problemi profondamente soggettivi ».

In realtà lo stesso riconoscimento dei pericoli e dei limiti cui è soggetta l'informazione scientifica nei confronti delle grandi masse può, già di per sè, costituire un credibile punto di partenza per un processo (necessariamente non facile, nè di breve durata) inteso a colmare il fossato che oggi divide la comunità degli studiosi dalla ordinaria società: tanto dovrà essere fatto, evidentemente, non sulla base di una aristocratica buona volontà, ma sotto la spinta di una matura coscienza civile che senta come indebito ogni squilibrio, ogni discriminazione fra gli uomini.

The Usefulness and Dangers of Popularisation

During the last few decades the complexity of scientific knowledge has been increasing with growing rapidity, while - owing also to the progress in technology - the results of research have been exercising an ever greater influence upon the life of society and upon man's very existence. If, at times, this influence has appeared in the form of a liberating force (freeing men from need, hunger and disease), in some cases it has seemed to assume a threatening aspect: there is the example of nuclear weapons or instruments for chemical of bacteriological warfare. Thus there has been an ever-widening gap dividing those working on scientific research - the trustees of a heritage of notions that is precluded to the majority, but also of considerable capacities of putting them into effect (i.e. power) - and the great public, the common people. Hence the need for the diffusion of scientific knowledge, under the strictest possible control, among the great masses of a society that is conditioned by science to such a large extent.

For this reason, the recent enterprise of the Council of Europe, in promoting a meeting of experts for a study of the problems connected with the popularisation of science (Strasbourg, April 19th-21st), cannot be considered as other than highly opportune. Even today, popularisation is a word that may prompt a suspicion of patronising intentions and has often served to disguise

rough approximations that are not always disinterested. It does not appear that these accusations can be levelled at the promoters of the meeting and at those taking part in it.

As stated in the paper presented by two of the latter, Renaud Dulong and Werner Ackermann, of the laboratory of social psychology in Paris: « The popularisation of science would seem capable of providing man, whatever may be his socio-cultural level, with instruments that enable him to make a conscious assessment of all that is going on around him. Popularisation would therefore mean placing, at the disposal of all, what has been conceived and elaborated by the few. In reality, the problem is more complex: the truth revealed by the cultivated populariser is not objective scientific truth but a truth of opinion. Comparing popularisation with teaching, it might be said that the latter is characterised by a movement of man towards science, by an effort of objectivity, whereas the former appears as a movement of science towards man, with all the subjectivity that this involves. The themes constituting subjects of popularisation are problems pre-existing in the mind of the readers, to which science could offer solutions. They are concerned with man's relationships with his origins and with nature, the origin of the universe, man's relations with his fellow-men and with his cultural environment, and finally his relationships with death and the end of the universe. It is these very relationships, which were formerly defined in a relatively stable manner by myths and religions, that are continually being rendered controversial by the great scientific discoveries. Today, the mythical or religious solutions often conflict with the scientific solutions. With these premises, it is a mere illusion to think that popularisation can impart objective knowledge. For its reader, it must contribute to the solution of problems that present themselves to him at the level of his more immediate daily life: that is, profoundly subjective problems ».

In reality, the very recognition of the dangers and limitations to which scientific information is subject, with regard to the great masses, may in itself constitute a credible starting-point for a process (which will necessarily be neither easy nor short) aimed at bridging the gulf that today divides the community of experts from ordinary society. Obviously this will have to be carried out not on the basis of an aristocratic goodwill but prompted by a mature civil conscience that feels that there should be no inequality or discrimination among men.

Un censimento dei ricercatori

Un primo gruppo di tredici censimenti riguardanti altrettanti settori di ricerca biomedica sono stati promossi, in Italia, dal Consiglio Nazionale delle Ricerche attraverso il proprio laboratorio di studi sulla ricerca e la documentazione. I settori sottoposti a questa indagine sono: sistema nervoso centrale; utilizzazione di elaboratori elettronici per ricerche non di carattere statistico; tumori; sistema cardiovascolare; processi infiammatori; metabolismo dei farmaci; sistema neurovegetativo; farmacologia clinica; processi degenerativi; immunologia; trapianti d'organo; biologia molecolare; utilizzazione di metodi gascrimatografici.

La realizzazione dei censimenti è stata affidata all'Istituto di ricerche farmacologiche « M. Negri » di Milano, una istituzione indipendente che da tempo conduce ricerche scientifiche.

I censimenti disposti dal C.N.R. serviranno a mettere a disposizione di tutti gli interessati, e in particolare dei ricercatori, informazioni di carattere pratico oggi non sempre facilmente reperibili, ed in particolare: 1) l'elenco dei ricercatori italiani suddivisi per campi di attività (elenco alfabetico ed elenco analitico per settore); 2) l'elenco dei laboratori italiani, con dettagliate notizie sulle attrezzature e sulle metodologie (elenco alfabetico e ripartizione geografica); 3) la bibliografia delle recenti pubblicazioni degli autori italiani censiti dal 1967 (elenco alfabetico ed analitico).

L'utilità pratica di questa iniziativa è evidente, come sa chiunque, operando nel campo della ricerca biomedica, abbia sperimentato l'insufficienza delle informazioni che talvolta si oppone ad un reale coordinamento dell'attività svolta nel settore. Valga d'esempio un episodio per più di un verso paradossale: un precedente censimento effettuato nel 1968 dallo stesso Istituto « Negri » e riguardante i ricercatori che utilizzano metodi di coltura dei tessuti, portò a scoprire che alcuni laboratori italiani si rivolgevano regolarmente ad istituti americani per farsi inviare ceppi di cellule tumorali, ignorando che essi erano, in certi casi sin da parecchi anni, coltivati e disponibili presso altri laboratori nazionali operanti, in certi casi, a non più di qualche centinaio di metri dai richiedenti.

È un esempio, fra i molti che si potrebbero citare, della mancanza di conoscenza reciproca verificabile fra i ricercatori italiani, ed alla quale l'iniziativa del C.N.R. e dell'Istituto « Negri » intende porre rimedio.

A Census of Research Workers

An initial group of thirteen censuses regarding as many sectors of biomedical research have been promoted, in Italy, by the C.N.R. - National Research Council - through its own laboratory for studies on research and documentation. The sectors included in this extensive study are: the central nervous system; the use of electronic computers for research of a non-statistical nature; tumours; the cardio-vascular system; inflammatory processes; the metabolism of drugs; the neuro-vegetative system; clinical pharmacology; degenerative processes; immunology; organ transplants; molecular biology; the use of gas-chromatographic methods.

The task of carrying out the censuses has been entrusted to the « M. Negri » Institute of Pharmacological Research in Milan, an independent institution that has for some time been engaged upon scientific research.

The C.N.R. censuses will serve to provide all concerned, and particularly research workers, with information of a practical nature that is not always readily obtainable today, and in particular: 1) a list of Italian research workers classified according to fields of activity (an alphabetical list and an analytical list by sectors); 2) a list of Italian laboratories, with detailed information on the equipment and methods employed (alphabetical list and geographical distribution); 3) a bibliography of the recent publications by Italian authors listed since 1967 (alphabetical and analytical lists).

The practical usefulness of this enterprise will be obvious to anyone working in the field of biomedical research that has experienced the shortage of information that sometimes prevents a real co-ordination of the activity carried out in the sector. As an example of this, mention may be made of an episode that is paradoxical in many senses: a previous census taken in 1968 by this « Negri » Institute, devoted to research workers that use tissue culture methods, led to the discovery that certain Italian laboratories were regularly applying to American institutions to have strains of tumour cells sent to them, ignorant of the fact that these strains had been cultivated - in some cases for many years - and were available at other Italian laboratories operating, in certain cases, not more than a few hundred yards away from them.

This is one example, among the many that could be quoted, of the lack of reciprocal knowledge that is to be found among Italian research workers, and it is this lack that the C.N.R. and the « Negri » Institute intend to remedy by means of this enterprise.

Un convegno sull'inquinamento marino

Dal 4 al 7 Ottobre si svolgerà a Messina il 5° Colloquio Internazionale di Oceanografia Medica organizzato dall'Istituto di Idrobiologia della Facoltà di Scienze, sotto l'egida della Mediterranean Association of Marine Biology and Oceanography. Presidenti del Colloquio saranno il Prof. J. C. Senez, professore di microbiologia presso la Facoltà di Scienze di Marsiglia, il Prof. A. Giovanardi, direttore dell'Istituto di Igiene dell'Università di Milano, e il Dott. M. Aubert, coordinatore del gruppo di lavoro di Oceanografia medica della M.A.M.B.O.; il segretario generale è il Prof. Sebastiano Genovese, direttore dell'Istitituto di Idrobiologia della Facoltà di Scienze di Messina.

I temi del colloquio sono strettamente connessi ad alcuni problemi che negli ultimi tempi si sono manifestati con drammatica gravità: le conseguenze biologiche dell'inquinamento marino; le fonti di inquinamento delle acque e i meccanismi di autodepurazione; l'inquinamento marino nei suoi riflessi sulla salute pubblica.

A Meeting on the Problem of Marine Pollution

From October 4th to 7th, there is to be held at Messina the Fifth International Meeting of Medical Oceanography, organised by the Institute of Hydrobiology of the Faculty of Sciences, under the auspices of the Mediterranean Association of Marine Biology and Oceanography. The Meeting will be held under the Chairmanship of Prof. J. C. Senez, professor of microbiology at the Faculty of Sciences in Marseilles, Prof. A. Giovanardi, Director of the Institute of Hygiene at Milan University, and Dr. M. Aubert, co-ordinator of the M.A.M.B.O. working group on medical oceanography. The secretary general is Prof. Sebastiano Genovese, Director of the Institute of Hydrobiology of the Faculty of Sciences in Messina.

The themes of the Meeting are closely related to certain problems that have arisen with dramatic seriousness in recent years: the biological consequences of marine pollution; the sources of pollution of waters and the mechanisms of self-purification; marine pollution and its effects upon public health.

The First Symposium of Cybernetics and Pedagogy

On the occasion of the International Year of Education promoted by UNESCO, an important initiative has been taken by the St. Marino Republic's Institute of Cybernetics Research. The first Symposium on Cybernetics and Education, supported by the Italian National Committee of UNESCO, took place in The Conference Hall of St. Marino.

Luigi Volpicelli acted for the UNESCO Committee and was chairman of the coordinating committee. In his opening address he pointed out authoritatively the aims of UNESCO when naming the year 1970 « International Year of Education ».

For Silvio Ceccato the need to introduce Cybernetics into the field of Education was mainly justified by a shortage of teachers and an increasing growth rate of knowledge. Cybernetics was able to tackle these problems in various ways: with the programmed learning machines; with the study of the balanced relationship between teachers and pupils; with the introduction of cybernetic machines in the humanistic research fields; and with the construction of models of mental processes.

Ceccato laid most emphasis on the last approach, and was doubtful about the value of the other, because the ability to construct models of mental processes could modify the very nature of teaching. This point was shown by giving a lesson on cardinal and ordinal numbers according to the old and the new method. To make up for the lack of teachers, he suggested larger investment in audio-visual aids.

For Luigi Meschieri the use of cybernetic instruments and mechanical teaching aids was dependent on the attitude of the teacher, for he can use them in many different ways, or not use them at all. Even when familiar with them, the attitude of the teacher towards new educational techniques might vary between attraction and repulsion. On the one hand he might find the pleasure of handling modern instruments, more sophisticated then the traditional methods of teaching; on the other hand he might feel a reverential fear or an unconscious rivalry for the machine. For example the machines' efficiency could show up a serious deficiency in the teachers' knowledge and so undermine their authority with the pupils. This conflict, possibly unconscious in part, would tend to create resistance to the introduction of cybernetic techniques into the classroom.

Aureliano Casali dealt with various problems of communication, particularly with those which control reception and production of speech. Thus if speech recorded from a good Italian speaker was filtered through a low-pass filter, there came a point when a foreigner, even with a good knowledge of our language, could not understand it. The same tape recording, however, played back to an Italian (or a person with Italian as mother language) was easily understood. As a matter of fact the Italian would also help himself with his innate knowledge of word production in interpreting the filtered speech.

The same author reported another experiment, relating to learning theory; the transmission of meaningful signals by the visual pathway and of interfering signals by the auditory one. The main conclusion of this work for teaching were: during the learning processes interfering stimuli should be kept to the minimum; under proper conditions, two different pathways (e.g. visual and auditory) could be conveniently used to carry the same information; the phonological features of the teachers' speech should be as similar as possible to those of the pupils.

Leonardo Trisciuzzi suggested that in learning the important feedback could be defined as a self-correcting process which operates by rejecting the many « wrong » alternatives of the « right » answer. This includes: (a) the negative feedback as a device for re-shaping of the cognitive field and for automatic control; (b) the positive feedback as a device to stress the results of the negative feedback and to alter the value of the input (both qualitatively and quantitatively); (c) appreciating the difference between the actual result and the intended result. The last point seemed the most important for future investigations on this feedback mechanism as a framework for teaching.

For Enrico Maretti from the interaction between algorithms of the computers and Physiology one should expect not only an improvement of the research work but a criticism of the procedures to analyse philological material (the « text »). This critical contribution pointed out that the teaching component could control the analytical procedures, which must be realized in complete and not contradictory terms, but also as a separation between the algoritmic and the creative aspects.

Moreover the further reinforcement of the didactic or corrective action that may be found in the examination of the procedures by entirely interactive means. Among the most significant examples of this situation, he mentions the textual criticism and the typological arguments in the classification of papyrological texts.

Giacomo Cives outlined two apparently contradictory directions in contemporary educational theory: a trend towards rationalization and formalism on the one hand (as shown by the use of cybernetic models and teaching machines) and a trend towards a valuation of the whole individual including his emotional aspects, on the other. A balanced evaluation of both trends should open the way to a practical use of disciplines like Cybernetics, Information Theory, in connection with education, and stimulate the school to avail itself of Sciences and Technology. Fields where the merging between Cybernetics and Teaching was already being carried on were: the development of teaching machines; the use of cybernetic processes, the study of the codes in the transmission and reception of information, and the study of selective information filters (as a defence against redundancy and noise).

The interaction between Cybernetics and Teaching would help to eliminate rhetoric on the part of the teacher, would tend to make teaching more rational, and would promote interdisciplinary approaches.

Filippo Greco, reporting his experience with teaching machines, elucidated their utility when rapid learning is required.

Quoting examples from flight-simulators, he compared the limitations of human instructors and of machines.

Ugo Barbano hinted at the distrust of the teachers for Cybernetics, and explained that although we could view the school as one of the most slowly moving institutions, we could attribute this both to mental laziness of men and to the sense of educational responsibility. Man is what he does for what he knows for the way he learnt. In the age of technology and of dominating mass-media, both of which tend to produce alienation and conditioning, we hoped that education would lead to the ability to resist these trends. Otherwise the concepts of free will, which resulted from reflection of logical judgement, become endangered. Cybernetics was most useful as regards the transmission of information; through it the pupil could come into touch with the technical world and become familiar with it. Cybernetics would help one to understand cognitive processes or « schematismus latens » of thinking.

For Luciana Fontana the problem of communication constituted one of the leading themes in the general controversy over the needs of the modern school. Ordinary teaching methods were inadequate since they failed to offer to the individual students the necessary means to control the physical world as it was shaped by up-to-date technology. This necessitated a renewal of both content and method in teaching. Given the difficulty to predict what knowledge and ability might be necessary for the person concerned, the school should offer everyone the basic instrumental abilities, i.e. teaching people *how* to learn in relation to the individuals' cognitive processes. It was therefore necessary for the school to organize a range of general methods, the principles of which could be applied to a particular education programme which, as far as could be seen, was fitted to that individual's future. Among the new proposals for a permanent process of self-learning, programmed teaching constituted a sound example of a technology of teaching, stemming from interdisciplinary contributions of other sciences.

Giuseppe Lisciani commented upon the hypothesis that Cybernetics can contribute to a scientific theory of teaching. The relationship between Cybernetics and Education is not the same as that between Education and Sociology or Psychology or that between Education and Chemistry and Physics. According to Bruner four points lead towards a teaching as a science:

1. individual set;
2. structure of knowledge;
3. pattern of learning;
4. consequences.

Further points are: input behaviour, teaching targets, educational procedures, evaluation of the outcome.

However a full use of education processes requires a careful consideration of the two styles of presentation, inferential and declarative. The system of the education is a set of interacting systems, whose behaviour can be better understood and controlled by Cybernetics. Feedback mechanisms must therefore be discussed in political terms, since we have to know what are the aims of our actual behaviour.

Repubblica di San Marino, Centro di Ricerche di Cibernetica.

A. Casali

RECENSIONI - BOOK REVIEWS

L. Geymonat, *Storia del pensiero filosofico e scientifico*, Garzanti Editore, Milano, 1970/1971, Vol. I: *L'antichità. Il Medioevo*, 736 pp., figs., Vol. II: *Il Cinquecento. Il Seicento*, 706 pp., figs., Vol. III: *Il Settecento*, 720 pp., figs.

Italian culture has for at least two centuries strictly excluded a sense of reality from its outlook and reduced its operations almost to liturgical acts, to a game amongst experts.

Why this occurred, and whose interests this privileged isolation served, need not to be discussed here. It is, however, interesting to remember the deadening consequences of such a state of affairs - the essential immobility, the progressive impoverishment of content, hardly hidden by subtle lessical inventions, the inevitable unproductiveness resulting from mulling over the same subjects again and again with an inexhaustible subtlety.

Within that culture, Philosophy was expected to sharpen the tools of the game and hold together in some way the walls of the ivory tower.

For that reason the school - in a country where analfabetism cut off most of the population from the chances of a fully aware civil participation - was, and partly still is, the place where talkers, critics, dialectitians, students of stylish speech and literature were bred.

To put together in a single treatise, as Ludovico Geymonat does, the history of philosophical and scientific thought - showing that there are no grounds for keeping them apart on either side of a boundary; or, even worse, for attributing them to different levels of cultural value - is equivalent to driving culture out of its protected corners and conferring on it a stature which is greater than that of its formal cloaks.

To those who have freed themselves from the shell of prejudices formed over the centuries, Geymonat's arguments may appear only too natural: « Philosophical thought and scientific thought are not in any sense antithetical but they are two faces of the same rationality which is laboriously gaining ground in the history of man. The « vision of the world » which philosophers and scientists construct, revise and construct again are never wholly satisfactory, nor ultimate, or complete.

« However they are truly rational just because they are not ultimate; they belong to an open minded process which refuses to end in dogma or indisputable truth. These visions are built up on the basis of specific arguments, and they are stimulating even for those who may want to criticize them on the basis of other arguments. Their rôle is to stimulate the progress of Reason, to open new perspectives to her, and to make her both more cautious and brave. In this articulated collaboration no issue is solely the responsibility of either the philosopher or the scientist ».

Thus it constitutes something of a scandal that this simple statement, supported by a rigorous and conscious adherence to reality, should amount to an article of all-out struggle. All the other merits of Geymonat's opus (and they are of no mean order) descend from this opening statement —and also from the author's qualifications both as a lucid, wide-awake philosopher and as a man of science with a healthy respect for methodological strictness. Nor should we omit to mention, among others, the merit of having clarified the necessary connections between each stage of the development of thinking and the socio-economic circumstances in which the same stage came into being; or of having recognized the importance of educational problems —not only because such problems are themselves of an intellectual nature, but also because in all times the function of the school has been to bridge the gap between culture and the society that produces it.

The reader of these pages, in which the author never tries to hide behind a screen of improbable or impossible neutrality, may note that the demarcation lines that are drawn across every historic era are essentially always the same, in spite of countless accessory differences: because essentially the same is the relationship between men facing one another across those fundamental fences, no matter what the instruments with which they try to overthrow others or they strive not to be overthrown by others. So there is practically no chapter that does not confront us in some guise or another with the themes that have always been, and still are, at the center of any vital debate.

From our reading of the first three volumes (out of the six that will constitute Geymonat's opus) we must remark that the language itself is far more linear and terse than anything we have grown accustomed to find in books of philosophy and science—not a trifling fact in a country were many have hoped for, and some have actually achieved, reputation on the strength of obscurity in language. And last, we must mention that for the writing of specific portions of his subject matter Geymonat has enlisted the help of other scholars akin to himself. The fact that the presence of more than one voice does not disrupt the unity of treatment must be accepted as further proof of methodological rigorousness—in this case evinced by the remarkable coherence of the various parts.

A. De Murtas

Z. A. Medvedev, *The Rise and Fall of T. D. Lysenko*, Columbia University Press, London, 1969, xx+284 pp., figs., 90s.

The main episodes of the adventure of Trofim D. Lysenko in USSR are fairly well known to geneticists of the world outside the Soviet Union. Information, albeit incomplete, has filtered through the iron curtain in several circumstances. The verbatim proceedings of the famous session of August 1948 of the Lenin Academy of Agricultural Sciences, which marked the official establishment, sponsored directly by Stalin, of Michurinism as the only legitimate theory of heredity and of its practical applications, were given the widest circulation through the world in authorized translations in several languages. The geneticists thus learned to their utmost astonishment that the science of genetics as had logically developed on solid experimental ground through the work of a great number of first rank scientists, was a reactionary view, a rotten product of a bourgeois idealistic philosophy. Mendelism, Morganism, Weismannism - as classical genetics was labeled by Lysenko and his supporters - was nothing else than metaphysics, guilty of having produced anti-Darwinism, racism, anti-Marxism, in complicity with capitalistic imperialism. It was an improductive way of looking at biological events, responsible for keeping down agricultural production, to the sole advantage of the capitalists, the enemies of the workers. Geneticists were amazed in learning that a sort of pastiche of old theories clearly disproved by a long accurate experimentation, such as the most naïf lamarckism, was the only true science of heredity, fulfilling the imperative of the marxistic creed and of the sound materialistic philosophy. This confuse, contradictory, experimentally undemostrated view of the phenomena of biological heredity was presented by Lysenko as the elaboration of the obsolete tenets of I. V. Michurin, a Russian plant breeder of the old generation (he died in 1936). In name of this « theory », with the blessing of the supreme political authorities (Stalin first, and subsequently Khruschev), Lysenko, from the year 1936 onwards, had succeded gradually in seizing the power in many branches of the biological sciences. The great geneticist N. I. Vavilov, who introduced in the USSR the theory and the methods of classical genetics and organized a great number of interconnected experimental stations, and was made President of the Lenin Academy of Agricultural Sciences, was arrested, as a consequence of the slanderous attacks by Lysenko and died in prison in 1943. Other geneticists and general biologists who dared to oppose to the follies of Lysenkoism the voice of the sound science, i.e. of the science based on its own principles and experimental evidence and not inspired by any philosophical or pseudophilosophical creed and by the ignorance of politicians, were accused, persecuted, dismissed from their posts, which were given to Lysenko himself or to his supporters.

The rise of Lysenko started in the thirties and lasted about 30 years. It was only after Khrushchev's dismissal of the post of Chairman of the Council of Ministers and First Secretary of the Central Executive Committee (October 14, 1964) that Lysenko's decline began. It was then possible to the surviving biologists of the Vavilov's school and to those of the new generation who had the opportunity to study thoroughly the situation and to make their own judgment, to demonstrate not only the absurdity of Lysenko's tenets, the falsity of his claims (e.g. to be able to transform a biological species into another, wheat into rye, rye into oat, etc.), but also the absolute inconsistence of his methods for improving production, or for creating new varieties of crops or cattle. Nowadays genetics, the real science of genetics, is emerging again in USSR, under the guidance of some valiant biologists, whose activity in this field was completely inhibited during the Lysenko's dark age.

Most of these events were more or less known to the scientific world of the western countries, and much has been written thereabout. Of course all people who had enough biological culture to grasp the essence of the alledged theory, and sufficient freedom of thought (i.e. the great majority of scientists operating in the western countries) felt very deeply about this scornful attack to scientific freedom, occurring in the 20th Century. But several aspects of this sad story remained obscure, many details unknown. To many people not knowing exactly the political background it was hard to understand how it did happen, difficult to evaluate the strenght of the opposing trends and the reasons for the victory of the false one.

Here comes a book which provides an answer to all the questions, giving a very detailed history of the events, with a first hand documentation. The author, a young biologist working at the Laboratory of Medical Radiology in Obninsk, near Moskow, (recently the newspapers announced that he has been dismissed) started gathering data and personal recollections in 1962, while Lysenko was still in the height of his power. His work was continued in 1963-64 and completed in 1965-67. The manuscript was widely circulated among USSR scientists in order to get further details, correction, complements. Sometimes in 1965 M. I. Lerner, professor of Genetics at the University of California, Berkeley, came in contact with the author, and volunteered to translate his work when and if it was published in the USSR. In 1967 Medvedev informed Lerner that publication was to be delayed, because the authorities had decided that the year 1967 being the fifthieth anniversary of the Revolution was not a suitable time to bring out books critical of the Soviet regime. Subsequently Lerner came into possession, through unofficial channels, of a microfilm of the typescript, and decided to translate and publish it. « The decision to do so was not an easy one. (*quote from Lerner's Foreword*). Regardless of the route by which the manuscript reached me, the possibility of reprisals against its author exists. It appears, however, that Medvedev is willing, in the light of his circulation of the manuscript in the Soviet Union at the height of Lysenko's last surge of power (chapter 9) to take whathever risks are involved in publishing the book here, for the good of his country ..... Full exposure of what happened in the course of Lysenko's rise and fall can do nothing but good for Soviet science and the Soviet economy. Indeed, as Medvedev attributes patriotic motives to Vavilov, Pryanishnikov, and others, so it is obvious that his own manuscript was written out of concern for the welfare of science and the national economy of his fatherland ».

« To me (*it is still Lerner speaking*) chapter 6 is one of the highlights of the book. It presents the terrifying picture of the methods of the Lysenko takeover and the consequences of it. The courageous story of the struggle waged by Vavilov, Pryanishnikov, Tulaikov, Sabinin and others in the earlier phases and by Medvedev himself, Sukachev, and still others in the last stages, represents as glorious a chapter in defense of scientific freedom and human dignity as can be found in the annals of civilization. But the events of 1948-1952 are portrayed in so vivid a manner that the specter of Orwell's 1984 hauting our society becomes convincingly real ».

Biologists, scientists in general and all those who are concerned with freedom of thought and progress of science should read this book. The sorrow, the awe that this frightful story will raise in their souls may be relieved by the testimony of the courageous attitude of several scientists who fought bravely against Lysenko's forgery. Much praise deserves especially Medvedev's courage and endurance.

Roma, Università, Istituto di Genetica.

G. Montalenti

G. W. Stocking, Jr, *Race, Culture and Evolution* (Essays in the History of Anthropology), Collier-Macmillan Limited, London, 1968, xx+380 pp., 95s.

This is not a complete history of Anthropology and of the concept of race, but a collection of very thourough and exhaustive essays on some aspects of the main subject. Each of the eleven chapters is in some way an independent essay (some of them have been previously published, some are inedit) which is introduced by a comment of the author. The titles, which give an idea of the contents, are the followings: On the limits of « presertism » and « historicism » in the historiography of the behavioral sciences; French anthropology in 1800; The persistence of polygenist thought in post-darwinian anthropology; Matthew Arnold, E. B. Taylor and the uses of invention; « Cultural darwinism » and « philosophical idealism » in E. B. Taylor; The dark-skinned savage: the image of primitive man in evolutionary anthropology; From physics to etnology (being the story of the explorations and of the ideas of Franz Boas; *n.of the r.*); The critique of racial formation; Franz Boas and the culture concept in historical perspective; Lamarckism in American social science, 1890-1915; The scientific reaction against cultural anthropology (1917-1920).

Particularly interesting is the last chapter, which recounts the dramatic events by which Boas, who had been a dominating personality in American anthropological science, was censured in 1919 at the Cambridge meeting of the American Anthropological Association and stripped of his membership in the Association's governing council. This was an episode of the fight between physical anthropology and Boas' cultural anthropology, which, in turn, is an expression of the two attitudes toward human races: physically and mentally different, or substantially equal. Eventually Boas resumed his leading position, and « it was Boasian anthropology, rather then the racialist writers associated with the eugenics movement, which was able to speak to Americans as the voice of science on all matters of race, culture and evolution: a fact whose significance for the recent history of the United States doubtless merits further exploration ».

Although not complete, and mainly centered on the history of American cultural versus physical anthropology, this book (perhaps exceedingly analytical for the non specialists) is very instructive. The last sentence quoted above opens the hope to see in the next future the continuation of this study.

Roma, Università, Istituto di Genetica.

G. Montalenti

D. K. Pickens, *Eugenics and the Progressives*, Vanderbilt University Press, Nashville, 1968, xii+260 pp., $7.95.

Picken's book, dealing with the history of the eugenic movement in USA is in some way complementary to Stocking's book. Here again, the conflict between a naturalistic conception of man and society and the belief that something else characterises human existence, namely civilization, is the main theme. The author investigates how the galtonian creed was interwoven with philosophical, social and racial implications. Right in his introduction he presents the tension between the anthropologist A. L. Kroeber (author of «The superorganic », 1917) and Galton's views, noting that « this intellectual tension ....is the crux of modern's man dilemma: what is animal and what is cultural in human nature? ». In the following pages the rise of eugenics is expounded and analysed in all his connexions, and finally the decline of galtonian eugenics in the United States and its causes are considered. Bibliographical references are abundant, but not easy to trace, because they are not indexed. There are a few misprints and a systematic mistake: the geneticist Herman J. Muller being constantly referred to as Herbert J. Muller.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

I. M. LERNER, *Heredity, Evolution and Society*, W. H. Freeman and Company, Berkshire, 1968, XVI+308 pp., figs., 68s.

One leit motif of the objection of the students to the teaching systems adopted in most of our schools up to university is that courses are too much fact-laden and isolated, as it were, from the wider context of cultural and social implications of science. According to the author of this book, even the courses on the natural sciences which the students in the humanities are due to attend suffer the same limitations. They are introductory courses in physics, chemistry, and biology more suited for students intending to pursue these subjects in greater detail later.

In the view of the author « a different approach to the biological education of a non scientist is required. It is not simply the presentation of facts that is objectionable; it is difficult (sometimes impossible) to explain principles without resort to facts because abstractions without illustrations have no substance. The difficulty lies more in the selection of facts to be presented ». The criteria adopted by the author in selecting facts and presenting the matter are discussed in the preface. The following abstract of the table of contents may give an idea thereabout. The Introduction begins with a statement on the concept of organic evolution as the most important biological generalization in the intellectual history of man; an explaination of what is genetics follows. Chapter 2 deals with Life (properties of living matter, levels of organization, position of man in nature, origin of life). Chapter 3 is devoted to Darwinism; chap. 4 to the Evolutionary record. The subject of chap. 5 is Man (origin, cultural evolution, population problems). In the following chapters the main facts of genetics, including the recent acquisitions on cell metabolism are exposed at some lengtht: 6. Information machinery of the cell; 7. Biological communication between generations; 8. Sex; 9. Gene interaction; 10. Polygenic inheritance; 11. Nature and nurture; 12. Population genetics; 13. Selection; 14. Mutation; 15. Artificial mutagenesis. The subsequent chapters are devoted to problems directly interesting mankind: 16. Human diversity; 17. Formal genetics of man; 18. Immunogenetics; 19. Mating systems in man; 20. Management of the human gene pool; 21. Genetics and politics (the Lysenko's adventure). The last chapter, by title Envoi, considers: The shape of things to come; Directions of future research; Genetics and mankind's concern. A list of further readings is followed by the aknowledge of « credits » to a number of authors whose ideas and data were presented in the course of the book. Descriptive material, most of the tabular material (and some biographical sketches) have been segregated into inserts called "boxes,, some of which include illustrations. Subject matter of peripheral relevance to the central topics has also been so treated. A Teacher's manual is appended as a separate booklet in which further bibliography is offered, followed by indications on teaching material (films) a specimen of quizzes, a specimen problem set and a specimen final examination.

The wealth of data presented, the lively and profound discussion of a great number of topics of specific or general interest, and the constant concern of pointing out any possible implication of scientific results or concepts with social affairs make of this excellent book (which is based on a course that the author has been teaching to students not majoring in biology) a pleasant and enlightening reading not only for students but also for expert geneticists. Text and illustrations are well produced; there are few misprints.

Roma, Università, Istituto di Genetica.

G. Montalenti

Sir J. Hutchinson (ed.), *Population and Food Supply* (Essays on Human Needs and Agricultural Prospects), Cambridge University Press, London, 1969, viii+144 pp., figs. and tables, 30s.

In 1966 the Faculty of Agriculture of Cambridge University decided to organize a series of public conferences on the theme of population and food supply. These lectures were repeated in the following year and are published, with slight modifications, in the present volume. The book is therefore the work of a number of authors (eight, in fact) and lacks the unity and continuity of subject matter that would perhaps be desirable in an book for a wide readership. But the simple fact of seeing the names of a geneticist and a physiologist, an economist and a human ecologist, an agronomist and a geographer all within one work gives an idea of the complexity of the problems in question. This is undoubtedly important, because today's public is convinced that the problem of world hunger does exist (but really convinced, with neither small nor sizeable mental reservations?) but also almost (self-) convinced that, for better or worse, it will be resolved. Instead this interdisciplinary examination of the situation should show them for a start that the problem is real, even if not susceptible of precise quantitative evaluation and presenting a great variety of aspects.

It is also obvious that miraculous solutions should not be expected in the coming decades, that is, in the period during which the world's population will possibly double. The distribution of American surpluses to starving populations, even if possible, is no solution: the solution is that developing countries increase both their food production and the production and exportation of industrial products.

The reader should remember that the market demand for food must not be confused with the concrete need for it and that, in short, the whole question is enormously complicated and affects economic and biological problems, those of landownership, the conservation and distribution of produce, etc. At any rate one thing is clear, and this is that even if we would like to consider many problems of food production resolved and even if we maintain that world agriculture is able to make much quantitative progress, there is nonetheless no reason to believe that such progress can continue indefinitely. Therefore all reasonable people must hope for a stabilization of population, especially since at a certain point the problem is not only that of food but also of other requirements like water and even space (as various European countries are beginning to realize). Now, whether one likes it or not, the only way to control population is by birth control. It is not enough to build organizations to fight shortages and distribute food if at the same time a continually growing population prevents the accumulation of the reserves and capital needed to better the modes of agricultural production. To abolish hunger in the world without preventing the population from growing is like emptying a tub with a pail while leaving the faucets open. This is the basic conclusion of the book, the principal merits of which are to be seen in its rich detail, in the broadness of the cultural base on which it rests, and its clarity and incisiveness.

Siena, Università, Istituto di Botanica.

G. Sarfatti

G. Radnitzky, *Contemporary Schools of Metascience*, 2 vols., Akademiförlaget, Göteborg, 1968, Vol. 1: *Anglo-Saxon Schools of Metascience*, xxiv+200 pp., figs.; Vol. 2: *Continental Schools of Metascience*, xii+200 pp., figs., SKr. 27.00 each volume.

« Metascience » whom this treatise by Radnitzky is devoted includes both the examination from the logical, semantic and epistemologic points of view of the ensembles of knowledge produced by empirical researchers, and the aspects of scientific research, particularly of the interdisciplinary one, suitable of analysis in terms of control and communication theory and in terms of cultural anthropology and interaction between science and society.

The treatise has systematical rather than historical purposes and is carefully divided in two volumes and, within each volume, in the various chapters and sections corresponding to the elaborate analytical subdivision adopted by the Author. The first volume is a critical report on the metascience of logical empirism and presupposes consequently a certain knowledge of the European and American developments of this current. The second volume is in practice independent of the first, because it is devoted in prevalence to «continental» contributions to the metascience of human sciences, a field where logical empiricism has not given results comparable to the ones obtained in the field of mathematical, physical and natural sciences.

The Author points out the « ermeneutic-dialectical » aspect of researches performed in continental Europe under the different labels of historicism, existentialism, phenomenology and marxism. In particular, he dwells upon the attempts by K. O. Apel and others towards a synthesis considering both the developments of psychoanalysis and Karl Popper's rationalistic criticism, and concludes by drawing the scheme of an ethics which could provide a unifying base for the human sciences and « secure the philosophical mediation of theory and practice ».

The second edition of this book, published in one volume in 1970, contains substantial improvements in the Introduction and in the sections dealing with H. Törnebohm's methodological theory of confirmation and with explanation.

Roma, Università, Istituto di Filosofia.

V. Somenzi

A. Rosenblueth, *Mind and Brain* (A Philosophy of Science), The M.I.T. Press, London, 1970, xiv+128 pp., 56s.

The mind-brain problem is not only a cogent and thorny topic in the fields of psychology, neurophysiology and philosophy of science, but it represents one of the fundamental metaphysical issues in the whole history of philosophical and scientific thought.

It is a great satisfaction to have with this little book a clear assessment of the questions involved in the definition of the mind-brain relations by an author who, by a rare combination, happens to be a leading neurophysiologist and an acute philosopher of science. The problem is so large and has so many ramifications that its analysis and its solutions implicate a rearrangement of a multitude of concepts and theories, as to arrive to a new philosophy of science, as the subtitle informs us.

The explanatory hypothesis proposed by the author rests on 7 postulates, which take into account the major philosophical, psychological and neurological objections that can be made against it.

In short, what is offered is a « two monistic theory » instead of a monistic or a dualistic one. It leans essentially on two cardinal points: the unexceptionable determinism implicit in the laws of physics, which regulate all the processes in the material universe our brains included, and the mental characteristics of the human perceptual knowledge. The mental events are always correlates of the neurophysiological phenomena occurring in the brain, and cannot form a link in the causal chain of events.

The free will is only our awareness of a specific mental process, and is itself a correlate of a determined spatio temporal pattern of neuronal activity.

There is no doubt that this book will become a landmark in the research and speculations on this most fascinating subject.

Roma, Università, Istituto di Filosofia.

A. Serrai

Nuova storia universale dei popoli e delle civiltà, Vol. IV: *L'ellenismo e l'ascesa di Roma*, by M. A. Levi, Unione Tipografico-Editrice Torinese, Torino, 1969, xvi+494 pp., figs. and tables, L. 6.000.

The fact that it should be a technologist to write in *Scientia's* pages about this book by an historian is not without some significance.

It takes one back to the old and memorable discussion of 1911 between Enriques and Croce, between, in other words, *Scientia* and *Critica*. Today the discussion would have no reason to be carried on if, as we firmly believe, the science of man is also his history. Enriques and Croce also held this view, but unfortunately they lived in a period that allowed of little reciprocal understanding and therefore found themselves adversaries rather than collaborators. Nonetheless the question still persists today as a contrast between the so-called « two cultures »; and it crops up in an even worse form when the « de-Hellenization » of European culture is discussed.

Instead, the many-faceted relations that link us to the ancient world need to be approached anew in the light of fresh criteria and new concepts. We can in no way cast off these relations, for the spirit of the *techne* of the modern world is, still, the spirit of Hellenism, and this will be increasingly the case in the future. If we lost the sense of direction of the historical evolution that perfectly becomes today's science, the misfortune would be very great.

For obvious reasons, we do not presume to pass judgment on this book. It arrives, however, with the right timing on the desks of those scientists who have not remained enclosed in a now unserviceable mechanicism.

Salerno, Università.

V. TONINI

M. D. VERNON, *Human Motivation*, Cambridge University Press, London, 1969, VIII+190 pp., 45s.

As a whole the psychological literature concerning the motivation of behaviour (drives, instincts, needs, interests, stimuli) is less extensive than the one concerning the cognitive processes (perception, thought, learning, etc.). Moreover the literature on human motivation is poorer than the one on animal motivation. With respect to other fields the research on motivational and emotional processes presents more methodological difficulties and occupies a comparatively backward position. The laboratory methods do not seem to be adequate and the need is felt for new methods, among which the fittest could presumably be the observation carried out in conditions which are as close as possible to the natural ones.

At present there is a very strong request for books on motivation, books which should be used mainly by students. Strictly speaking there are many volumes on this argument but often they are limited, that is they treat only one aspect of the motivational processes (homeostatic, sexual, parental, cognitive, emotional motivations, etc.) or only one approach (i.e. neurophysiological, ethologic, etc.). Otherwise one can find volumes where heterogeneous contributions, as to the argument and year of publication, are collected in an anthological form. There are few organic works, some of which are particularly clear and complete, such as the little volume « Motivation and Emotion » by E. J. Murray.

The noteworthy characteristic of this volume by Vernon (who is already known to the Italian public for many publications, in particular those on perception) is that it concerns in a specific and comprehensive way the subject of human motivation. Besides studying the development of motivation in the child, the satisfaction of biological needs, motivated behaviour in emergency reactions, emotions, the problem of activation and arousal, social motivations, interests, etc., the conclusive chapter is of particular interest and deals with those situations in which motivations, finding an obstacle to their expression, give rise to frustration and conflict situations (Lewin's theory on conflict, the anxiety problem, the Ego defense mechanisms, are illustrated).

The general theories on motivation are briefly described as the book, in the author's intentions, is meant to be a guide for students who, in the first place, need concrete data in order to have a solid basis which will later enable them to meet problems which are more abstract and of more difficult application in a human field.

This useful book is enriched by a bibliographical index consisting of more than 300 items and by a precise indication of worth studying books.

Bologna, Università, Istituto di Psicologia.

R. Canestrari

F. E. Eckardt (ed.), *Fonctionnement des écosystèmes terrestres au niveau de la production primaire*, (Actes du colloque de Copenhague), Unesco, Paris, 1968, 516 pp., figs. and tables, 55 F.

In recent years ecology has become a discipline « à la mode ». In fact its name goes back to the 19th Century (Haeckel, 1866) and its basic principles were already laid in the last decades of that century. The fact that it is becoming more and more popular among biologists as well as among laymen is mainly due to two circumstances, one of scientific, the other of practical character. As to the first one, it should be pointed out that ecology has been for a long time more or less confined to a merely descriptive stage: only recently has it begun to develop on a quantitative basis; the slowness of its development toward the stage of a more scientific attitude is due to the difficulties inherent in the subject, which are determined by the enormous number of variables at work in the ecosystems and the extremely complex net of relationships between the organisms and their abiotic and biotic environment. The practical reason is that, especially under the urge of the dreadful increase of human population in the world, the evaluation of the ressources which mankind can draw out of the different natural or artificial ecosystems has become an imperative task.

Unesco put a considerable effort in developing the theoretical and practical aspects of such ecological problems as are concerning the welfare of mankind. Dr. F. E. Eckardt of Montpellier was the organiser of the Copenhagen Symposium, the main purpose of which was the rationalisation of the methods of exploiting the biosphere. This was intended as the first of a series of symposia on this subject, and it was devoted especially to the study of terrestrial ecosystems at the level of primary production. The sessions included three kinds of contributions: introductory expositions, a series of papers on the techniques for the study of production of the plants of the system and a general discussion centered on the problems of choice, delimitation, equipment of the experimental sites.

Dr. Eckardt has opened the series of papers by preliminary remarks concerning the structure and functioning of eco-systems and the organization of the Symposium. Section I, Fundamental aspects had nine papers; not less than 31 were the contributions to Section II, Research methods; three were pertinent to Section III, Choice of experimental sites. The concluding Section, General discussion, had two papers. Every paper is followed by the discussion which occurred after its presentation and by the list of bibliographical references. Hundred and twenty were altogether the attendants to this symposium, which is certainly one of the most important and constructive ever organized in the field of ecology.

Roma, Università, Istituto di Genetica.

G. Montalenti

M. Mauss, *Manuale di etnografia* (Series: *Saggi XXS*, No. 6), Jaca Book, Milano, 1969, VIII+238 pp., L. 3.000.

In recent years and with growing interest, particularly as a result of Lévi-Strauss's editing of his principal works, Marcel Mauss has come to have the standing of a pioneer and prophet of the European social and anthropological sciences. Within the French sociological school he is undoubtedly, as Paul Mercier has stated, the main teacher and source of inspiration for the French anthropology of today. The works of Levi-Strauss, as well as those of scholars like Balandier, Berque and Bastide, are direct descendants of those of Mauss. In fact, it should be made clear at the outset that Mauss occupies a place of particular prominence compared to other authors of even the same sociological school, among them Durkheim, Hubert and Lévy-Bruhl. The works of these authors have certain serious limitations which stem from certain arbitrary principles, an excessive theoretical and synthetic zeal, and scarce sense of the concrete and of reality. Mauss, on the other hand, felt very strongly the need to found a school of true ethnological fieldworkers. And it was for this reason that he dedicated some of his academic courses to classification, to an inventory of methods, themes employed in direct inquiries *in loco*, and various aspects of culture susceptible of ethnographical analysis. He reveals a sense of the concrete, of the multiplicity and specificity of cultures and societies, a constant attention to cultural and social variability, and a methodical unwilling-ness to formulate « laws » except at the conclusion of lengthy research - a point which he, with exemplary modesty, never himself presumed to have reached. The integration of « ethnographic observation » and « theory » is fundamental for him. In the present « textbook », which is the fruit of this idea and conviction, he reiterates the ethnologist's obligation to master the patrimony of knowledge and theories that already exist in his field before undertaking research. Such are the practical perspectives of the « ethnological pedagogy » with which he prepares to gather together, in this sort of ethnographical *vademecum*, that patrimony of orientations and methodological knowledge necessary to guide a concrete piece of research. The systematic review of the aspects of the cultures to be investigated takes into account problems of demography, human geography, techno-morphology, and then technology, aesthetics, economics, law, religion, science. Each of these divisions contains within it the many-faceted possibilities offered by a study and experience of very broad scope, such as that Mauss had acquired especially from the ethnological study of the people living in the French ex-colonies (although he was not without considerable familiarity with other peoples as well). This book is still useful as a guide today, and it illustrates, as nothing else could in so brief a space, the variety of human culture and conduct, even if some of the definitions provided now seem faulty in the light of more refined research techniques and a more developed problematic. A considerable part of the book, given the author's particular interest, is devoted to a systematic study of magic and religion. The original text was first published in 1947 (second edition 1967) edited by Denise Paulme, a student of Mauss's, and is still today the only work of its kind in the whole of French anthropology, for Griaule's work, *Méthode de l'Ethnographie* (1957), covers a different kind of problems preliminary to fieldwork itself, those of general methodology. The Italian publication of Mauss's work may be seen as the continuation of the publishing endeavor already begun by Einaudi with its publication in 1965 of an important collection of Mauss's essays, *Teoria generale della magia*, which followed the publication of another work written by Mauss in collaboration with Durkheim and Hubert, *L'origine dei poteri magici* (1951). The usefulness of the present volume is not limited to what ethnographers can gain from it: it also sheds light on the whole field of ethnological and anthropological studies and is rich in extremely helpful bibliographic data. The Italian edition is usefully accompanied by brief notes on the present state of ethnological studies in Italy.

Roma.

V. LANTERNARI

L. VON BERTALANFFY, *General System Theory* (Foundations, Development, Applications), George Braziller, New York, 1968, XVI+290 pp., figs. and tables, $8.95.

The concept of « system theory » has developed gradually, during the passed thirty years, in a manner different from the wishes of one of its main founders, the German-Canadian biologist and philosopher Ludwig von Bertalanffy. This volume, containing ten or so Bertalanffy's essays and papers appeared in the period 1940-1967, aims to point out the really wider opening of new horizons of the original « Allgemeine Systemtheorie » in comparison to the present «systems engineering», mainly referred to the high complexity machinery and to the technical aspects of its various interactions with the human operator. Even if there is in common, between the two methodologies, the tendency to consider the organizatory aspects giving to a system of natural or artificial components properties qualitatively differents from those concerning its single components or only their mere sum, the Author's « organismic » conception appears in fact as an attempt in a new « Mathesis universalis », able to comprise in a theoretical unitary frame both physical and chemical phenomena and biological, psychological, psychopathological, linguistic and social phenomena, where the phenomena examined by the systems engineering are to be enclosed as very particular aspects.

The volume offers a rather summary idea of the programs and results of the new field of research opened by Bertalanffy, but its extensive bibliography orients readers also with reference to parallel movements, from the neopositivistic one for the unity of science to cybernetics and mathematical biophysics, which have often met with successes much more considerable just in virtue of their greater independence from the philosophical prejudicials characterizing the statement of the « General System Theory ».

Roma, Università, Istituto di Filosofia.

V. SOMENZI

M. BURNET, *Cellular Immunology*, Cambridge University Press, London, 1969, X+726 pp., figs. and tables, £5.10.0.

This volume on Immunology, by M. Burnet, is certainly one of the most significant works on the subject that has appeared in recent years, both on account of the vast amount of factual knowledge revealed and because of the shrewdness with which the individual facts are interpreted, many of these interpretations being wholly original and stimulating.

The volume consists of two complementary parts.

The first part, as the author states in the introduction, aims at presenting immunisation essentially « as a Darwinian process of hereditary variations, selection through proliferation and death among the mobile cells of the body of mammals ». Consequently, it is of a more strictly biological nature and is therefore also intended for experts in other fields of Biology. The second part, on the other hand, is more technical, which means that it is chiefly intended for immunologists.

The guiding purpose of the work is to give support, on the basis of experiments, to the well-known - and brilliant - clonal theory that the author introduced in Immunology several years ago. For this purpose, the author has had the excellent idea of providing a list of demonstrations of experimental facts or of cases of human pathology that are particularly consistent with this theory. For each of the more important subjects, the list gives the number of the relevant chapter, which is the same for both parts of the volume.

As a whole, the volume is of extreme interest and is bound to become a classic of Immunology.

Napoli, Università, Istituto di Patologia Generale.

L. Califano

H. W. Woolhouse (ed.), *Dormancy and Survival* (Symposia of the Society for Experimental Biology, No. XXIII), Cambridge University Press, London, 1969, viii+598 pp., figs. and tables, £5.

The present volume contains the papers presented at the twenty-third Symposium of the English Society for Experimental Biology, the title of which was « Dormancy and Survival ».

These contributions are of really exceptional interest, not only because they give an account of studies carried out on particular subjects but especially because many of them are framed in terms of general questions. In short, the actual purpose of this Symposium was to contribute to an understanding of the problem of biological dormancy and especially of the mechanisms of survival in extreme conditions: in other words, that complex biological phenomenon known empirically as latent life, such as that of spores (from bacterial to fungous), that of protozoa in the cystation stage, that of nematodes in the hibernation stage and, last but not least, that of seeds and bulbs.

The various authors have admirably achieved their purpose, or at least some of the phenomena considered have been explained and the intrinsic problems in others have been outlined.

Since the symposium is concerned with survival in a broad sense - that is to say, every form of survival in extreme environmental conditions - there are also papers on the physiology of vegetation in the desert and in the humid areas of the tropics, as well as on the hibernation of mammals.

Among other interesting papers is that of J. Levitt, entitled « The growth and survival of plants at extreme temperatures: a unified concept ». This particular contribution is cited for the very reason that it expresses a tendency - common to other contributions, as already mentioned - towards research on general biological concepts.

Napoli, Università, Istituto di Patologia Generale.

L. Califano

C. D. Broad (ed.), *Induction, Probability, and Causation*, D. Reidel Publishing Company, Dordrecht, 1968, xii+296 pp., Dfl. 48.

This volume contains Broad's collected papers about induction and probability, precisely: a) a paper on the connexion between induction and probability, b) a paper on the principles of demonstrative induction, both published in *Mind*, respectively in 1918-20 and 1930, c) two essays about the principles of problematical induction and of mechanical and teleological causation respectively, published in the *Proceedings of the Aristotelian Society*, in 1927-28 and in 1935. There are also reviews, already appeared in *Mind*, of volumes by Keynes, Johnson, Von Mises and Kneale. An essay by G. H. Von Wright, devoted to Broad's thinking on induction and probability follows. The book ends with a reply of the Author to various critics. Broad's fundamental concept asserts that induction is only probabilistic, for either applying to the principle of simple) enumeration or to the principle of hypothetical method, formal mistakes are made. With regard to the philosophy of probabilities, Broad's views are similar to Keynes and Johnson's ones.

Milazzo (Messina).

G. Vaccarino

G. G. Patzig, *Aristotle's Theory of the Syllogism* (A Logico-Philological Study of Book A of the Prior Analytics), D. Reidel Publishing Company, Dordrecht, 1968, xviii+216 pp., Dfl. 45.

Aristotelian logic is examined by means of the modern formal logic's methods, employing the results by G. Lukasiewicz (whom the book is dedicated), appeared in the fundamental book « Aristotle's Syllogistic from the standpoint of modern formal logic », Oxford, 1951 (English translation). The Author bears constantly in mind also another classic work in the field — « Analytics » by Sir David Ross (Oxford, 1949). The aim he has in mind to pursue is to connect the fundamental results obtained by Lukasiewicz to the traditional interpretations (Prantl, Zeller, Überweg, Maier, Trendelenburg, etc.), which among other things derived Aristotle's syllogistics from his methaphysics.

The author allows that the presentation by Aristotle may have been influenced by his ontological trend, but he thinks that, should a dependence be admitted, there is inclination for the one leading back ontology to logic. Among the particular theses, we remark that, according to the author, the omission by Aristotle of the fourth sillogistic figure is a necessary consequence to his fundamental definitions, whereas, according to Lukasiewicz it is nearly attributable to negligence.

Milazzo (Messina).

G. Vaccarino

B. A. Lengyel, *Introduction à la physique du laser*, Editions Eyrolles, Paris, 1968, 358 pp., figs. and tables, 69 F.

This book, translated from English, intends to give a panoramic survey of all the knowledge in the field of laser physics on the basis of publications which have appeared before 1965. It has a didactic purpose and is intended for students of physics and engineering, with the aim of giving an introduction to laser physics, the theory of its functioning, the constructive principles of various types of laser (solid state, liquid and gas laser), the study of the oscillations that are stabilized in the laser, the non-linear phenomena. A short chapter is dedicated to applications, and mention is made of speed measurements and other optical measures, of the employment as a source for spectroscopic analysis, the utilization in the fields of communications and of metallurgy, and lastly in biology and medicine.

Provided with an extensive bibliography and written in an easy and clear manner, it is a very useful book for all those who have a basic knowledge of physics and intend to enter into this field of research that day by day proves to be more and more interesting and important.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

Oeuvres de Salomon Rosenblum, Gauthier-Villars, Paris, 1969, 364 pp., figs. and plates, 76 F.

Salomon Rosenblum (1896-1959) was, along with J. F. and I. Joliot-Curie, one of the main protagonists of French nuclear physics in the early after-war period. Among his discoveries, the one of the sharp structure of the thorium C alpha spectrum and its coordination to the gamma rays emitted by that matter had a fundamental importance in the knowledge of atomic nucleus. The reviewed volume is a collection of the most significative works by this French physicist. In a limpid and concise introduction, masterly written by Francis Perrin, the figure of Rosenblum and his contribution to the development of nuclear physics are exposed to give the reader the elements requisite for a deeper comprehension of the meaning of the various researches performed by Rosenblum.

At the end of the volume we find the complete listing of Rosenblum's scientific papers, extending along a period beginning in 1925 and ending with his death in 1959.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

G. P. Perona, *La fisica concettuale* (Series: *La Scienza del Pensare*, Vol. I), Unione Tipografica Editrice Piacentina, Piacenza, 1969, 134 pp., figs., L. 4.000.

In Hegel's mind to learn a philosophic method and apply not it seriously to real objects of research, say natural science, was just like to learn swimming without plunging. Yet this laudable premise, claimed as a personal one by the Author who, being an idealist, conforms himself to Hegel, has no furthering in the course of the book, which is to be included in the old tradition of Italian actualism.

Neverthless there is newness in the argument: a « rational » interpretation and a philosophical review of the Relativity and Quanta theories, included in a general conception of physical categories. In fact, Italian idealism has usually limited natural science to instrumental and unhistorical pseudoknowledge, undeserving the attention of philosophers. In the Author's explanation the main reason for such indifference of « philosophers » (that is idealists, because he considers not empirism, materialism and language analysis as philosophies, but as « particular scientific positions, which may bring some help to a certain science... or also to all, but not in pursuing a superior whole view.. ») is the difficulty to learn « the mathematical algorithms and the physical symbolism reserved to adepts and that philosophers consider themselves dispensed from knowing... ». The scarce penetration of science in Italian culture is the basis, besides idealistic obscurantism, also of the ingenuous fetichism of science. On the other side, according to the Author, physicists, being not methodical philosophers, could never solve the serious contradictions of modern physics, due to the lack of rational interpretations and comprehensive critical reviews of the concepts. We find here the reproposition, instead of the actual interdependence between science and philosophy, mutually useful, of Gentile's pedagogical hierarchy between teacher (philosopher) and pupil (scientist), in which it is duty of the former to inspire in the latter the initially absent rational selfconscience. So philosophy, imposing its concept with the label « universal and necessary », « logically deduced and not assumed in advance », would come back, in Gentile's way, to be « the science including all the remaining sciences without being included by them ».

Then the Author treats a concept of ether as a philosophical scheme of physical considerations and mathematical formulations, which appear, for the rapidness of treatment, neither always opportune nor fitting. From this comes out a work pervaded of goodwill, but featuring an idealistic mixing of formulas, concepts and models, without the necessary mediations and punctualizations requested for the still pursued rational unification of modern physics.

Roma, Università, Istituto di Fisica.

A. Rossi

J. C. Pecker, *L'astronomie expérimentale* (Series: « *La Science Vivante* »), Presses Universitaires de France, Paris, 1969, 156 pp., figs., tables and plates, 20 F.

I. S. SHKLOVSKY, *Supernovae (Interscience Monographs and Texts in Physics and Astronomy*, Vol. XXI), John Wiley and Sons Ltd., London, 1968, VIII + 444 pp., figs. and tables, 150s.

B. L. VAN DER WAERDEN, *Erwachende Wissenschaft*. VOL. 2: *Die Anfänge der Astronomie* (Series: *Wissenschaft und Kultur*, Vol. 23), Birkhäuser Verlag, Basel, 1968, XII + 376 pp., figs., Fr. S. 38.

How fashionable astronomy has become with the beginning of space voyages, can be seen from the remarkable amount of astronomical publications which nowadays appear.

These three books are of different character, going from the beginning of astronomy to its most recent developments.

The booklet by Pecker, professor at the Collège de France and director of the Nice Observatory, sets the question whether astronomy and astrophysics are experimental sciences. It is certain that in their modern development, on the one hand with the possibility of reproducing in our terrestrial laboratories some celestial phenomena, and on the other hand with the employment of artificial satellites by means of which the direct examination of some celestial bodies has started, we have reached a point at which observation and experimentation have brought new knowledge of the Universe.

In the first part of the book, the author expounds mathematically the fundamental principles of celestial mechanics, that together with those of astronautics are useful to compute the orbits of satellites and to guide them in their explorations of the solar system. In the second part are discussed the direct explorations of the Moon and the nearest planets, Mars and Venus. Considerations follow on the plurality of inhabited worlds and speculations that today are pursued of the possibility of the existence of life like ours on other planets outside the solar system. A book on the whole original in its contents, adapted for students having good mathematical knowledge.

The study of the so-called « supernovae » stars was still lacking in the astronomical literature; they were discovered not many years ago, and thus called for their very high luminosity (in comparison with the novae) when their explosion occurred. In this book the author gathers a good deal of observational and theoretical material on this grandiose celestial phenomenon.

Today, owing also mostly to radio-astronomy, we have acquired noteworthy knowledge on the complex physical processes that occur in the nebulae which are certainly the remains of the supernovae. It has been discovered, for example, that the optical radiation of the Crab nebula, which represents the rest of the cosmical catastrophe due to the explosion of the supernova 1054, is of a synchrotronic nature. Although the causes of the supernova's explosion is not yet explained with certainty, nor how these stars differ from all the others, the researches based on the application of the most different methods and of which an account is given in this book, are fascinating. The book is divided into four chapters, one of which is dedicated to the Crab nebula. It is noticed that the translation from the original Russian is not quite adequate.

The purpose of Van der Waerden's book, as the author himself explains, is to give in a clear manner comprehensible to a large class of people, therefore not only for astronomers, an outline of the beginning of astronomy in Egypt and then in Babylon. In reality the astronomy of ancient Egypt was very primitive and essentially a religious doctrine used for practical purposes, such as for the calender. After 1800 b.C. it can be said that there existed no progress in astronomy on the part of the Egyptians. The Babylonian astronomy instead continued to develop in a really admirable way from about 1200 to 150 b.C. and it is this development that is mainly dealt with by the author. The book is divided into six chapters that deal with: the beginning of astronomy on the part of the Egyptians and Babylonians, the epoch of the Assyrians, the new Babylonian and Persian epoch, Babylonian computation of the Moon as well as of the planets, the religion of the stars, astrology and astronomy, and lastly, the development of Babylonian astronomy.

On the whole an accurate work, widely documented, that can be useful not only for a knowledge of the history of astronomy among those peoples, but also of their general culture at the time of the revival of science.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

M. Hack (ed.), *Mass Loss from Stars* (Proceedings of the Second Trieste Colloquium on Astrophysics, 12-17 September 1968), (Series: *Astrophysics and Space Science Library*, Vol. 13), D. Reidel Publishing Company, Dordrecht, 1969, xii+346 pp., figs., No price stated.

The editor, Margherita Hack, director of the Trieste Observatory, comments that this book contains the papers presented during the second colloquium on astrophysics held in Trieste. The problem of mass loss from stars is an important one, since it is related to their evolution; it has been studied both observationally and theoretically. The book begins with a detailed account by Deutsch of our present knowledge concerning mass loss from stars due to events in their interior, whereby atomic processes convert mass into energy. This conversion may be a violent one, as in the outbursts of novae and supernovae, or a steady one, as in the Sun, where it causes also the solar wind.

The authors, 44 in number, who gave papers at the colloquium, can be classified into two groups: those who consider the problem on the basis of observations of various stars having atmospheric motions with gas expansion or phenomena in extended atmospheres; and those who deal with the problem theoretically on the basis of models of various types of stars. The book is consequently divided into five sections. The first and third sections discuss observed mass loss from single stars and binary stars. The second and fourth are concerned with the theory of mass loss from single stars and close binaries. The fifth section relates to mass loss deduced from observations of unstable stars. As an example (Faraggiana and Hack), there is spectroscopic evidence for mass loss from the semi-regular M6 variable CH Cygni, indicating that its mass loss is at the rate of 10^{-8} solar mass per year.

The book is an excellent survey of this important problem.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

S. CHAPMAN and R. S. LINDZEN, *Atmospheric Tides* (Thermal and Gravitation), D. Reidel Publishing Company, Dordrecht, 1970, x+200 pp., figs. and tables, Dfl. 38.

Everyone is familiar with sea tides. Now the tidal forces must affect the whole fluid constituent of the earth surface: that is, both the sea and the atmosphere. In the case of the atmosphere the phenomenon is not complicated by irregular boundaries and changes in depth; however, besides the gravitational action of the sun and the moon, there is the thermal action of the sun, which excites « thermal tides ». The problem involves hydromagnetic forces, molecular viscosity and conduction, modified composition at high levels, etc. Moreover the strength of the tides increases with the height in the atmosphere and in the ionosphere, strong winds exist and the tidal changes are reckoned in kilometers.

« Atmospheric Tides » describes the present state of affairs in this far from simple matter and indicates the main lines for future progress.

The first chapter contains both the history of the problem and the outlines of the methods employed for research. Starting from Pytheas, Bacon, Newton and Laplace, it ends with rocket measurements (which exclude, once and for all, Kelvin's resonance theory).

The second chapter is divided into two parts. It deals with solar daily atmospheric oscillations and with lunar atmospheric tides as revealed by meteorological data.

The third and last chapter contains the quantitative theory of atmospheric tides. It gives the reader the most information; all the aspects of the problem and all the effects involved are taken into consideration. The comparison between theory and data is followed by a sound criticism at the end of the chapter.

The book as a whole is a complete and self-contained account of the subject. It can be read by non-specialists in atmospheric problems, although its level is a very high one and full use is made of mathematics.

A sad event gives this work a particular meaning. « Atmospheric Tides » was published in 1970, just after the death (on June 16th, at the age of 82) of one of the two Authors: Sidney Chapman. This circumstance reveals to us the famous scientist working us to the last moment of his life which was so valuable for scientific progress.

Firenze, Osservatorio Astrofisico di Arcetri.

G. CALAMAI

L. HOUZIAUX and H. E. BUTLER (eds.), *Ultraviolet Stellar Spectra and Related Ground-Based Observations* (I.A.U. Symposium No. 36, held in Lunteren, The Netherlands, 24-27 June 1969), D. Reidel Publishing Company, Dordrecht, 1970, XVI+362 pp., figs. and tables, Dfl. 60.

After every important discovery in a particular branch of scientific researches we notice that researchers start working so intensely in the new field that shortly afterwards it is necessary to review the situation, finding the connections with the other sectors of science and avoiding that the new and promising vein becomes barren. This case fits also space research, in which the original impetus, obviously necessary, was represented by the rapid technological improvement devoted to the acquisition of the observational experimental data. Of course, to reduce these data and be able to suggest in future the most significant experiments, it is necessary to connect the various aspects of knowledge in a single scientific problem and this was just the aim of the I.A.U. Symposium on UV stellar spectra and corresponding ground-based observations, whose proceedings are published in the here reviewed volume.

This aim has been surely achieved and the volume gives a wide and full view of the status of man's knowledge on stellar spectra.

The volume is divided into 3 parts, the first is devoted to the study of stellar fluxes; following various contributions to the problem of absolute calibration in the various spectral regions, are taken into consideration interstellar extinction and theoretical models of the fluxes, to permit then a critical comparison with the observations and obtain, in this way, conclusive interpretations. In the second part are discussed the spectra of lines, beginning with an interesting series of space observations of UV spectra obtained at ground level; some contributions on the fundamental theory for the UV spectra interpretation and finally on the importance of our knowledge of the Sun conclude this second section of the volume. The last part is devoted to interstellar absorption and emission, mainly from the observational point of view.

The book is consequently very interesting, scientifically important and pretty well refined by the publisher in the sharpest details.

Firenze, Osservatorio Astrofisico di Arcetri.

R. Falciani

L. Chertok (ed.), *Psychophysiological Mechanisms of Hypnosis* (An International Symposium sponsored by the International Brain Research Organization and the Centre de Recherche, Institut de Psychiatrie, La Rochefoucauld, Paris), Springer-Verlag, Berlin, 1970, VIII + 208 pp., figs. and tables, DM 48.

The Psychophysiological Mechanisms of Hypnosis edited by L. Chertok contains all the papers presented at the International Symposium on Hypnosis held in Tourtour (France) on October '67. After an historical introduction in which L. Chertok has summarized the physiological, psychological and psychoanalytical approaches to the study of hypnosis, S. Black presented some experimental data on the Expectancy-wave (E-wave of G. Walter) during suggestion and hypnosis. P. Buser and G. Viola found that pressure on the skin of dorso-lumbar zone in rabbits can induce inhibition of rhytmic limb flexion and extension, a fact which provides an explanation of the akinetic immobilization which occurs when the rabbit is forced into a supine posture. The electroencephalographic aspect of hypnosis was considered by H. Gastaut, with attention to analogous situations such as zen and yoga meditations or pre-sleep patterns during which the subject experiences some degree of hypnagogic thinking. Neurophysiological studies on hypnosis were presented by J. Hoskovee (evaluation of the pavlovian theory of hypnosis), D. Langer (changes in blood circulation during hypnosis), E. Sokolov (mechanisms of habituation in vertebrate single neurons and in invertebrates), P. Wall (physiological control in sensory pathways with special reference to pain). At the end various contributions are presented dealing with the hypnotic susceptibility and with the nature of posthypnotic suggestion. The book presents interesting and stimulating, although not always scientifically orthodox, data and extrapolations useful for the understanding of this fascinating and yet unknown phenomenon.

Milano, Università, Laboratorio di Fisiologia Umana.

M. MANCIA

G. A. PARTEL (ed.), *Space Engineering* (Proceedings of the Second International Conference on Space Engineering, held May 7-10, 1969, in Venice, Italy) (*Series Astrophysics and Space Science Library*, Vol. 15), D. Reidel Publishing Company, Dordrecht, 1970, x+728 pp., figs. and tables, Dfl. 140.

This volume contains 49 papers presented at the Conference, including several works of high scientific interest. The subject matter is divided into nine parts which relate respectively to structures and materials; guidance and control systems; propellant and combustion I, II; propulsion I, II; auxiliary power systems; testing; supports; accessories.

Besides contributions from the two leading countries « in space » there are accounts of experimental work by what are minor ones from this point of view, and a full representation of those taking part in the European space programmes.

Special interest attaches to the papers from USA on the Mariner missile to Mars (by Fearey, Renzetti, Casani), on problems of combustion (by Forbes, Murthy, Torda) and on propulsion systems for space vehicles such as the Gemini and Apollo capsules (by Feldman and Moberg). From the USSR there is a consideration, by Academician Petrov, of problems arising in the guidance control of satellites and these are also treated in a French contribution (by Léorat, Gauvrit, Brosson, Capraro) illustrated by an interesting mathematical model of the guidance for a communication satellite.

The Italian participation is substantial too and a particular mention should be made here of what has been supplied by the Fiat research workers I. Capriolo, V. Sacchi and E. Padovano; also by the group formed by R. C. Michelini and R. Ghigliazza of the Institute for Applied Mechanics in the University of Genoa as well as by D. Dini and E. Gimelli at the Nuova San Giorgio.

The typography is good and the work should be of a great value to studians people of space engineering problems because of the high level at which it covers a wide range of questions.

Torino, Politecnico.

P. M. CALDERALE

T. SARPKAYA (ed.), *Proceedings of the 1970 Heat Transfer and Fluid Mechanics Institute* (held at the Naval Postgraduate School, Monterey, California, June 10-12, 1970), Stanford University Press, Stanford, 1970, XII+370 pp., figs. and tables, $15.00.

This collection of 23 papers in the 1970 Proceedings will be of great interest to all specialists in the heat and fluid dynamics of boundary layers and turbulence at contact between a moving fluid and a wall; and the book is a pleasure to read.

Of the 23 papers, six are largely experimental, the remainder predominantly analytical or numerical. They are interspersed with five general lectures bearing the names of distinguished scientists in this field: H. Schlichting, C. S. Yih, G. K. Batchelor, M. Van Dyke and F. Kreith.

The authors are mainly Americans, with two Canadians, two Germans, one British and no Russians.

The American authors include persons with names of various origins; in particular, there are, as usual, Chinese names associated with some of the most advanced of the mathematical-physics papers.

This evident concentration of American papers may impose an unwelcome restriction on the expert reader. But if it is remembered that the 23 papers were selected according to strict criteria of originality from as many as 112 submitted, one can appreciate the great difficulties overcome by Dr Sarpkaya and the Editorial Committee in choosing such advanced and often difficult contributions.

Of the 23 papers, nine deal specifically with problems of the turbulent boundary layer (Nos. 1 - 3 - 8 - 13); three include also the effect of radiation on turbulent heat echange (Nos. 3, 17, 18); two concern vibrations in a moving fluid, offering an interesting prospect of the subject of vibrational order, stochastic and otherwise, vorticity and turbulent kinetic energy (Nos. 4, 5); four discuss two-phase or two-component motions (Nos. 4, 6, 10, 20); two are about mass transfer and fluid-wall contact with injection and chemical reaction (Nos. 11, 15); three are on transfer under supersonic conditions and shock waves (Nos. 7, 12, 18); three deal with slip, dissociation and magnetohydrodynamics (Nos. 13, 14, 22); one concerns transient heat conduction with boundary convection and periodic power generation, with general graphs for plane and cylindrical geometry (No. 19); one concerns turbulent activation by microfins in a high-temperature gas (No. 23); and two describe original and up-to-date techniques of numerical calculation and matched asymptotic expansion (Nos. 16, 21).

Apart from its obvious scientific and academic interest, the book is vivid and meaningful because it gives a snapshot of the work and progress in turbulence in the year 1969. This work seeks to escape from the deadlock which has for some years been the fate of several conventional models for the boundary transfer of mass, momentum (friction) and heat.

Until computers allowed the solution, by practicable means, of the elegant mysteries of the non-linear Navier-Stokes equations and their extrapolations, it seemed that the models of the boundary layer and turbulence, made famous by Reynolds, Prandtl, Taylor, von Kármán and others up to Lighthill and Spalding, were good approximations to the physical truth. But with the arrival of the computer, a steadily increasing uneasiness appeared, and a lack of confidence in the validity of extrapolating the Navier-Stokes equations to *non*-laminar conditions of motion, the case which is in fact relevant to most applications of heat exchange.

Many authors abandon the idea of turbulent diffusivity, at least as involving quantities constant throughout the thickness of the fluid; and even the concept of viscous laminar flow (which is in principle never irrotational) loses any exact significance.

Thus new lines of research arise in which there is an attempt to restate the problem of turbulence in a more general form, supplementing the classical equations of conservation and transport by two new equations, those of turbulent energy transport and decay.

In a brief account such as the present review there would be no point in trying to discuss the large range of critical analysis of diffusivities, tangential stresses at the walls, velocity and temperature distributions, stability of turbulent ordering, position of maximum velocity and zero tangential stress, the essential difference between internal and external, two-dimensional and three-dimensional flow or the theoretical and numerical techniques used in the analysis.

A careful study of this volume is warmly recommended to all who are concerned with basic research on turbulent heat exchange. As well as ideas of stimulating originality, it contains an extensive, detailed and specialised bibliography.

Milano, Politecnico, Istituto di Fisica Tecnica.

R. Arienti

LIBRI RICEVUTI - BOOKS RECEIVED

A partire da questo numero le opere elencate nella rubrica "Libri ricevuti,, non verranno più raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

Starting with the present number, the works listed in the section "Books received,, will no longer be grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

ADAMS A. - The Eternal Quest.
ALLAND A. JR - Adaptation in Cultural Evolution: an Approach to Medical Anthropology.
ARMELIN G. - L'association acupuncture-ozonothérapie.
BAJA R. - La méthode biologique.
BASSET A. and MALEVILLE J. - Les eczémas et leur traitement.
BECKER W. and CONTOPOULOS G. (eds.) - The Spiral Structure of our Galaxy.
BENIRSCHKE K. (ed.) - Comparative Aspects of Reproductive Failure.
BENNET TH. P. - Elements of Protein Synthesis.
BERTEIN F. - Bases de l'électronique quantique.
BONTE A. - Introduction à la lecture des cartes géologiques.
BRANDT J. C. - Introduction to the Solar Wind.
BROCARD H. and LEMOYNE T. - Courbes géométriques remarquables. (Courbes spéciales). Planes & Gauches.
BUNGE M. - La causalità.
CAMPBELL J. A. - Chemical Systems.
CARNOT L. - Réflexions sur la métaphysique du calcul infinitésimal.
CASTAING R. - Cours de thermodynamique statistique.

CEREIJIDO M. and ROTUNNO C. A. - Introduction to the Study of Biological Membranes.
CHAMBADAL L. - Dictionnaire des mathématiques modernes.
CHERTOK L. (ed.) - Psychophysiological Mechanisms of Hypnosis.
CHIODI G. M. - Legge naturale e legge positiva.
CLARKE C. A. - Human Genetics and Medicine.
CLARKE G. M. - Statistics and Experimental Design.
COOPER B. E. - Statistics for Experimentalists.
CORDATI ROSAIA L. and TORNATORE L. - Matematica.
DALRYMPLE G. B. and LANPHERE M. A. - Potassium-Argon Dating.
D'ANS-LAX - Taschenbuch für Chemiker und Physiker.
Department of Terrestrial Magnetism.
DE PLAEN J. L. - Aspects dynamiques du métabolisme glucidique chez l'embryon de rat. Action de substances hypoglycémiantes et tératogènes.
DI AICHELBURG U. - L'Uomo.
DUMOULIN J. and DE BISSCHOP G. - La galvanothérapie.
EINSTEIN A. - La théorie de la relativité restreinte et générale.
FIRSOFF V. A. - Vie, intelligence et galaxies.
FLORKIN M. and STOTZ E. H. (eds.) - Comprehensive Biochemistry.
FRANÇON M. - Physique CB-BG.
FRÉDÉRIQUE - Les enfants et la mathématique.
GAGNAIRE D., JAULMES P. and BUGAREL R. - Extraction et purification des espèces chimiques.
GEYMONAT L. - Storia del pensiero filosofico e scientifico.
Grande Dizionario Enciclopedico U.T.E.T.
GRASSÉ P.-P. (ed.) - Traité de Zoologie. Anatomie, Systématique, Biologie.
HAAGE W. - Guide des Cactus.
HALL V. E., GIESE A. C. and SONNENSCHEIN R. R. (eds.) - Annual Review of Physiology.
HARDEN JONES F. R. - Fish Migration.
HARMUTH H. F. - Transmission of Information by Orthogonal Functions.
HELFRICH H. W. JR (ed.) - The Environmental Crisis.
HILL A. V. - First and Last Experiments in Muscle Mechanics.
HUSSON R. - Glossaire de biologie animale.
JACQUARD A. - Structures génétiques des populations.
JERSILD A. T. - Psicologia dell'adolescenza.
KEATTCH C. J. - An Introduction to Thermogravimetry.
KERVRAN C. L. - Transmutations biologiques en agronomie.
KIRSCHENMANN P. P. - Information and Reflection.
KOPAL Z. - Widening Horizons.
KOURGANOFF V. - Introduction à la physique des intérieurs stellaires.
LAGRIFFE L. - Vieux remèdes du temps présent.
LAKATOS I. and MUSGRAVE A. (eds.) - Criticism and the Growth of Knowledge.
LARGEAULT J. - Logique et philosophie chez Frege.
LASZLO E. and WILBUR J. B. (eds.) - Human Values and Natural Science.
LEFEBVRE H. - Du rural à l'urbain.

LERMAN I. C. - Les bases de la classification automatique.
LIPPARD S. J. (ed.) - Progress in Inorganic Chemistry.
LIVOLSI M. - Comunicazioni e cultura di massa.
MACINNIS A. J. and VOGE M. (eds.) - Experiments and Techniques in Parasitology.
MANNO V. and PAGE D. E. (eds.) - Intercorrelated Satellite Observations Related to Solar Events.
MARITZ J. S. - Empirical Bayes Methods.
MAYER H. M. and WADE R. C. - Chicago: Growth of a Metropolis.
MEDVEDEV Z. A. - The Rise and Fall of T. D. Lysenko.
MEYNELL G. G. and MEYNELL E. - Theory and Practice in Experimental Bacteriology.
MONOD M. - Manuel d'application du Test du Village.
MOORE P. - Eléments d'astronomie.
NEEDHAM J. - Clerks and Craftsmen in China and the West.
NÖTZOLD G. (ed.) - Die arabischen Länder.
OHNO S. - Chromosomes sexuels et gènes liés au sexe.
PARFENOFF A., POMEROL CH. and TOURENQ J. - Les minéraux en grains.
PAULING L. - General Chemistry.
PECKER J.-C. - Experimental Astronomy.
PERREAU P. (H. SOUBIES-CAMY) - L'enregistrement magnétique de mesures.
PIOTTE J.-M. - La pensée politique de Gramsci.
PIRCHNER F. - Population Genetics in Animal Breeding.
PIRENNE M. H. - Optics Painting and Photography.
POPPER K. R. - Logica della scoperta scientifica.
Progress in Brain Research.
QUÉNU J. - Nouvelle pratique chirurgicale illustrée.
RICHARD G. (ed.) - Territoire et domaine vital.
RIVIER W. - De l'imaginaire au réel.
ROHAULT J. - A System of Natural Philosophy.
ROUBINE E. (ed.) - Mathematics Applied to Physics.
ROURE F. and BUTERY A. - Mathématiques pour les sciences sociales.
RUNCORN S. K. - The Application of Modern Physics to the Earth and Planetary Interiors.
RUSSELL F. S. - The Medusae of the British Isles.
SARPSKAYA T. (ed.) - Proceedings of the 1970 Heat Transfer and Fluid Mechanics Institute.
SCHEIDEGGER A. E. - Theoretical Geomorphology.
SCHWARZBACH M. - Berühmte Stätten geologischer Forschung.
Selected Lectures of the Royal Society.
SERRE J.-P. - Cours d'arithmétique.
SETTEMBRINI P. (ed.) - Medicina 3.
SIMPSON G. G. - La géographie de l'évolution.
SLETTEBAK A. (ed.) - Stellar Rotation.
SMITH H. M. - Principles of Holography.
SOLARI G. C. and GONZALES G. C. - Los médicos y el control de la natalidad.

SOURIAU-HOEBRECHTS C. - La presse maghrébine.
SRB A. M., OWEN R. D. and EDGAR R. S. (eds.) - Facets of Genetics.
TEMAM R. - Analyse numérique.
The Growth Points of Physics.
TIMMONS C. J. (ed.) - Modern Reactions in Organic Synthesis.
VALSON C.-A. - La vie et les travaux du Baron Cauchy.
VELIMIROVIC M. - Atlante Atomico.
VERNIN G. - La chromatographie en couche mince.
VON KOVACS E. - Raketen-Einmaleins.
WACKENHEIM A. and ESCUDERO L. - Neuroradiologie.
WALES A. P. (ed.) - World Directory of Booksellers.
WEDEPOHL K. H. (ed.) - Handbook of Geochemistry.
WINAND J. - Aspects qualitatifs et quantitatifs du métabolisme lipidique de la souris normale et de la souris congénitalement obèse.
ZÁRUBA Q. and MENCL V. - Landslides and their Control.

ADAMS, A., **The Eternal Quest** (The Story of the Great Naturalists), Constable and Company Limited, London, 1970, 510 pp., 70s.

The story of the naturalists who, over the centuries, made the chief contributions to man's search for his place in the natural world is an absorbing one. The study of natural science flourished at the time of the early enquiries of Aristotle, Lucretius and Pliny, before the onset of the dark ages and the long dormancy of scientific investigation in the known world. This hunger for knowledge was not reawakened till the early days of the Renaissance, when Copernicus, Kepler and Galileo began to melt the ice of ignorance. In England, they were followed by such men as Francis Bacon, Robert Boyle, Isaac Newton and the astronomer Halley. By the beginning of the 18th century, the age of reason increasingly required that order should replace chaos in the field of natural history, and Linnaeus and Buffon followed their remarkable careers against its background. In France, Lamarck and Cuvier made great discoveries in invertebrate zoology and fossils and they are followed in the story by the great naturalists Alexander Wilson and Audubon. Next come the explorer Humboldt, the great geologist Lyell and the palaeontologist Agassiz, before we reach the larger than life size figures of Darwin, Wallace and Huxley, and finally Mendel's great work on genetics. No two of the great naturalists were alike, but all of them, in the author's words, were 'representative of all men's thinking, blinded by prejudice and preconceived ideas, yet driven by intellectual discontent and therefore continually searching'.

ALLAND, A. Jr, **Adaptation in Cultural Evolution: an Approach to Medical Anthropology,** Columbia University Press, London, 1970, x+204 pp., 63s.

The subject of this book is the impact of disease-related human behavior upon the evolution of culture. The author believes that the neo-Darwinian theory of evolution can be applied successfully to socio-cultural change. He indicates how some of the materials and techniques of medical anthropology can be used to integrate data on human behavior into an evolutionary framework. Examples and hypotheses are drawn from the field of medical anthropology, epidemiology, and public health. The fields covered include ecology of disease in relation to culture, the application of Darwinian theory to cultural anthropology, historical epidemology, native medical practice, and the impact of western medicine on non-western peoples. The opening chapter provides a basic introduction to terms in medical anthropology, for interested non-specialists. The book concludes with a summation of the hypotheses proposed throughout and a discussion on the research implications of these hypotheses for anthropology. While the book presents a specific approach to medical anthropology, it will be of interest to others concerned with the development of this new subdivision of anthropology.

ARMELIN, G., **L'association acupuncture-ozonothérapie** (Une thérapeutique nouvelle), Librairie Maloine S.A., Paris, 1970, 112 pp., 19 F.

Cet ouvrage est le fruit d'une longue et persévérante expérimentation clinique. Il témoigne que l'association de deux thérapeutiques actives l'acupuncture et l'ozonothérapie, utilisées jusqu'ici séparément, apportent par leur conjonction une somme majorée de résultats. Convaincue de l'intérêt fondamental de cette méthode pour le maintien de l'équilibre organique, l'auteur souhaiterait entraîner l'adhésion du plus grand nombre de praticiens.

CONTENTS. - *I: L'ozone.* - *II: Le traitement.* - *III: La méthode.* - *IV: Les résultats.*

BAJA, R., **La méthode biologique,** Masson et C.ie, Paris, 1969, VIII+210 pp., figs., 26 F.

L'objet initial de ce recueil était modeste. Il s'agissait, pour l'auteur, de fournir à ses élèves quelques textes scientifiques pouvant offrir une base concrète à leur réflexion sur les méthodes biologiques. Ce livre rassemble des textes écrits par les grands fondateurs de la biologie, ceux qui lui ont fixé une méthode responsable de l'essor prodigieux qu'elle a connu dans les cent dernières années. On y a ajouté le minimum de commentaires, tout le travail a consisté à les choisir et à les grouper en laissant le plus possbile la parole aux savants. Le choix est fondé sur l'intérêt méthodologique des textes en donnant autant que possible la préférence aux initiateurs. L'ordre de groupement permet d'illustrer les différents aspects de la méthode. Ainsi conçu, ce recueil intéresse le « scientifique » et le

« littéraire ». Au scientifique, il offre une occasion de réfléchir. Il s'agit pour lui d'une sorte de « logique pratique » enseignée par la méthode inductive et permettant d'inférer les grands principes à partir des processus mêmes de la pensée des savants. Au littéraire, il présente une petite « logique illustrée » montrant comment les grands principes logiques du raisonnement inductif enseigné en philosophie trouvent leur application concrète dans les démarches réelles de la recherche biologique. Aux uns et aux autres, il offre l'occasion d'un effort de synthèse faute duquel il n'est pas de véritable culture.

BASSET, A. and MALEVILLE, J., **Les eczémas et leur traitement** (Series: *Pour Le Praticien*), Librairie Maloine S.A., Paris, 1970, 224 pp., figs., 48 F.

Le livre du Professeur A. BASSET et du Professeur agrégé J. MALEVILLE de la Clinique Dermatologique de Strasbourg, a le mérite de considérer l'eczéma sous un angle très pratique, tel qu'il se présente journellement au médecin praticien. Mais il fait aussi une analyse de certaines notions traditionnellement admises et il dresse un bilan des nouvelles acquisitions dans la connaissance de la maladie. Après un rappel anatomo-clinique et étiopathogénique permettant de fixer les limites de cette dermatose de plus en plus fréquente actuellement, les auteurs passent en revue les différents types cliniques d'eczéma et s'efforcent d'en démêler les mécanismes étiologiques pour en déduire des conduites pratiques. Ils discutent d'une façon très précise le problème de la corticothérapie parentérale, des régimes alimentaires, des cures thermales, de la vaccination chez les eczémateux sans négliger l'aspect psychologique et les implications médico-légales. Une importante iconographie en noir et blanc et en couleur illustre les différents types d'eczéma et les principales affections avec lesquelles il ne faut pas les confondre.

BECKER, W. and CONTOPOULOS, G. (eds.), **The Spiral Structure of our Galaxy** (Symposium No. 38 held in Basel, Switzerland, August 29 - September 4, 1969), D. Reidel Publishing Company, Dordrecht, 1970, XIV+478 pp., figs. and tables, Dfl. 80.

This book is an account of the recent observational and theoretical results concerning spiral structure, with the main emphasis on the spiral structure of our Galaxy. Part I deals with spiral structure in other galaxies, where observations are much easier than in our own Galaxy. There is a wealth of spiral forms in other galaxies, and also a continuous transition between regular and barred spirals. Spiral features are observed — clearly in the hydrogen line and the dust, but other optical and radio observations are also very informative. Part II contains many new observational results about the spiral structure of our Galaxy: we have now a much better picture of the distribution of its neutral and ionized hydrogen. From the optical point of view extensive work has been done recently covering the neighborhood of the Sun and many more distant regions. Observations of bright objects, like open clusters and association, give most of the information, but much progress has been made also in the study of later type stars, dust, and particular spiral features. The theoretical developments (Part III) contain many recent results of the gravitational theory of spiral structure. The search of spiral modes and their evolution constitute the main present problems of the linear theory, while non-linear effects are emphasized. Some mechanisms for the generation of spiral waves are suggested. A comparison with barred spirals is made and numerical experiments providing spiral waves are described. The last Part contains various comparisons between theory and observations. The gravitational theory explains many features and provides suggestions for the understanding of various phenomena. Magnetic effects are insufficient in explaining the main features of spiral structure; however, magnetic fields in spiral arms play an important role in e.g. polarization. The book ends with a set of desiderata for future galactic work.

BENIRSCHKE, K. (ed.), **Comparative Aspects of Reproductive Failure** (An International Conference at Dartmouth Medical School, Hanover, N.H., July 25-29, 1966), Springer-Verlag, Berlin, 1967, XXII+486 pp., figs., DM 68.

Scientific discoveries are made at an ever increasing pace and all too frequently, knowledge of these advances remains overlong the private domain of specialists. New

developments in the area of reproduction and, more particularly, of reproductive failure are no exception. It was for these reasons that scientists of a variety of disciplines gathered for a week-long discussion of these advances designed to enhance contact between allied disciplines and to stimulate the use of new research techniques by workers in these fields.

BENNET, Th. P., **Elements of Protein Synthesis** (An Instructional Model plus a Guide to the Instructional Model), W. H. Freeman and Company Ltd., Berkshire, 1969, 36 pp., figs., 28s.

This scientifically accurate model illustrates what is perhaps the most important concept in modern genetics: how the information stored in genes is translated into the specific protein molecules that determine the structure and function of cells. *The Model*: The main part of the model represents a ribosome. Moving parts represent messenger RNAs, transfer RNAs, and amino acids specific for the transfer RNAs. In using the model, the student attaches the moving parts to the representation of the ribosome and systematically performs the sequential steps of protein synthesis, from amino acid activation to the completion of a protein. *The Pamphlet*: The accompanying illustrated pamphlet, containing instructions for using the model, explains biosynthetic processes and the flow of genetic information in enough detail to enable even those who have no background in biochemistry to understand them. The pamphlet also contains review questions that cover the core of contemporary molecular genetics, problems related to the use of the model, and a reading list.

BERTEIN, F., **Bases de l'électronique quantique,** TOME II: **Absorption et émission de champ électromagnétique,** Editions Eyrolles, Paris, 1969, 604 pp., figs. and tables, 190 F.

L'ouvrage actuel fait suite au premier tome « Mécanique Quantique des électrons et atomes » des « Bases de l'Electronique Quantique » du même auteur. Il se propose de développer l'application du formalisme usuel de la Mécanique Quantique à l'étude de l'émission et de l'absorption de champ électromagnétique par la matière. Un tel sujet étant bien entendu extrêmement vaste l'auteur envisage essentiellement, selon l'acception usuelle de l'expression « Electronique Quantique », les propriétés analysables à l'aide de circuits. Il s'agit là d'un domaine de première importance bien qu'en grande partie très récent, puisqu'il mène par l'intermédiaire des masers aux lasers générateurs de lumière. L'ouvrage comporte essentiellement trois parties: La première partie expose les propriétés générales mises en oeuvre par l'émission et l'absorption. Elle expose d'abord le mécanisme des transitions qui décrit de manière dynamique l'interaction de deux objets élémentaires. Elle en montre la mise en oeuvre dans l'interaction d'une particule matérielle et d'un champ électromagnétique, ce dernier étant quantifié ou non. Les observations expérimentales portent sur des objets « macroscopiques » formés d'un grand nombre de particules, aussi convient-il d'exposer les propriétés de tels objets et particulièrement celles des gaz. Ces propriétés s'expriment par des lois statistiques de populations à l'équilibre et également par des descriptions d'états à l'aide de l'opérateur « densité ». La deuxième partie traite de l'émission et de l'absorption de champ par un gaz pris à l'équilibre. L'émission, domaine des opticiens, n'est que brièvement traitée; on insiste essentiellement sur l'absorption des champs de radiofréquences et hyperfréquences. Il s'agit là principalement de l'absorption magnétique des atomes dans les gaz et aussi de celle de noyaux et d'ions dans les cristaux; les développements mènent aux spectroscopies de résonance magnétique nucléaire et électronique, techniques précieuses d'investigation des propriétés de la matière. Les principes des circuits correspondants sont mis en évidence. Les gaz portés en permanence hors d'équilibre à l'aide de méthodes de « pompage » manifestent une émission « stimulée » de champ et c'est à cette émission qu'est consacrée la troisième partie. On étudie tout d'abord les processus de pompage, puis les dispositifs réalisant l'émission stimulée: ce sont pour les hyperfréquences les masers, et pour la lumière les lasers, appareils dont il est superflu de souligner ici l'importance pour les télécommunications. L'auteur exploite largement le formalisme de l'électronique des circuits: du point de vue pédagogique c'est là un précieux élément de liaison et d'unité entre deux domaines apparamment très distants, celui des radiofréquences et celui de la lumière.

BONTE, A., **Introduction à la lecture des cartes géologiques,** 4th ed., Masson & C.ie, Paris, 1969, 278 pp., figs. and plates, 48 F.

La carte géologique, qui figure sous un aspect conventionnel l'état de nos connaissances sur le sous-sol, représente l'intersection avec la surface topographique des formations qui constituent l'écorce terrestre. Lire la carte géologique c'est, à partir de cette représentation en plan, reconstituer dans l'espace et traduire en coupes ou blocs-diagrammes l'allure des couches géologiques. La lecture des cartes géologiques suppose connues certaines notions préliminaires sur les cartes topographiques et sur les conventions admises dans la représentation du sous-sol. Elle suppose également la connaissance, non seulement des différentes dispositions que peuvent affecter les couches géologiques, mais aussi de l'allure que présente l'affleurement de ces structures. Tous ces éléments sont présentés de façon simple et aussi schématique que possible pour permettre au lecteur d'arriver progressivement à une interprétation rapide des cas les plus courants. Quelques indications, nécessairement très sommaires, sur la façon de lever la carte géologique ont été données dans le but de montrer au lecteur non praticien, la grosse part d'interprétation qui entre dans l'élaboration d'une carte géologique. Enfin, pour le géologue appliqué, un chapitre spécial est consacré aux constructions graphiques qui peuvent intervenir dans les levers de précision à grande échelle ou pour l'établissement de prévisions dans les travaux de Génie Civil.

CONTENTS. - *I: Les principes fondamentaux de la Géologie. - II: Notions de tectonique. - III: Les cartes topographiques. - IV: Les cartes géologiques. - V: Principaux types de structure. - VI: La lecture des cartes géologiques. - VII: Le lever des cartes géologiques. - VIII: Constructions graphiques relatives aux cartes géologiques.*

BRANDT, J. C., **Introduction to the Solar Wind,** W. H. Freeman and Company Ltd., Berkshire, 1970, XIV+200 pp., figs. and tables, 94s.

That something besides light comes to the earth from the sun has long been suspected, but only in 1962, when Mariner II made direct measurements in space, was it established beyond doubt that a steady flow of ionized hydrogen particles sweeps out through the solar system. Since that time the solar wind has attracted the attention of increasing numbers of theorists and has been the object of many sophisticated space experiments, including one performed by the first men on the moon. *Introduction to the Solar Wind* is the first systematic introduction to the subject. Written at an intermediate level, it covers all aspects from the stellar structure required to produce an extended atmospheric envelope, or corona, to the impact of the solar wind on the bodies of the solar system and the influence of stellar winds on stellar structure and evolution. All methods of observation are presented, although emphasis is on broad principles, which are treated as an integral and natural part of solar physics and astronomy. The solar wind is important in discussions of topics as widely separated as the formation of ionic comet tails and the theory of general relativity and is therefore a remarkably unifying subject. Because *Introduction to the Solar Wind* treats all of the implications of the phenomenon in detail, it will be useful in a wide variety of courses for upper-division undergraduate and first-year graduate students. Such courses include those on the solar wind, the solar system, solar-terrestrial physics, solar physics, and others in departments of astronomy, physics, and electrical and aerospace engineering. *Introduction to the Solar Wind* is also an excellent introduction to the subject for scientists in other fields and for technical and professional workers in space-related research and engineering.

BROCARD, H. and LEMOYNE, T., **Courbes géométriques remarquables. (Courbes spéciales). Planes & Gauches.** TOME III, Librairie Scientifique et Technique Albert Blanchard, Paris, 1970, 304 pp., 48 F.

Dans ce volume III on a rassemblé une masse de renseignements sur les cubiques comme on ne trouve pas d'ailleurs. On trouvera également dans ce volume sur les courbes unicursales des propriétés découvertes en France avant même que ces courbes eussent reçu nom. Une notice sur les enveloppes résume les propriétés essentielles de celles-ci. La rubrique sur les épicycloïdes indique les propriétés essentielles de ces courbes remarquables.

BUNGE, M., **La causalità** (Il posto del principio causale nella scienza moderna), (Series: *Universale Scientifica*, No. 47-48), Editore Boringhieri, Torino, 1970, 468 pp., figs., L. 2.500.

Inquadrandola nel contesto più esteso del determinismo, Bunge studia in questo libro una categoria posta dalla speculazione filosofica tradizionale a fondamento dell'intera scienza: quella di nesso causale. Termini di riferimento del suo studio sono da un lato le opposte formulazioni dei *causalisti* (da Aristotele a Kant, a Planck, a Meyerson) e degli *acausalisti* (da Hume a Bertrand Russell, ai sostenitori dell'interpretazione « di Copenaghen » della meccanica quantistica, agli empiristi logici), dall'altro gli enunciati della scienza, intesi nella più ampia delle accezioni compatibili con i requisiti di rigore che la definiscono. Il problema della causalità non è quindi formulabile per Bunge negli schemi di un'astratta ricostruzione logica: le interconnessioni del discorso scientifico sono troppo complesse perchè sia possible proiettarle sul piano di una mera analisi formale. Questa va quindi integrata prendendo in esame gli effettivi risultati e metodi della scienza. Soltanto in tal modo potrà porsi il problema dei numerosi livelli che strutturano il discorso scientifico e quello dei salti qualitativi che rendono discontinuo il passaggio dall'uno all'altro di tali livelli. Né diversamente potrà dispiegarsi agli occhi del filosofo l'intero spettro delle categorie della determinazione, quali ad esempio quella statistica, il cui impiego è oggi rilevantissimo, e quella teleologica, usata nello studio degli esseri viventi. La causalità non occupa nel discorso scientifico una posizione privilegiata, né d'altra parte è possibile prescinderne senza rendere la considerazione di tale discorso parziale: la categoria di causa va affiancata ad altre categorie che, a livelli per lo più di complessità maggiore, articolano la nostra ricostruzione concettuale del mondo.

CAMPBELL, J. A., **Chemical Systems** (Energetics, Dynamics, Structure), W. H. Freeman and Company Ltd., Berkshire, 1970, XIV+1112 pp., figs. and tables, 104s.

This college textbook in general chemistry — written by a former director of one of the most influential high school chemistry curriculum projects, CHEM Study — is designed for students of varying preparation who have a strong interest in science. Specifically intended for those who plan to major in science or engineering, *Chemical Systems—Energetics, Dynamics, Structure* provides a through introduction to general chemistry together with enough physical chemistry to serve as background for all further undergraduate science courses that these students may take. It contains the fundamental material needed by students with minimal backgrounds, and, clearly set off in optional sections, a large amount of advanced material that will interest and challenge even the best-prepared students. The book is divided into seven parts, each of which addresses itself to a question that is central to the study of chemistry: 1) What models are useful in describing molecular behavior? 2) What holds molecules together? 3) To what extent do reactions occur? 4) How do reactions occur? 5) Why do net reactions occur? 6) How does structure affect properties? 7) How are chemical reactions used? More than 350 solved exercises illustrate points under discussion, and an additional 700 problems appear at the ends of chapters, grouped according to increasing difficulty.

CARNOT, L., **Réflexions sur la métaphysique du calcul infinitésimal,** Librairie Scientifique et Technique Albert Blanchard, Paris, 1970, XIV+154 pp., figs., 12 F.

CONTENTS. - *I: Principes généraux de l'analyse infinitésimale. - II: De l'algorithme adapté à l'analyse infinitésimale. - III: Des méthodes par lesquelles on peut suppléer à l'analyse infinitésimale.*

CASTAING, R., **Cours de thermodynamique statistique,** Masson & C.ie, Paris, 1970, 346 pp., figs., 48 F.

Une partie importante de ce cours est consacrée à l'introduction, à un niveau élémentaire, et à la discussion des concepts fondamentaux de la Mécanique statistique; elle tend à montrer en quoi cette discipline se distingue essentiellement de la Mécanique « précise » — classique ou quantique —, non seulement par ses méthodes, mais aussi et surtout par ses objectifs. Après quelques rappels sommaires de Mécanique classique, les notions de prédiction statistique, de valeur moyenne et de dispersion sont introduites à partir d'exemples simples tels que le jeu de dés et la « marche au hasard »; la réconci-

liation entre « réversibilité microscopique » et « irréversibilité macroscopique » est discutée en détail dans le cadre de la Mécanique classique. Quelques éléments de Mécanique quantique permettent ensuite d'aborder l'étude générale de la Mécanique statistique; les propriétés « température » et « entropie » en découlent naturellement, ainsi que les « Principes » de la Thermodynamique. Les applications générales de ces Principes, qui sont du ressort de la Thermodynamique classique, ne sont pas abordées dans cet ouvrage, qui traite dans sa dernière partie quelques-unes des applications spécifiques de la Thermodynamique statistique, telles que les équilibres chimiques, les vibrations thermiques des solides ou la théorie électronique des métaux.

CEREIJIDO, M. and ROTUNNO, C. A., **Introduction to the Study of Biological Membranes,** Gordon and Breach Science Publishers, London, 1970, x+262 pp., figs., £10.10.

This concise, easily readable book presents a picture of what membranes are and how they work. It uses a multidisciplined approach in which contributions from enzymology, electronics, crystallography, electronmicroscopy, thermodynamics, general physiology and others are taken into account. The text is supplemented by a bibliography of more than 700 references. Although written primarily for biophysicists, it emphasizes basic concepts and principles of membrane phenomena in a way that can easily be understood by non-specialists and graduate students in any branch of science.

CHAMBADAL, L., **Dictionnaire des mathématiques modernes** (Series: « *Dictionnaires de l'Homme du XX*^{e} *Siècle* »), Librairie Larousse, Paris, 1969, 256 pp., 9,90 F.

Le *Dictionnaire des Mathématiques Modernes* de Lucien Chambadal permettra de mieux connaître et comprendre les mathématiques dites « modernes », en présentant les notions premières, les structures fondamentales, l'équivalence des diverses terminologies en exposant les raisons de leur évolution et en précisant les notations. Chaque concept introduit est suivi d'exemples et de résultats importants. L'ouvrage contient également de nombreuses notices sur les mathématiciens contemporains et des indications sur les grands mathématiciens du passé, au total près d'un millier d'articles. En fin de volume, on trouvera aussi une liste des notations, des abréviations et des symboles les plus courants, ainsi qu'une bibliographie. Le *Dictionnaire des Mathématiques Modernes* sera un guide précieux à tous les niveaux de l'enseignement, et aussi, pour le grand public qui ne peut rester à l'écart de ce grand mouvement de la pensée mathématique contemporaine.

CHERTOK, L. (ed.), **Psychophysiological Mechanisms of Hypnosis** (An International Symposium sponsored by the International Brain Research Organization and the Centre de Recherche, Institut de Psychiatrie, La Rochefoucauld, Paris), Springer-Verlag, Berlin, 1970, VIII+208 pp., figs. and tables, DM 48.

There have been many periods in the history of hypnosis when outstanding members of the scientific community became intrigued and fascinated with the phenomenon, only to lose interest and allow it to become the province of the charlatan and fiction writer. The Second World War provided considerable impetus to the clinical use of hypnosis and since then there has been an enduring revival of both scientific and clinical interest. The participants to the Symposium, met both formally and informally over the five-day period, and were encouraged to formalize and update their manuscripts and to include, when appropriate, some of the material that had emerged from the discussions, thus making available much of the content of the Symposium. These papers will help colleagues in many fields to see the relationships between their own interests and those of others separated by disciplinary or national boundaries.

CHIODI, G. M., **Legge naturale e legge positiva** (Nella filosofia politica di Tommaso Hobbes), Casa Editrice Dott. A. Giuffrè, Milano, 1970, 200 pp., L. 2.200.

CONTENTS. - *I: Il problema del rapporto tra la legge naturale e la legge civile. - II: Un'antropologia Barocca. - III: La struttura del corpo artificiale. - IV: Analisi del rapporto tra le due leggi. - V: Verso i diritti inalienabili. - VI: La dottrina politica hobbesiana sullo sfondo della rivoluzione inglese.*

CLARKE, C. A., **Human Genetics and Medicine** (Series: *Studies in Biology*, No. 20), Edward Arnold (Publishers) Ltd., London, 1970, vi+74 pp., figs., tables and plates, 15s.

Medical genetics may also help us to outwit our inheritance. Diseases do not always manifest themselves even in the presence of the appropriate genetic constitution, and it will become increasingly possible to offset the action of deleterious genes. Nevertheless it is first necessary to understand classical Mendelian genetics in the context of disease. The corner-stone is the single gene type of pedigree in which affected and unaffected individuals segregate like Mendel's peas, but it will soon be appreciated that illness is much more often caused by a subtle interaction between genetic and environmental factors, so that it is obligatory to understand as well something of polygenic inheritance. In the early chapters of this book, the various methods of inheritance are discussed, the first being that of a typical dominant trait.

CLARKE, G. M., **Statistics and Experimental Design** (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1969, xii+162 pp., figs. and tables, 42s.

Statistical methods have been applied more widely and for a longer time in biology than many other branches of science, yet only recently have students in universities and colleges undertaken systematic basic training in statistics while studying biology. Most students of biology, however, have little opportunity to do any mathematical or much numerical work for at least two years before entering higher education, and so the facility for thinking in terms of symbols is undeveloped. This book is based on lectures given to biology students fairly early in an honours degree course, and the subsequent performance of the students suggested that some of the ideas behind, and the limitations of, standard statistical procedures had been imparted as the methods themselves. Accordingly, while the common statistical methods of biology are presented the use and the reasons for them are explained in an 'applied' way. Many of the principles of elementary statistical methods apply quite generally over a wide range of sciences, and the book may be of use to scientists in disciplines other than biology.

COOPER, B. E., **Statistics for Experimentalists,** Pergamon Press Ltd., Oxford, 1969, x+336 pp., figs. and tables, 70s.

The book provides the experimental scientist with a working knowledge of statistical methods, and instills a searching approach to the analysis of data. The book will be invaluable to scientists of many different disciplines who have to interpret the results of their experimental work, and the book's subjects are arranged according to the type of experiment to which they refer rather than to the position they occupy in statistical theory. Thus an experimentalist can follow the analysis of results of a given experiment as a continuous process, uninterrupted by reference elsewhere in the text. The subject coverage, in statistical terms, includes Estimation, Tests of Significance, Power of Tests, Methods of Analysis of Variance and Covariance, Experimental Design, Function Fitting, Count Rate Problems, and Distribution-Free Methods, and there is discussion of the fundamentals of Probability, Distribution and Moments to give a sound basis to the later topics. At the ends of chapters there are sections of problems which are in the nature of practical work, designed to extend the reader's understanding of the text and to enable him to identify the statistical techniques required in different practical situations. Hints and detailed answers to these questions appear at the end of the book.

CORDATI ROSAIA, L. and TORNATORE, L., **Matematica** (Lavoro guidato. Teoria. Scuola media), 3 vols., Loescher Editore, Torino, 1970, xii+336 pp., iv+184 pp. and iv+176 pp., L. 1.600, L. 1.100 and L. 1.100 respectively.

Questo corso di matematica per la scuola media si compone di sei volumi. Tre volumi di « Teoria » e tre volumi di « Esercitazioni » per ogni anno di studio. I volumi di Teoria sono strettamente complementari dei Volumi di Esercitazioni. Le Esercitazioni invece possono essere adottate anche autonomamente.

Contents. - **Vol. I.** I. Aritmetica. *I: Come si scrivono i numeri.* - *II: I numeri naturali.* - *III: Le potenze.* - *IV: Multipli e divisori.* - *V: Le frazioni.* - *VI: Frazioni a confronto.* - *VII: Calcolo con le frazioni.* - II. Geometria. *VIII: Insiemi di punti.* - *IX: Gli angoli.* - *X: Perpendicolarità e parallelismo.* - *XI: Confronto e misura dei segmenti.* -

XII: Le distanze. - XIII: Figure congruenti. - XIV: Figure e trasformazioni. - **Vol. II.** I. Aritmetica. *I: I numeri razionali. - II: Rapporti e proporzioni. - III: Radici quadrate. - IV: Relazioni, funzioni, proporzionalità. - V: Matematica applicata.* - II. Geometria. *VI: Equivalenza ed aree. - VII: La similitudine.* - **Vol. III.** I. Algebra. *I: Numeri relativi. - II: Equazioni e grafici. - III: Matematica applicata.* - II. Geometria. *IV: Il cerchio e i poligoni regolari. - V: Operazioni nel piano. - VI: Figure nello spazio.*

DALRYMPLE, G. B. and LANPHERE, M. A., **Potassium-Argon Dating** (Principles, Techniques and Applications to Geochronology), W. H. Freeman and Company Ltd., Berkshire, 1969, XIV+258 pp., figs. and tables, 70s.

The first chapters of this book review the history and theory of radioactive dating methods and outline the fundamentals of physics and chemistry on which all radioactive clocks rely. These chapters are followed by a discussion of the discovery and distribution of potassium and argon isotopes, by an explanation of the way their interaction can produce a successful geological chronometer, and by a description of the techniques commonly used to measure potassium and argon, with emphasis on means of measuring argon, a topic that is not described thoroughly in other available literature. The remainder of the book is concerned with making the technique work, and deals with some of the problems and practical limitations associated with the method. There are chapters that discuss the problems of accuracy and precision, extraneous argon, and argon loss. One chapter contains a list of minerals that have been found useful in dating, and offers suggestions about preparing samples of them. There are suggestions also of how to evaluate age data, and, finally, examples of the application of potassium-argon dating in several modern research problems. Because this monograph contains a substantial amount of practical information and explains fundamental assumptions that are made whenever a potassium-argon age is calculated, it will be useful to archeologists, anthropologists, and other prehistorians as well as to geologists, chemists, and physicists.

D'ANS-LAX, **Taschenbuch für Chemiker und Physiker,** BAND II: **Organische Verbindungen,** Springer-Verlag, Berlin, 1964, VIII+1178 pp., DM 48.

CONTENTS. - *I: Nomenklatur und Ringbezifferung,* von K. DIMROTH. - *II: Chemische und physikalische Daten organischer Verbindungen,* von K. DIMROTH. - *III: Ergänzung zur Tabelle 12. - IV: Thermochemische Daten organischer Verbindungen. - V: Kritische Daten organischer Verbindungen. - VI: Verzeichnis der Summenformeln der in den Tabellen gebrachten organischen Verbindungen.*

Department of Terrestrial Magnetism (Reprinted from Carnegie Institution Year Book 68 for the year July 1, 1968-June 30, 1969. Issued February 1970), Carnegie Institution, Washington, 1970, 132 pp., figs., tables and plates, No price stated.

CONTENTS. - *I: Introduction. - II: Astrophysics. - III: Biophysics. - IV: Geophysics.* - References Cited. Bibliography. Personnel.

DE PLAEN, J. L., **Aspects dynamiques du métabolisme glucidique chez l'embryon de rat. Action de substances hypoglycémiantes et tératogènes** (Series: « *Médico-Monographies d'Agrégés* »), Editions Arscia S.A., Bruxelles, 1970, 250 pp., figs. and tables, 500 F.B.

CONTENTS. - I. Métabolisme glucidique de l'embryon isolé de rat au cours de l'organogenèse. *I: Composition glucidique et azotée de l'embryon de rat. - II: Respiration embryonnaire. - III: Métabolisme glucidique et lipidique de l'embryon. - IV: Métabolisme embryonnaire: renseignements obtenus par l'emploi de radioisotopes.* - II. Influence de substances hypoglycémiantes et tératogènes sur le métabolisme maternel et embryonnaire. *I: Influence des substances hypoglycémiantes sur la glycémie de la rate adulte. - II: Influence de traitements hypoglycémiants sur la composition embryonnaire. - III: Influence de substances hypoglycémiantes sur la respiration embryonnaire. - IV: Influence de substances hypoglycémiantes sur le métabolisme embryonnaire du glycogène, du glucose et de l'acide lactique. - V: Influence de substances hypoglycémiantes sur le métabolisme embryonnaire: renseignements obtenus par l'emploi de radioisotopes.*

DI AICHELBURG, U., **L'Uomo,** 2nd rev. ed., Unione Tipografico-Editrice Torinese, Torino, 1970, XVI+548 pp., figs. and tables, L. 10.000.

Quest'opera, che si sviluppa secondo un piano logico ed armonico permettendo così la comprensione dei complessi fenomeni che regolano la vita dell'uomo, è stata aggiornata dall'Autore sulla base delle più recenti acquisizioni che hanno informato il campo della biologia, specialmente per quanto riguarda la genetica, gli enzimi, la lotta contro le principali malattie, i trapianti di organi e le altre conquiste della scienza medica attuale. L'Autore ci ha dato con questo libro una sintesi delle conoscenze positive che abbiamo sull'uomo, accessibile a qualsiasi persona colta. I precedenti storici dell'uomo come tale, cioè l'origine della sua specie e i comportamenti superiori o simbolici dell'uomo, sono trattati nei due ultimi capitoli.

CONTENTS. - *I: Il sistema nervoso. - II: Le sensazioni. - III: L'apparato circolatorio. - IV: L'apparato respiratorio. - V: Le ghiandole endocrine e gli ormoni. - VI: Il ricambio. - VII: La genetica. - VIII: Le età dell'uomo. - IX: L'origine della specie umana. - X: I comportamenti simbolici e sociali dell'uomo.*

DUMOULIN, J. and DE BISSCHOP, G., **La galvanothérapie,** Librairie Maloine S.A., Paris, 1970, 160 pp., figs., 20 F.

Il est assez paradoxal en 1970 d'écrire un livre sur la galvanothérapie, vieille méthode électrologique quelque peu délaissée depuis quelques années. Les auteurs imprégnés des méthodes d'électrologie de la Salpêtrière ont voulu montrer que si la médecine avait fait un immense pas en avant avec les nouveaux médicaments, sulfamides, antibiotiques, antalgiques, cette discipline électrique avait gardé toute sa valeur en 1970. Cette méthode peut, à condition d'en connaître parfaitement les applications et le maniement, rendre de grands services aux malades principalement dans les nombreuses séquelles de traumatismes inhérents aux accidents de notre vie moderne. S'inspirant des travaux de Duchenne de Boulogne, Remak, Erb, d'Arsonval, Lapicque, Bourguignon et Humbert, ce livre a pour but de démontrer que la galvanothérapie appliquée sur la base de solides connaissances neurophysiologiques, contribue très souvent à la guérison. L'ouvrage est divisé en deux parties. La première est un rappel des bases générales de la méthode selon les conceptions bien codifiées de l'électrologie française. Dans la seconde partie, les applications de la galvanothérapie sont reprises pour chaque grande spécialité médicale. Ce livre est complété par une bibliographie et un index alphabétique permettant de retrouver aisément pour chaque affection les modalités thérapeutiques. Ce livre s'adresse aux électrologistes, aux rhumatologues, aux neurologues et à tous ceux qui s'occupent de réadaptation.

EINSTEIN, A., **La théorie de la relativité restreinte et générale** (Exposé élémentaire. La relativité et le problème de l'espace), Gauthier-Villars, Paris, 1969, VI+180 pp., 19 F.

Dans cet ouvrage qui a pour but de faire connaître, d'une manière aussi exacte que possible, la Théorie de la relativité, A. Einstein expose d'une façon concise et lumineuse l'ensemble des découvertes qui constituent la Théorie de la relativité, laquelle marque la révolution la plus grande dans l'histoire de la physique. Toutes les découvertes extraordinaires, qui ont vivement frappé les esprits et valu à leur auteur l'admiration universelle, sont décrites dans un langage simple et convaincant, où l'appareil mathématique est réduit à quelques formules algébriques élémentaires. La lecture de ce livre suppose à peu près des connaissances de bachelier et permettra à tout esprit cultivé de prendre connaissance des conceptions originales et hardies d'un des plus grands génies de notre temps.

CONTENTS. - *I: Théorie de la relativité restreinte. - II: La théorie de la relativité générale. - III: Reflexions sur l'univers considéré comme un tout.*

FIRSOFF, V. A., **Vie, intelligence et galaxies** (Series: *Science-Poche*), Dunod, Paris, 1970, VIII+134 pp., figs., tables and plates, 8,70 F.

Cet ouvrage tente de répondre de façon claire et originale à ces questions, sans pour cela sacrifier la rigueur scientifique des hypothèses ou des démonstrations et sans tomber dans la science-fiction. L'auteur y présente en particulier une théorie personnelle sur la nature de l'intelligence. En affirmant que celle-ci est une partie essentielle de la réalité physique, une entité de même ordre que l'énergie et la matière, il propose une discussion passionnante sur les relations possibles entre la physique sub-atomique, la conscience et le libre arbitre.

FLORKIN, M. and STOTZ, E. H. (eds.), **Comprehensive Biochemistry,** Vol. 18: **Lipid Metabolism,** Elsevier Publishing Company, Amsterdam, 1970, xvi+398 pp., figs. and tables, Dfl. 75.

Lipid Metabolism poses special problems to the organism in that lipids are sparingly soluble in water. These problems are clearly evident in the contents of Chapter I on Assimilation, Distribution and Storage. This chapter is composed of three parts, the first by John M. Johnston on Intestinal Absorption of Fats, dealing principally with neutral fats, but also with phospholipids and cholesterol. The second part concerns the Mobilization, Transport and Utilization of Free Fatty Acids (Scow and Chernick), particularly the effect of fasting, diabetes, and several hormones on free fatty acid metabolism. The third part by D. S. Robinson (Oxford) is entitled the Function of the Plasma Triglycerides in Fatty Acid Transport, and offers a quite extensive treatment of plasma lipoproteins and their variations in health and disease. Chapter II (Marinetti) reviews mechanisms of the Biosynthesis of Triglycerides in liver, adipose tissue, and intestine and the control processes involved. In succeeding chapters the complexities of Phospholipid Metabolism are unravelled by Thompson, and of the specialized Gangliosides by Svennerholm. The biosynthesis and nature of the unique lipids found in bacteria are described by O'Leary (Cornell) in the chapter entitled Bacterial Lipid Metabolism. The special problems and types of lipids found in nervous tissue are discussed by A. N. Davison. In the final chapter of the volume Bressler (Duke) presents the topic Fatty Acid Oxidation and Carnitine. Missing from the volume, but to be published later, is the chapter on Fatty Acid Oxidation and Synthesis, to be contributed by Wakil.

FRANÇON, M., **Physique CB-BG** (Première année), 2nd rev. ed., Masson et C.ie, Paris, 1969, 334 pp., figs., 39 F.

Ce cours de physique est destiné aux étudiants du premier cycle Chimie-biologie et Biologie-géologie (CB, BG). Toutefois il peut être consulté par les étudiants du certificat préparatoire aux études médicales (C.P.E.M.) et par tous les étudiants qui ont besoin d'acquérir quelques connaissances en physique sans que cette science soit leur spécialité. Au lieu d'exposer les phénomènes d'interférences à l'aide de l'expérience de Young, comme on le fait traditionnellement, l'auteur a préféré prendre l'exemple de l'interféromètre de Michelson. Les explications sont tout aussi simples et on a les éléments nécessaires pour la description de l'expérience Michelson-Morlay dans le chapitre sur la relativité. Les phénomènes de diffraction ont été présentés de façon que l'étudiant puisse comprendre le fonctionnement du microscope optique sous l'aspect « optique physique ». La comparaison entre le microscope optique et le microscope électronique en est ainsi rendue plus simple. Comme le programme le prévoit, quelques notions ont été introduites sur les électrons dans les cristaux et il a paru plus utile de présenter les montages concernant l'amplification et la production d'oscillations, à l'aide des transistors, en abandonnant les tubes électroniques. Certaines questions qui débordent le programme, comme par exemple l'holographie ou le laser, ont été esquissées; car l'auteur n'a pas voulu rédiger un cours de physique sans indiquer ici et là des réalisations modernes susceptibles d'intéresser l'étudiant. En raison de l'importance de la microscopie notamment pour les biologistes, les géologues et les médecins, l'auteur a donné quelques développements sur les principales techniques microscopiques à la fin de cette deuxième édition.

FRÉDÉRIQUE, **Les enfants et la mathématique,** Vol. 1, Marcel Didier, Bruxelles, 1970, xxii+350 pp., 280 F.B.

La forme du présent ouvrage est largement influencée par les conclusions que l'on a pu tirer des cours de formation continue des enseignants organisés par le Centre Belge de Pédagogie de la Mathématique: Les situations qui conviennent à l'enseignement aux élèves sont les meilleures pour la formation de leurs maîtres. — Ne proposer aux professeurs en exercice que des matières directement utilisables dans leur propre enseignement. — Inculquer en même temps la mathématique nouvelle et la pédagogie de son enseignement. — Le présent document relate les grands moments de cet enseignement de manière aussi vivante que possible. Il décrit les situations pédagogiques originales utilisées dans 130 leçons et laisse au lecteur le soin d'imaginer en toute liberté un déroulement possible des leçons de routine intermédiaires. Les enfants de six ans ne s'intéressent pas volontiers au même object pendant plusieurs jours consécutifs. Dans la classe, il est nécessaire d'alterner les sujets. Afin de faciliter la lecture, le présent ouvrage s'écarte de l'ordre chronologique et procède à des groupements par thèmes. Les leçons évoquées sont datées da manière à permettre au lecteur de s'y retrouver et d'emporter de l'expérience une vue aussi fidèle que possible. L'analyse ci-dessous la rétablit dans sa chronologie.

GAGNAIRE, D., JAULMES, P. and BUGAREL, R., **Extraction et purification des espèces chimiques** (Series: « *Monographies de Chimie Organique* », Vol. V), Masson et C.ie, Paris, 1969, 560 pp., figs. and tables, 170 F.

Cet ouvrage est consacré aux procédés d'analyse immédiate. Un aspect de ce problème avait fait l'objet des volumes II et III des Monographies de Chimie organique, qui exposent de façon approfondie les méthodes chromatographiques. Ce nouveau volume insiste moins sur les applications analytiques, mais davantage sur les techniques de séparation, quelle que soit l'échelle des opérations. C'est dire que cette étude ne se limite pas aux opérations de laboratoire, mais aborde les procédés utilisés dans les ateliers de fabrication. Les applications de ces méthodes, souvent très générales, ne sauraient d'ailleurs pas se limiter aux produits organiques, mais s'étendent à tout le domaine de la chimie. L'ouvrage examine les différents procédés, traités à la fois par leur justification théorique et par leur aspect technique. Il fournit ainsi des documents précieux à tous ceux qui ont à affronter des difficultés expérimentales dans l'isolement des espèces chimiques pures. En même temps il forme, par ses développements théoriques, une base solide pour un examen efficace de l'évolution future que pourraient subir les différentes méthodes. L'ensemble comporte trois parties. La première traite les procédés de séparation en milieu homogène: - la diffusion thermique dans les gaz, extraction liquide-liquide (méthode de Craig), - la cristallisation, y compris la fusion de zone. La deuxième partie est consacrée à une étude théorique et technique de la distillation. Elle aborde la discussion des opérations continues et discontinues, ainsi que celle de l'azéotropisme. Elle décrit les colonnes à distiller, utilisables au laboratoire, et les appareils industriels. Enfin, la troisième partie aborde les séparations hétérogènes, telles que la filtration, l'extraction des solides par les solvants et la séparation électrostatique.

CONTENTS. - *I: Les méthodes de séparation des mélanges homogènes*, par D. GAGNAIRE. - *II: Les méthodes de séparation par diffusion*, par D. GAGNAIRE. - *III: Extraction liquide-liquide*, par G. GAGNAIRE. - *IV: Cristallisation*, par D. GAGNAIRE. - *V: Distillation*, par P. JAULMES. - *VI: Séparation des mélanges hétérogènes*, par R. BUGAREL.

GEYMONAT, L., **Storia del pensiero filosofico e scientifico**, Garzanti Editore, Milano, 1970/71, VOL. I: **L'antichità. Il Medioevo**, 736 pp., figs., VOL. II: **Il Cinquecento. Il Seicento**, 706 pp., figs., VOL. III: **Il Settecento**, 720 pp., figs.

Questa storia del pensiero filosofico e scientifico è stata progettata secondo un preciso impianto storico critico, che tiene conto dei risultati più persuasivi raggiunti dalla storiografia filosofica. Lo sviluppo della filosofia non è esposto e valutato come qualcosa di autonomo rispetto a quello delle scienze; è noto che i grandi dibattiti filosofici e scientifici si sono sempre condizionati a vicenda, per cui la loro piena comprensione è possibile solo se si ha presente il quadro unitario da essi costituito. Altrettanto importante risulta il costante collegamento con la struttura economico-sociale in cui tali indirizzi sorsero e si affermarono. Questo nesso permette di porre nella giusta dimensione soprattutto i problemi dell'educazione e della scuola che hanno sempre avuto un particolare peso nei dibattiti culturali. Sulla base di questa impostazione, l'opera si configura come il risultato scientifico e culturale più importante di tutta l'attività di Ludovico Geymonat che fin dagli anni trenta — con i primi studi di filosofia della scienza — ha sempre sostenuto un'operante unità del sapere filosofico e scientifico.

Grande Dizionario Enciclopedico U.T.E.T., 3rd rev. ed., Unione Tipografico-Editrice Torinese, Torino, 1966, 1967 e 1968, **Vol. I: A-Apo**, XXVIII+868 pp., figs. and tables, **Vol. II: App-Beq**, XX+912 pp., figs. and tables, **Vol. III: Ber-Caq**, XX+868 pp., figs. and tables, **Vol. IV: Car-Cle**, XXIV+874 pp., figs. and tables, **Vol. V: Cli-Dan,**

XXIV+902 pp., figs. and tables, **Vol. VI: Dao-El**, XXIV+864 pp., figs. and tables, L. 18.000 each volume.

Il Grande Dizionario Enciclopedico ha una sua particolare impostazione chiaramente delineata ed un disegno organico costantemente presente. Riproduce tutta la multiforme e vastissima materia prescritta dai programmi scolastici col metodo indicato dai programmi stessi nei vari gradi e ordini di scuole, classiche, scientifiche, tecniche, industriali, commerciali, nautiche, militari, ecc. Nello stesso tempo si è fatto spazio a una nuova serie di notizie su argomenti che pur non formando parte dei programmi delle scuole secondarie integrano questi e rientrano nel patrimonio culturale di ogni persona di media cultura. Così il lettore troverà numerose ed esaurienti voci relative alla medicina, alla psicologia, al diritto, alle scienze politiche e sociali, alla cinematografia, allo sport, al giornalismo e all'abbigliamento, ecc. Grandemente esteso, fino ad includervi tutti i capolavori della letteratura mondiale, è anche il numero dei riassunti delle opere letterarie sino a dare un panorama completo della letteratura narrativa. Numerosi sono anche i riassunti dei libretti delle opere musicali più note e dei film più significativi, data la crescente diffusione della cultura musicale e cinematografica. Ricca e stimolante è infine anche la citazione di numerosi personaggi contemporanei, in merito ai quali tuttavia estremamente cauto e obiettivamente documentato è ogni apprezzamento critico. Il numero dei collaboratori, accuratamente selezionati, è ampio e differenziato, e le branche del sapere a ciascuno di essi affidate sono rigorosamente tipizzate. Ciascuna voce, a parte i contributi redazionali, è opera di uno studioso in quell'argomento particolarmente versato, il quale, senza rinunciare a nulla in fatto di precisione scientifica, ha sempre cercato di essere agevolmente compreso anche da chi affronta per la prima volta la materia trattata. Particolarmente curata appare poi la struttura interna delle singole voci, trattate talvolta da più autori in relazione al significato che il termine ha nelle singole discipline, secondo uno sviluppo logico, fatto evidente dalla disposizione tipografica delle diverse parti, tale da invitare a una lettura distesa dell'intera voce. Ogni materia è stata suddivisa secondo criteri rigorosamente uniformi. È infine da rilevare il fatto che accanto a voci brevi di definizioni e di sintesi si trovano articoli di più largo orizzonte e di più diffusa trattazione, strettamente connessi tra loro attraverso una fitta rete di rimandi, per offrire al lettore la possibilità di integrare una notizia con quelle che la riprendono e la sviluppano.

GRASSÉ, P.-P. (ed.), **Traité de Zoologie. Anatomie, Systématique, Biologie,** VOL. XIV: **Reptiles. Caractères généraux et anatomie,** ISSUE II, Masson & C.ie, Paris, 1970, 712 pp., figs. and plates, 225 F.

Le Traité de Zoologie poursuit sa marche vers son achèvement. Aujourd'hui paraît le fascicule 2 du tome XIV, consacré à l'étude des Reptiles. Cette classe a joué un rôle capital dans l'histoire des Vertébrés, c'est d'elle que sont issus, indépendamment les uns des autres, Oiseaux et Mammifères. Leurs formes primitives riches en potentialités évolutives sont d'un intérêt exceptionnel. Aucun zoologiste « vertébriste » ne peut se désintéresser d'elles. Le présent volume concerne l'anatomie. Les pages réservées à la diagnose de la classe, à l'étude du squelette, sont écrites par le Professeur Jean GUIBÉ du Muséum national d'Histoire naturelle. Dans un style clair, il expose la structure du squelette des Reptiles et en dégage les grandes caractéristiques; une part prépondérante est accordée à l'anatomie du crâne. Musculature et locomotion font l'objet de chapitres qui donnent une bonne mise au point de nos connaissances sur ces deux sujets. Le chapitre relatif au système nerveux central a été rédigé par le Professeur Jean ANTHONY du Muséum national d'Histoire naturelle. Cette étude exhaustive laisse loin derrière elle toutes les revues faites jusqu'ici sur l'anatomie de l'encéphale reptilien. Il s'en dégage une vision très claire des particularités fondamentales de ce système nerveux. Le Professeur J. GUIBÉ a traité des organes sensoriels, à l'exception de l'oeil, chapitre rédigé par le regretté A. ROCHON-DUVIGNEAUD et mis à jour par Pierre-P. GRASSÉ. Les chapitres qui traitent des appareils circulatoire, respiratoire et digestif ont pour auteur le Professeur Jean GUIBÉ; le Docteur R. DUGUY, directeur du Muséum national d'Histoire naturelle de La Rochelle, a rédigé les pages consacrées à l'étude du sang. Le si important chapitre qui traite des venins et de l'envenimation est l'oeuvre du Dr Paul BOQUET, chef de service à l'Institut Pasteur. Il tient le plus grand compte des tout derniers progrès accomplis par nos connaissances dans ce domaine. Quant à l'appareil venimeux, il est décrit et étudié par le Professeur J. ANTHONY. Ce premier volume consacré aux Reptiles est richement illustré et continue brillamment la tradition du TRAITÉ DE ZOOLOGIE. Il sera suivi sous peu d'un second volume.

HAAGE, W., **Guide des Cactus** (Series: « *Les Guides du Naturaliste* »), Delachaux & Niestlé, Neuchâtel, 1970, 280 pp., figs., Frs. 35.

Dans ce guide richement illustré de 220 photographies en couleurs et de 658 dessins dus à la plume de Kurt Schulze, près de 600 plantes sont décrites et analysées par Walther Haage, membre de l'Organisation internationale de Recherches sur les Plantes succulentes. Chaque plante est représentée dans la marge en face de la description qui lui correspond. Un grand nombre d'entre elles sont également représentées, en fleurs, dans les photographies en couleurs qui illustrent ce remarquable ouvrage. La première partie du guide traite des méthodes à employer pour acclimater et soigner vos cactus, avec des conseils sur: — La croissance, en fonction de la lumière, de l'air, de la terre, de la nourriture et de l'eau. — Les soins à donner suivant les saisons. — Les méthodes de culture. — Le bouturage et la greffe. — Les ennemis et les maladies. De nombreux dessins et tableaux explicatifs enrichissent encore ce texte d'une lecture agréable. Un index analytique, placé en fin de volume, donne pour chaque plante, dont le nom est indiqué en latin, sa référence française, ainsi que les renvois aux pages du texte et, lorsque le cas se présente, sa référence aux illustrations.

HALL, V. E., GIESE, A. C. and SONNENSCHEIN, R. R. (eds.), **Annual Review of Physiology,** VOL. 32, Annual Reviews, Inc., Palo Alto (California), 1970, x+636 pp., figs. and tables, $10.00.

CONTENTS. - *I: The Road a Scientist Followed:* Notes of Japanese Physiology as I Myself Experienced it, by GEN-ICHI KATO. - *II: Transport Across Cell Membranes,* by R. WHITTAM and K. P. WHEELER. - *III: Endocrine Regulation of Calcium Metabolism,* by D. H. COPP. - *IV: Skeletal Muscle,* by A. SANDOW. - *V: Digestion: Intestinal Secretion,* by T. R. HENDRIX and T. M. BAYLESS. - *VI: CNS at the Cellular Level: Identity of Transmitter Agents,* by C. HEBB. - *VII: The Functional Organization of Invertebrate Ganglia,* by E. R. KANDEL and I. KUPFERMANN. - *VIII: Central Pathways of Vision,* by G. S. BRINDLEY. - *IX: Pain,* by R. K. S. LIM. - *X: Overall Regulation of the Circulation,* by J. A. HERD. - *XI: Systemic Circulation: Local Control,* by S. MELLANDER. - *XII: Comparative Physiology: Chromatophores,* by M. FINGERMAN. - *XIII: Heart,* by M. N. LEVY and R. M. BERNE. - *XIV: Respiration,* by R. A. MITCHELL. - *XV: Reproduction,* by D. T. ARMSTRONG. - *XVI: Comparative Physiology: Electric Organs,* by M. V. L. BENNETT. - *XVII: The Lymphoid System,* by R. A. GATTI, O. STUTMAN and R. A. GOOD. - *XVIII: The Regulation of Extracellular Fluid Volume,* by O. H. GAUER, J. P. HENRY and C. BEHN.

HARDEN JONES, F. R., **Fish Migration,** Edward Arnold (Publishers) Ltd., London, 1968, VIII+326 pp., figs. and tables, £6.

No general account of fish migration has been published in the English language since the appearance of Alexander Meek's book in 1916. In *Fish Migration* Dr. Harden Jones has summarized the evidence relating to homing and migration in salmon, the eel, herring, cod, and plaice. The sensory channels and behavioural mechanisms involved in homing and migration are considered in the light of the available data which are critically reviewed. There are chapters dealing with biological aspects of fish migration and with the methods and techniques used in their study. The work is superbly illustrated with numerous detailed maps, charts and diagrams, many of which are taken from British Admiralty, Meteorological Office or Ordonance Survey maps. Its appeal will be to an audience of graduate students and research workers in universities and institutions of zoology, hydrography and marine biology etc., throughout the world.

CONTENTS. - *I: Introduction. - II: Biological Aspects of Migration. - III: Methods of Studying Fish Migrations. - IV: Salmon and Trout. - V: The Eel. - VI: The Herring. - VII: The Cod. - VIII: The Plaice. - IX: A Recapitulation. - X: The Reactions of Fish to Stimuli. - XI: Movement without Reference to Local Landmarks. - XII: Homing and the Use of Local Landmarks: Pilotage. - XIII: Epilogue.*

HARMUTH, H. F., **Transmission of Information by Orthogonal Functions,** 2nd rev. ed., Springer-Verlag, Berlin, 1970, XII+326 pp., figs. and tables, DM 40.

The orthogonality of functions has been exploited in communications since its very beginning. Conscious and extensive use was made of it by KOTEL'NIKOV in theoretical work in 1947. Ten years later a considerable number of people were working in this field rather independently. However, little experimental use could be made of the theoretical results before the arrival of solid state operational amplifiers and integrated circuits. The advance of semiconductor technology produced the first really new, useful example of orthogonal functions: the little-known Walsh functions. In this book emphasis is placed on the Walsh functions, since ample literature is available on sine-cosine functions as well as on block pulses and pulses derived from them.

HELFRICH, H. W. Jr, (ed.), **The Environmental Crisis** (Man's Struggle to Live with Himself), Yale University Press, London, 1970, x+188 pp., 18s.

The combined scourges of water and air pollution, mismanaged national resources and open spaces, increasing and unplanned populations, urban congestion, and mismanagement of our national energies are creating human despair in the midst of unprecedented technological advances. The papers in this book, representing the Yale School of Forestry's 1968-69 lecture series, deal with our survival in the face of the rapid deterioration of our natural surroundings. The contributors, distinguished authorities in the fields of science, law, regional planning, economics, and government, examine the ecological and ethical problems plaguing mankind. Focusing attention on the critical environmental crisis, these papers are intended to stimulate a search for practical solutions.

HILL, A. V., **First and Last Experiments in Muscle Mechanics,** Cambridge University Press, London, 1970, XVI+142 pp., figs. and tables, 55s.

A highly personal account of a life-time's work in muscle physiology: the experiments made and the results obtained are described with a refreshing zest and enthusiasm and a great deal of speculative and provocative comment. Hill's classical paper with H. S. Gasser in 1924 on the dynamics of muscular contraction forms the first chapter. The chronicle of the experiments includes puzzling things (many to be resolved later) as well as the more easily understandable results, and recalls both the excitements and the disappointments. An analysis is made of muscle physiology by considering the force-velocity relation, the speed of fibre contractions, the development of the active state, the elastic energy in a contracting muscle, the tension-extension curve and the internal redistribution of length. The book concludes with the address given by A. V. Hill at the International Congress of Physiology in Tokyo in 1965, in which the tradition and personalities of earlier congresses are vividly and amusingly described, and references are made to the relevance of muscle physiology in human endeavour. This account of muscle mechanics will appeal not only to physiologists and biophysicists but also to people interested in physical education and athletics.

HUSSON, R., **Glossaire de biologie animale,** 2nd rev. ed., Gauthier-Villars, Paris, 1970, VIII+300 pp., 35 F.

Le grand développement pris à l'époque contemporaine par les Sciences biologiques a amené celles-ci à faire de plus en plus largement usage d'une terminologie qui, à première vue, peut paraître rébarbative à celui qui veut s'initier aux grands problèmes de la Biologie et, de ce fait, risque d'être rebuté ou dérouté par un vocabulaire technique particulier. L'auteur a réuni dans ce glossaire les termes spéciaux les plus couramment utilisés, glossaire auquel le non-spécialiste aussi bien que l'étudiant pourra se reporter pour avoir, non seulement une orthographe exacte, mais aussi une définition et une explication précises, ainsi que la racine étymologique qui, bien souvent, éclaire la signification du vocabulaire et en justifie l'emploi. L'auteur n'a fait figurer dans ce glossaire que les termes biologiques, ayant laissé de côté tous ceux qui ne relèvent que de la pure description zoologique; de plus, il a allégé le plus possible: c'est ainsi qu'ayant défini «chromosome», les adjectifs en dérivant comme chromosomique ou chromosomien n'y furent pas inclus. Le but de ce glossaire est avant tout d'être utile aux étudiants en Sciences Naturelles des Facultés des Sciences, aux étudiants en Médecine et Pharmacie, aux étudiants qui se destinent aux carrières agronomiques, en un mot à tous ceux qui, de près ou de loin,

s'intéressent aux Sciences Biologiques. Il a été tenu compte pour la bibliographie de cette deuxième édition de la plus récente littérature spécialisée jusqu'à juillet 1969 inclus.

JACQUARD, A., **Structures génétiques des populations,** Masson & C.ie, Paris, 1970, 400 pp., figs. and tables, 80 F.

Rédigé essentiellement à l'intention des étudiants, cet ouvrage s'adresse également aux sociologues, anthropologues, démographes, médecins qui veulent assimiler les concepts de la génétique pour enrichir ceux de leur propre domaine, ou qui désirent mieux comprendre certaines affirmations exprimées par le généticien en un langage inutilement ésotérique. Les fondements logiques et biologiques de la génétique sont décrits dans une première partie. Les concepts, la terminologie et les notations de la génétique des populations (et notamment les « structures » géniques ou génotypiques) sont précisés avant d'aborder dans une seconde partie le modèle de référence constamment utilisé par les généticiens: la panmixie, qui décrit les équilibres auxquels parvient une population idéale sur laquelle n'agirait aucune cause d'évolution. Enfin, une troisième partie étudie successivement les conséquences des divers écarts au modèle panmictique: choix du conjoint, effets sélectifs, mutations, migrations, dérive au hasard, et montre quels sont les apports de la génétique des populations à la compréhension des problèmes posés par le racisme, l'eugénisme, l'évolution.

JERSILD, A. T., **Psicologia dell'adolescenza,** Editrice La Scuola, Brescia, 1970, 482 pp., figs. and tables, L. 4.500.

La ricerca di precise categorie scientifiche in cui inquadrare i pensieri dell'umanità sulla gioventù conduce ad almeno tre conclusioni. La prima: si trovano alcune categorie. La seconda: si nota come sia ben difficile parlare di « precise » categorie, data la sovrapposizione tra problemi e tra modi differenti di vedere o di osservare gli stessi eventi. La terza: risulta inequivocabilmente l'insufficienza della categoria scientifica per esaurire il grande problema umano della gioventù. Ma in questa sede è opportuno sottolineare ciò che alla scienza riesce, non ciò che essa dimostra di non poter concludere. È in questa serie di considerazioni che viene presentato in traduzione italiana un trattato sull'adolescenza che in America ha avuto la più ampia diffusione. Arthur Thomas Jersild è una personalità eminente nel campo della psicologia e dell'educazione americana. Autore di numerosi libri e contributi scientifici, egli è ormai un maestro riconosciuto della psicologia genetica od evolutiva. Il Jersild non dà mai o quasi mai, interpretazioni dello sviluppo giovanile sulla base di discordanti teorie di psicologia generale: propone un tema iniziale e poi lo svolge. All'inizio il tema può esser inteso come un'ipotesi di lavoro ed alla fine può venir considerato come una tesi dimostrata, se gli argomenti proposti vengono ritenuti sufficientemente capaci di reggere la dimostrazione. È appunto questo tema centrale che, sul piano psicologico, diventa essenziale per l'interpretazione della posizione scientifica del Jersild. Tutti i capitoli del libro sono incentrati su un'idea che li giustifica e dai quali richiede a sua volta giustificazione, ed è l'idea di « self ». Il self, o sè, « è la somma totale delle idee e delle disposizioni spirituali di una persona riguardo a chi e che cosa essa è ». Ma se questa è la posizione scientifica elementare, l'Autore la completa con un'altra rilevazione: « all'interno di ciascuna singola personalità c'è alcunché di durevole, che permane attraverso il tempo ».

KEATTCH, C. J., **An Introduction to Thermogravimetry,** Heyden & Son Ltd., London, 1969, x+60 pp., figs., £1.18.0.

The importance of thermogravimetry in research and routine control is rapidly increasing. However, there are few authoritative texbooks available which truly serve as an introduction to the subject. Written by an acknowledged expert in this field, most of the examples in the book are from the author's own practical experiences. Emphasis is correctly placed on the care needed in interpreting thermogravimetry curves and the book amply illustrates the wide range of applications in inorganic, organic and polymer chemistry as well as in the fields of minerals and the applied sciences.

CONTENTS. - *I: Origins.* - *II: Thermobalances.* - *III: Thermogravimetric Data.* -

IV: Interpretation of Data. - V: Applications-I (Inorganic Chemistry). - VI: Applications-II (Organic and Polymer Chemistry). - VII: Applications-III (Minerals and Applied Sciences).

KERVRAN, C. L., **Transmutations biologiques en agronomie** (Des exemples de travaux pratiques pour laboratoires), Librairie Maloine S.A., Paris, 1970, 118 pp., figs., 19 F.

Ce livre reproduit les conférences faites à Paris à l'Institut National Agronomique. C'est dire qu'il a surtout retenu des exemples applicables à l'agriculture. De ce fait, il sera utile à tous les étudiants des écoles supérieures agronomiques et aux enseignants des écoles d'agriculture de tous niveaux. Ce sera un livre de base pour tous les étudiants où la biologie est la discipline principale; ce sera pour eux un guide dans la réalisation d'expériences mettant en oeuvre la transmutation des éléments par la matière vivante. Il aidera l'agriculture à sortir de ce cercle vicieux de la pollution sans cesse accrue, conséquence d'un principe faux, adopté au siècle dernier, celui de la Restitution de Leibig, largement remplacé maintenant par la loi scientifique de la Substitution, formulée par l'auteur comme application de cette acquisition récente: la physique nucléaire biologique. Il sera lu avec profit de tous ceux qui sont soucieux de comprendre comment se nourrir efficacement, comment éviter certaines carences, chez l'homme ou l'animal.

KIRSCHENMANN, P. P., **Information and Reflection** (On some Problems of Cybernetics and how Contemporary Dialectical Materialism Copes with Them), D. Reidel Publishing Company, Dordrecht, 1970, xvi+226 pp., Dfl. 46.

This study focuses upon one of the major controversial issues in the philosophical interpretations of contemporary science. An attempt which is original in its systematic breadth and realistic in viewpoint is made at clarifying the complex problems of information which were engendered by cybernetics and communication technology. A special aspect of process or determinations within reality is here pointed to as the basis of one kind of study in this field. At the same time the book offers the most recent and complete exposition and a critical analysis of how Marxist-Leninist philosophers try to cope with these problems. For this purpose it also provides a novel summary of Dialectical Materialism as the basis for its treatment of problems of information. On the other hand, it indicates some of the directions in which Marxism-Leninism is developing. Although the book is mainly systematic rather than historical in character, most of the particular issues are given some historical perspective. The book was written for students of philosophy, in particular philosophy of science and Marxist-Leninist philosophy. Nobody, whether professionally or non-professionally interested in the philosophical aspects of cybernetics, the problems of information or recent developments of Dialectical Materialism and its treatment of Western ideas, should fail to consult this book. The historian of science will find ample material about the origin and fate of newly developing fields and their implications.

KOPAL, Z., **Widening Horizons** (Man's Quest to Understand the Structure of the Universe), Stanmore Press, London, 1970, 176 pp., figs., 30s.

The aim of this volume is to outline for the general reader the main landmarks of the endless course of Man's quest to recognise his position in the Universe — landmarks consisting of astronomical observations and the reasoning which can be based on them. From the realization in ancient Greece that the Earth was a sphere, to the distances of quasers measured today, the continuity of the search has remained unimpaired, and the meandering stream of human knowledge has increased to a mighty river, of which this book attempts to give a bird's eye view.

KOURGANOFF, V., **Introduction à la physique des intérieurs stellaires** (Series: *Monographies Dunod*), Dunod, Paris, 1970, xvi+208 pp., figs. and tables, 29 F.

Ce livre tente d'expliquer comment fonctionnent ces « réacteurs à fusion contrôlée » que sont les étoiles, sans noyer les raisonnements physiques dans le formalisme mathé-

matique utilisé par les spécialistes et sans supposer de la part du lecteur de connaissance en physique nucléaire. L'auteur insiste sur l'établissement de l'équation de l'équilibre mécanique de la matière stellaire (gazeuse) que l'on assimile souvent, à cause d'une analogie purement formelle, avec l'équation d'équilibre d'un liquide au repos. Prenant, comme hypothèse de travail, la distribution des densités massiques en fonction de la distance au centre de l'étoile pour un modèle du soleil dans un état d'évolution proche de son état actuel, il démonte le mécanisme physique par lequel la distribution de ces densités détermine celle des masses, des pressions et des températures à l'intérieur de l'étoile. Le problème de l'équilibre énergétique de l'étoile est ensuite envisagé dans ce livre et les principales réactions nucléaires en jeu sont considérées, en explicitant la composition des noyaux par des schémas convenables.

LAGRIFFE, L., **Vieux remèdes du temps présent,** Librairie Maloine S.A., Paris, 1970, 256 pp., 25 F.

Malgré les incontestables découvertes de la chimie moderne, la Médecine continue, aujourd'hui encore, à utiliser les propriétés salutaires des drogues tirées du règne végétal ainsi que celles des innombrables préparations auxquelles les « simples » ont rendu, au cours des siècles, les plus grands services à l'humanité. Même les adeptes de la biochimie actuelle ne peuvent dédaigner ces remèdes d'hier et les laisser tomber complètement dans l'oubli. C'est le but que se propose d'atteindre le livre des « *Vieux remèdes du temps présent* » qui constitue une véritable rétrospective de la petite histoire de la Médecine et de la Pharmacie. Sans chercher à faire une oeuvre à caractère technique, l'auteur s'est surtout ingénié à offrir au public non spécialisé auquel le livre est plus particulièrement destiné, un ouvrage d'un intérêt réel par la richesse de sa documentation et par la portée de quelques préparations d'hier, toujours valables aujourd'hui, qu'il a su choisir parmi tant d'autres afin de faire revivre les traditions médicales du Passé dont certaines ont constitué et constituent encore la base de la Pharmacopée actuelle.

LAKATOS, I. and MUSGRAVE, A. (eds.), **Criticism and the Growth of Knowledge** (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 4), Cambridge University Press, London, 1970, VIII+282 pp., 70s.

The volume arose out of a symposium on Kuhn's work, with Popper in the chair, at an international colloquium held in London in 1965. It is not simply a report of the discussion which took place there, for several of the papers have been expanded, written and rewritten between 1965 and 1969. The book begins with Kuhn's statement of his position, followed by seven essays offering criticism and analysis, and finally by Kuhn's reply. The book will interest senior undergraduates and graduate students of the philosophy and history of science, as well as professional philosophers, philosophically inclined scientists and some psychologists and sociologists.

LARGEAULT, J., **Logique et philosophie chez Frege,** Editions Nauwelaerts, Louvain, 1970, XXVIII-486 pp., 720 F.B.

CONTENTS. - Avant-propos: *Justification de la méthode.* - Préliminaire: *Frege, les intentions et les réalisations.* - I. La théorie logique. *I: La version du calcul des énoncés dans la Begriffsschrit. - II: Les idées primitives d'un calcul d'ordre supérieur considérées du point de vue sémantique. - III: Les fondements généraux du calcul d'ordre supérieur de Frege. - IV: Sur la notion de classe chez Frege et de quelques mauvais calembours.* - II. La réduction de l'arithmétique à la logique. *V: Où l'on voit comment certains termes arithmétiques sont définis explicitement dans un langage formalisé dont les symboles primitifs sont interprétés. - VI: Une controverse sur la notion d'axiome. - VII: Le logicisme et le formalisme.* - Conclusion: *Le déclin d'un absolu mathématico-logique, la notion de vérité.*

LASZLO, E. and WILBUR, J. B. (eds.), **Human Values and Natural Science** (Proceedings of the Third Conference on Value Inquiry, State University of New York, College at Geneseo), Gordon and Breach Science Publishers, London, 1970, XIV+292 pp., £6.5.0.

In an age when thermonuclear bombs threaten to blast man off the face of the earth, overpopulation threatens to constrain him to standing room in starvation, and pollution endangers his health, and when an impersonal technology, a rapidly changing condition of society, and a slipping relativity of values disfigure and distort man's self-image and his sanity, the radical estrangement of the traditional « two cultures » of the humanist and the scientist must be overcome. Scientists are increasingly accepting the responsibility for the practical consequences of their discoveries and appreciating that technology not only can work wonders for human betterment but may also, if unchecked, destroy us all. Humanists have come to realize that science is a major force in human life and not merely a theoretical exercise or a source of curious phenomena in the laboratory. In the face of grave dangers which threaten to become more acute with each generation—most of which dangers would not exist but for some discovery in the field of the natural sciences that through a short-sighted application has led to an upsetting of the balance of nature with respect to the human socio- bio-cultural ecology — the positive potential of science to enhance human values must be demonstrated. The Conference on Human Values and Natural Science, of which this volume contains the full proceedings, was called with this purpose in mind.

LEFEBVRE, H., **Du rural à l'urbain,** Editions Anthropos, Paris, 1970, 288 pp., 25 F.

La dispersion de ces textes n'est en un sens qu'apparente. Ils ont un centre théorique: le rapport « campagne-ville », à savoir une relation dialectique, une opposition conflictuelle qui tend à se dépasser lorsque simultanément l'antique campagne et l'ancienne ville se résorbent dans le « tissu urbain » généralisé. Ce qui définit la « société urbaine » s'accompagne d'une lente dégradation et disparition de la campagne, des paysans, du village, ainsi que d'un éclatement, d'une dispersion, d'une prolifération démesurée de ce qui fut jadis la Ville. Aujourd'hui, il ne serait plus question de rêver en proposant un « nouvel urbanisme ». Ce rêve eut un sens, peut-être, il y a une dizaine d'années. La question principale, en ce moment, ce serait plutôt d'aller jusqu'au bout de la critique radicale des projets dits urbanistiques, marqués d'un double stigmate: l'action de l'Etat, le déploiement du « monde de la marchandise ».

LERMAN, I. C., **Les bases de la classification automatique** (Series: *Programmation*), Gauthier-Villars, Paris, 1970, XIV+118 pp., 42 F.

Pour rassembler et comparer ses connaissances parcellaires, l'homme de science est amené à édifier des classifications: la plus vaste classification est celle du règne animal et végétal. Celle-ci a donné l'indispensable langage commun à tous les naturalistes. Cette classification du règne animal et végétal est une synthèse qui peut servir de modèle dans les domaines nouvellement explorés de l'écologie, de l'archéologie, de la physiologie, de l'économie, etc. Toutefois, mettre en forme des données pour élaborer une classification est une tâche qui prend beaucoup de temps au spécialiste; l'existence de l'ordinateur permet heureusement aujourd'hui de réaliser une solution automatique utilisant des algorithmes de calcul. Jusqu'ici, ce sont surtout les spécialistes qui ont cherché à résoudre cette question dans leur domaine propre, en faisant appel dans une large mesure à leur intuition et en proposant les algorithmes qui ne sont en fait que des procédures d'approximations plus ou moins justifiées. Bien au contraire, dans ce livre sont proposées des solutions clairement définies à partir d'une formulation mathématique du problème, formulation dépourvue d'arbitraire et aussi générale que possible (sans pour autant compromettre la fécondité des résultats). L'auteur met ainsi à jour et résoud de nouveaux problèmes de nature combinatoire ou statistique qui présentent un très grand intérêt pour le taxinomiste. Ce livre s'adresse donc d'abord aux mathématiciens statisticiens qui s'intéressent à l'application de leur méthode aux sciences de la nature et aux sciences de l'homme. Mais les spécialistes de ces domaines liront également avec profit cet ouvrage, car l'introduction détaillée rend abordable la formulation algorithmique des exemples réels traités, exemples tirés de la géographie économique (détermination des zones orientant le prix du blé au XIX^{e} siècle), de la psycho-sociologie (recherche des modèles proposés dans la littérature « pour enfants »), de la biologie (classification phylogénique des êtres vivants à partir de la composition en acides aminés de la cytochrome C).

LIPPARD, S. J. (ed.), **Progress in Inorganic Chemistry,** Vol. 11, John Wiley & Sons Ltd., London, 1970, x+402 pp., figs. and tables, 190s.

Contents. - *I: Vibrational Spectra and Metal-Metal Bonds*, by T. G. Spiro. - *II: Transition Metal Complexes of Azulene and Related Ligands*, by M. R. Churchill. - *III: Metal Tetrahydroborates*, by B. D. James and M. G. H. Wallbridge. - *IV: The Chemistry of the Dithioacid and* 1,1-*Dithiolate Complexes*, by D. Coucouvanis.

LIVOLSI, M., **Comunicazioni e cultura di massa** (Testi e documenti), (Series: *La Conoscenza Sociologica*), Ulrico Hoepli Editore, Milano, 1969, 516 pp., L. 4.800.

Contents. - I. Per una sociologia delle comunicazioni di massa. *I: Il tentativo di definire l'oggetto della disciplina. - II: Vecchi e nuovi oppositori. - III: Nuove prospettive metodologiche.* - II. Alcuni filoni della sociologia delle comunicazioni di massa. *I: La cultura di massa. - II: Gli effetti sociali della comunicazione di massa. - III: Il processo di comunicazione a due livelli.* - III. Contributi allo studio dei vari media. *I: Contributo allo studio di vari media.*

MacINNIS, A. J. and VOGE, M. (eds.), **Experiments and Techniques in Parasitology,** W. H. Freeman and Company, Berkshire, 1970, xvi+232 pp., figs. and tables, 40s.

Contents. - *I: Life Cycles and Growth. - II: Triggers Activating Larvae. - III: Behavioral Responses. - IV: Crowding and Competition between Parasites. - V: Acquisition of Nutrients. - VI: Nutritional Requirements and Growth. - VII: Effects of the Parasite on the Host: Pathology. - VIII: Effects of the Host on the Parasite: Immunology. - IX: Laboratory Maintenance of Parasites. - X: Hematologic Procedures. - XI: Sterile Procedures and in* Vitro *Techniques. - XII: Separation of the Major Chemical Fractions of Parasites. - XIII: Quantitative and Qualitative Determinations of Major Chemical Constituents. - XIV: Procedures in Immunology. - XV: Preparatory Techniques for Electron Microscopy*

MANNO, V. and PAGE, D. E. (eds.), **Intercorrelated Satellite Observations Related to Solar Events** (Proceedings of the Third Eslab/Esrin Symposium held in Noordwijk, The Netherlands, September, 16-19, 1969), D. Reidel Publishing Company, Dordrecht, 1970, xvi+628 pp., figs. and tables, Dfl. 115.

The European Space Research Organisation put its first satellite into orbit in March 1968 and was successful with two more before the end of that year. The third annual Eslab-Esrin Symposium dealt in some way with the results obtained. This book is the Proceedings of that Symposium which, for reasons which Dr. Trendelenburg explains in the introductory talk, concentrated on intercorrelating phenomena occurring during solar events and in particular during the event of 25 February 1969. It is generally acknowledged that space data could yield a much more fruitful harvest if measurements taken simultaneously in different regions of space could be compared and the detectors intercalibrated. Esro therefore sought right from the start to encourage this comparison of data. The first two days of the Symposium (16 and 17 September 1969) were devoted to review lectures on inter-related phenomena occurring on the sun, through interplanetary space and the magnetosphere right down to the earth's ionosphere. The last two days were used to hear papers presenting results obtained from the Esro and certain U.S.S.R. and U.S.A. satellites during the solar events around 25 February 1969. The Proceedings published here follow the same sequence as the Symposium presentations.

MARITZ, J. S., **Empirical Bayes Methods** (*Methuen's Monographs on Applied Probability and Statistics*), Methuen and Co. Ltd., London, 1970, viii+160 pp., tables, 50s.

The prior probability distribution plays a central role in the Bayes approach to statistics; the practical and philosophical problems associated with this concept have hitherto militated against widespread use of Bayesian techniques, but recent years have seen a marked revival of interest in Bayesian methods of statistical inference. This is thought to be partly due to the desire to make more effective use of ancillary information in the analysis data. This book assembles empirical Bayes methods which have already been developed, and proposes some new procedures for certain two-parameter problems. An attempt has been made to present procedures in such a way that potential users who have no experience in this field, but are familiar with traditional methods, will be able to adapt them for their own problems.

CONTENTS. - *I: Introduction to Bayes and Empirical Bayes Methods. - II: Estimation of the Prior Distribution. - III: Empirical Bayes Point Estimation. - IV: Testing of Hypotheses. - V: Two-parameter Problems. - VI: The Compound Decision Problem and EB Methods.*

MAYER, H. M. and WADE, R. C., **Chicago: Growth of a Metropolis,** The University of Chicago Press, London, 1969, x+510 pp., figs. and maps, £9.

This is the story of Chicago and how it grew. The book differs markedly from other « pictorial histories » of American cities. Instead of emphasizing society and customs, this volume deals with the physical conditions of life. In place of the conventional interest in « founding fathers » and leading families, it is more concerned with street scenes and ordinary people. Without neglecting the downtown, it also reaches into the residential areas and neighborhood shopping centers. Moreover, this volume is concerned with suburbs and « satellite » towns as well as the historic city. It encompasses the area from Waukegan on the north, to Elgin on the west, to Gary on the east. In short, it tries to reconstruct metropolitan Chicago and to see it as it appeared to successive generations of residents and visitors. This book also differs from other « pictorial histories » because it attempts to use photography as evidence instead of as mere illustration.

MEDVEDEV, Z. A., **The Rise and Fall of T. D. Lysenko,** Columbia University Press, London, 1969, xx+284 pp., figs., 90s.

As Zhores A. Medvedev states in his Preface, the three parts of the book represent the author's three points of view: as historian; as a witness to the events; and as an active participant in the last stages of Lysenkoism, which he helped to topple. The book is not only a history; it is also an indictment of a system of centralized control of science. We can all profit from its lesson. This book consists of three parts. The first two were written in 1961-1962 and augmented in 1963-1964; the third was written in 1966-1967. Each part, for various reasons, differs from the others in style and method of analysis of the facts. In the first place, the existing situations in science described in each section differed, and the problems around which the debate was carried on were changing, even though genetics remained the center of attention. In the second place, the different parts of the book were written under different conditions.

CONTENTS. - I. The First Phase: 1929-1941. *I: The Historical Background of the Controversy. - II: The Struggle Begins. - III: The First Phase Climax. - IV: Medical Genetics in 1937-1940. - V: The Agronomy Debate of 1935-1938.* - II. The New Phase: 1946-1962. *VI: The Postwar Period. - VII: The Two Trends up to 1963. - VIII: Lysenko's Agrobiology.* - III. The Last Phase: 1962-1966. *IX: The Events of 1962-1964. - X: The End of Lysenkoism. - XI: How Did It Happen?*

MEYNELL, G. G. and MEYNELL, E., **Theory and Practice in Experimental Bacteriology,** 2nd ed., Cambridge University Press, London, 1970, xiv+348 pp., figs., tables and plates, 75s.

The second edition of this book, like the first, is a documented guide to the fundamentals of microbiological technique. Although primarily intended for biologists turning to work with bacteria, much of the material is applicable to micro-organisms of all kinds. The major changes in this edition are a new chapter of genetic technique, including drug-resistance factors and other bacterial plasmids, and sections on photodynamic inactivation, fluorescence microscopy and autoradiography. In addition, a large part of the previous text has been rewritten, notably the sections on bacterial growth, the thermodynamics of disinfection, microscopy of bacteria and the interpretation of exponential processes in terms of birth-death models. About a third of the references are new and are again chosen less on priority than to illustrate particular points or because they provide a useful key to the literature. This book will interest all who work with micro-organisms, whether in biological research, industry or medical laboratories.

Contents. - *I: Bacterial Growth.* - *II: Bacteriological Culture Media.* - *III: Oxygen, Carbon Dioxide and Anaerobiosis.* - *IV: Sterilization.* - *V: Examination of Bacteria by Microscopy.* - *VI: Quantitative Aspects of Microbiological Experiments.* - *VII: Genetic Technique.*

MONOD, M., **Manuel d'application du Test du Village** (Technique projective non-verbale), (Series: *Actualités Pédagogiques et Psychologiques*), Delachaux et Niestlé, Neuchatel, 1970, 296 pp., figs., 42 F.

Le présent ouvrage, consacré au Test du Village, et la première partie d'une recherche sur la projection de l'Image du Corps. Il s'inscrit dans une perspective essentiellement didactique reflétant la discipline psychologique à laquelle l'auteur n'hésite pas à se plier. Le Test du Village a pour objet la compréhension de la personnalité totale. Le matériel, qui comprend 132 éléments, se présente comme un jeu de construction d'enfant à partir duquel le sujet est prié de construire un village sur une table carrée de 80 cm de côté. L'analyse de la construction permet de cerner la dynamique de base des échanges caractérisant la personnalité. A l'issue de la construction, un dialogue entre le psychologue et le sujet sur l'organisation du village permet de préciser les facteurs caractérisant l'attitude affective du sujet au moment de la passation.

MOORE, P., **Eléments d'astronomie** (Series: *Science-Poche*), Dunod, Paris, 1970, 150 pp., figs. and tables, 8,70 F.

Présenter, sans équations mathématiques, physiques ou chimiques, les différents éléments de notre univers et en décrire les moyens d'observation, répondre aux passionnantes questions concernant son évolution et le devenir de notre planète, tel est le but de cet ouvrage qui concerne, sans restriction, tous ceux qu'intéresse l'astronomie.

NEEDHAM, J., **Clerks and Craftsmen in China and the West** (Lectures and Addresses on the History of Science and Technology), Cambridge University Press, London, 1970, xx+470 pp., figs. and plates, 150s.

These essays are mainly by-products of the writing of *Science and Civilisation in China*, Dr Needham's great work on the history of Chinese science, scientific thought and technology, now in the third decade of its production. Many are lectures of varying degrees of formality, others communications to international symposia, the remainder publications in journals or collaborative works. Readers of the main volumes will be prepared for the impact of Dr Needham's astonishing expository gifts in a wide number of subjects. The whole range of science and technology seems to fall naturally within his compass. The present collection includes essays on cultural contacts and transmissions between East and West, astronomy and meteorology, metallurgy, engineering, nautical technology, alchemy, chemistry and pharmacy, and medicine. All these topics are presented to the reader through the unifying medium of the mind of one of the great scholars of our time; and all are directly related to his major theme and constant preoccupation—the enormous and hitherto undervalued contribution of Chinese culture to world science and technology as a whole.

NÖTZOLD, G. (ed.), **Die arabischen Länder,** VEB Hermann Haack, Gotha, 1970, 352 pp., figs., DM 24.

Contents. - I. Der arabische Raum. Übersicht. *I: Politische Gliederung.* - *II: Naturbedingungen des arabischen Raumes.* - *III: Wirtschaftshistorischer Abriss.* - *IV: Sozialökonomische Formationen.* - *V: Struktur, Verteilung und Entwicklung der Bevölkerung.* - *VI: Entwicklung und Verteilung der Produktion.* - *VII: Zur Entwicklung der*

Territorialstruktur der Volkswirtschaft in den arabischen Ländern. - II. Länderübersichten. *I: Maghreb. - II: Nilländer. - III: Arabische Halbinsel. - IV: Länder des Maschrik.* - Anhang.

OHNO, S., **Chromosomes sexuels et gènes liés au sexe**, Gauthier-Villars, Paris, 1969, x+194 pp., figs., 48 F.

L'originalité de l'ouvrage réside d'abord dans l'unité de plan de composition qui permet de dégager clairement l'ensemble des problèmes que posent les chromosomes sexuels et les gènes qui leur sont liés. De plus, la richesse des hypothèses émises se juge autant par l'abondance des références citées à leur appui que par la diversité des voies de recherche qu'elles tracent. En particulier, la notion de conservation d'un chromosome sexuel originel commun à tous les mammifères placentaires d'une part et aux oiseaux d'autre part est présentée de telle sorte qu'au lieu d'apparaître comme une extrapolation douteuse, elle se présente à nous comme une hypothèse séduisante, née d'un esprit extraordinairement synthétique.

CONTENTS. - *I: Évolution des chromosomes sexuels et des génomes des Vertébrés. - II: Évolution du mécanisme de compensation du dosage des gènes liés au sexe. - III: Sur les facteurs appelés déterminants sexuels et le fait de la détermination du sexe.*

PARFENOFF, A., POMEROL, Ch. and TOURENQ, J., **Les minéraux en grains** (Méthodes d'étude et détermination), Masson & C.ie, Paris, 1970, 578 pp., figs., tables and plates, 250 F.

La première partie de cet ouvrage est consacrée à l'exposé de quelques principes qui régissent la sédimentation des minéraux lourds: filiation, sédimentation inverse, sommation ou superposition de phases, triage ou dissociation de phases. Les auteurs envisagent l'intérêt et les applications de l'étude des minéraux lourds dans divers domaines des sciences de la Terre: sédimentologie, océanographie, stratigraphie, tectonique, paléogéographie, pétrographie, métallogénie, pédologie. La seconde partie traite des techniques de prélèvements, de séparation et de détermination des minéraux en grains par des méthodes chimiques, physiques ou optiques, qui sont présentées en tenant compte des progrès récents et de l'expérience personnelle des auteurs. Le lecteur trouvera non seulement les méthodes d'emploi d'appareils coûteux ou délicats mais aussi bien des « tours de mains » et des « recettes » faciles à mettre en oeuvre. La troisième partie est une monographie détaillée de 216 minéraux présentés par ordre alphabétique. Pour chacun d'eux sont indiqués successivement: l'aspect, les caractères cristallographiques, physiques, optiques, chimiques, l'altération, les confusions possibles, les conditions de gisement, l'utilisation et les principaux éléments de diagnose. La détermination des minéraux est facilitée par de nombreux croquis, une illustration photographique originale (9 planches en couleurs représentant 62 minéraux) et par des tableaux synoptiques à plusieurs entrées, parmi lesquels: les susceptibilités magnétiques, les constantes diélectriques, les indices de réfraction, les densités, la fluorescence, les raies de plus forte intensité obtenues sur les clichés de poudre aux rayons X. S'y ajoutent neuf tableaux hors texte de détermination des minéraux opaques et des minéraux transparents. Ce livre est à la fois un outil fidèle et sûr pour l'utilisateur et une petite encyclopédie d'applications pratiques et de recherche fondamentale.

PAULING, L., **General Chemistry**, 3rd ed., W. H. Freeman and Company Ltd., Berkshire, 1970, XVI+960 pp., figs. and tables, 66s.

This edition, like the preceding editions, is designed especially for use by first-year college university students who plan to major in chemistry or in closely related fields, and by other well-prepared students with a special interest in chemistry.

CONTENTS. - *I: The Nature and Properties of Matter. - II: The Atomic and Molecular Structure of Matter. - III: The Electron, The Nuclei of Atoms, and the Photon. - IV: Elements Compounds. Atomic and Molecular Masses. - V: Atomic Structure and the Periodic Table of the Elements. - VI: The Chemical Bond. - VII: The Nonmetallic Elements and Some of Their Compounds. - VIII: Oxygen Compounds of Nonmetallic Elements. - IX: Gas-*

es: Quantum Mechanics and Statistical Mechanics. - X: Chemical Thermodynamics. - XI: Chemical Equilibrium. - XII: Water. - XIII: The Properties of Solutions. - XIV: Acids and Bases. - XV: Oxidation-Reduction Reactions. Electrolysis. - XVI: The Rate of Chemical Reactions. - XVII: The Nature of Metals and Alloys. - XVIII: Lithium, Beryllium, Boron, and Silicon and Their Congeners. - XIX: Inorganic Complexes and the Chemistry of the Transition Metals. - XX: Iron, Cobalt, Nickel, and the Platinum Metals. - XXI: Copper, Zinc, and Gallium and their Congeners. - XXII: Titanium, Vanadium, Chronium, and Manganese and Their Congeners. - XXIII: Organic Chemistry. - XXIV: Biochemistry. - XXV: The Chemistry of the Fundamental Particles. - XXVI: Nuclear Chemistry.

PECKER, J.-C., **Experimental Astronomy** (*Astrophysics and Space Science Library*, Vol. 18), D. Reidel Publishing Company, Dordrecht, 1970, x+106 pp., figs. and tables, Dfl. 28.

This work deals with two types of problems: extra-terrestrial devices, considered as astronomical objects (Chapters I, II, III, IV), and the direct exploration of the astronomical universe (Chapters V and VI); through space research, these change the nature of astronomical research completely.

CONTENTS. - *I: Astronomy - An Experimental Science? - II: Artificial Satellites as Celestial Bodies, or the Introduction of « Experimental Celestial Mechanics ». - III: Initiation in Astronautics. - IV: What of Experimental Astrophysics? - V: The Direct Exploration of the Extraterrestrial World. - VI: The Plurality of Inhabited Worlds.*

PERREAU, P. (H. SOUBIES-CAMY), **L'enregistrement magnétique de mesures** (Series: *Techniques d'Instrumentation*), Editions Eyrolles, Paris, 1969, 136 pp., figs., 18 F.

Ce livre est le premier d'une nouvelle collection destinée à fournir, en matière d'instrumentation électronique moderne, une référence commune aux utilisateurs et à ceux qui les construisent. Dans l'esprit de ses auteurs, il doit constituer — comme les suivants — un précis auquel pourront se référer les ingénieurs non spécialistes, conduits à s'intéresser à cette technique. Suivant une méthode applicable à l'ensemble de la collection, on s'est attaché, dans ce premier livre, à montrer la place de l'enregistrement magnétique dans une chaine de mesures, ses méthodes, les caractéristiques métrologiques et les limites de chacune d'elles, en précisant les critères de jugement des performances et indiquant les normalisations de toute nature qui peuvent exister. Après les rappels généraux consacrés aux notions de base de l'électromagnetisme (chapitre 1) et aux principes mêmes de l'enregistrement magnétique (chapitre 2), l'ouvrage décrit la constitution et le fonctionnement d'un enregistreur magnétique (chapitre 3). Les différentes techniques d'enregistrement sont abordées dans les chapitres suivants: enregistrement direct (chapitre 4), enregistrement par modulation de fréquence (chapitre 5), enregistrement de signaux commutés (chapitre 6), enfin enregistrement numérique (chapitre 7), en indiquant à chaque fois les avantages et inconvénients relatifs de la méthode décrite, ainsi que ses limitations. Un dernier chapitre (chapitre 8) est consacré aux utilisations et applications de l'enregistrement magnétique. Ce premier volume a reçu un appui particulièrement efficace, à la fois pour sa conception et sa rédaction, de la part du Service Méthodes du Centre d'Essais en vol de Brétigny, et bénéficié de l'expérience acquise dans les diverses session de formation des personnels spécialisés de ce dernier. Il se recommande ainsi tout naturellement aux ingénieurs et techniciens désireux de se familiariser avec les méthodes de l'enregistrement magnétique de mesures et de les mettre en application dans les secteurs les plus avancés, notamment dans celui des industries aéro-spatiales où elles se sont rapidement imposées et continuent à étendre leur champ d'utilisation. Il est également destiné aux élèves des Grandes Ecoles et des Instituts Universitaires de Technologie, intéressés par les problèmes d'instrumentation qui se posent au monde industriel moderne.

PIOTTE, J.-M., **La pensée politique de Gramsci** (Series: *Sociologie et Connaissance*), Editions Anthropos, Paris, 1970, 302 pp., 20 F.

Abondante et inachevée, l'oeuvre de Gramsci mûrie au coeur du combat révolutionnaire en manifeste les richesses et les contradictions. Gramsci fut jugé tour à tour l'égal ou le continuateur de Lénine. A ce titre, son oeuvre, comme celle de Lénine, a donné lieu à bien des commentaires et bien des utilisations. Jean-Marc Piotte ne vient pas purement et simplement s'ajouter à cette liste. Saisi par le drame du théoricien militant emprisonné, contraint à livrer une oeuvre fragmentaire, l'auteur de cet ouvrage n'a pas cherché à faire oeuvre d'exégète ou de simple utilisateur, mais a préfére une autre perspective: restituer l'élan que l'emprisonnement d'abord, les utilisations posthumes ensuite, ont tenté de briser. Bien que nécessaire un tel travail ne manquait pas d'embûches; restituer de nos jours une oeuvre née dans l'Italie du début du siècle, parcellaire, parfois contradictoire, sousmise d'abord à la censure, ensuite à tous les tiraillements et toutes les réducteurs — la vérité de Gramsci, selon les « socialistes italiens », doit être cherchée entre 1919 et 1920 avant les *Quaderni de Carcere*, et dans la période qui les suivis pour les « communistes » de ce même pays. C'est précisément sur cet ouvrage, au centre du débat, que Jean-Marc Piotte fait porter l'essentiel de son analyse.

PIRCHNER, F., **Population Genetics in Animal Breeding,** W. H. Freeman and Company, Berkshire, 1969, XIV+274 pp., figs. and tables, 74s.

The simple Mendelian schemes that are used to explain the inheritance in animals of some all-or-none traits like coat color and blood group have not been successful in accounting for the inheritance of quantitative characteristics such as milk yield and growth rate. The application of population genetics, however, has proved much more useful. Largely because of this, animal breeding can now be conducted along strictly scientific lines. In this book a prominent European authority on animal genetics presents a comprehensive, basic exposition of pertinent population genetics in relation to theoretical animal breeding. The book is designed for advanced undergraduate or beginning graduate courses in animal breeding and population genetics. The author has revised and updated the 1964 German edition for this translation. An introductory chapter offers readers a view of the basic concepts of genetics and statistics. The following three chapters deal with the principles of population genetics relative to single genes, beginning with an exposition of the Hardy-Weinberg equilibrium. Quantitative variation is next introduced. Heritability and genetic correlations — basic concepts of theoretical animal breeding — are covered in subsequent chapters, and these are followed by chapters discussing selection and various aids used to improve selection. Then inbreeding and heterosis are treated. The final chapters of the book deal with breeds and breed formation, and with biochemical polymorphism. Population genetics and the theory of animal breeding have developed during the past several decades mainly as separate sciences. Because this book illustrates principles of population genetics with examples drawn wherever possible from animal breeding, it will be widely appreciated by all who work in either field.

PIRENNE, M. H., **Optics Painting and Photography,** Cambridge University Press, London, 1970, XXIV+200 pp., figs., £4.

Linear perspective is fundamental to the representation of a three-dimensional scene on a surface. It applies in a simple manner only to the special case of trompe-l'oeil pictures which are seen in three dimensions. However, as Leonardo da Vinci suspected, the problem is much more complex in the case of ordinary pictures, for here the spectator is to some extent aware of the characteristics of the picture surface. This gives stability to his percept of the depicted scene when he views it from the wrong position. Dr Pirenne examines the whole argument, starting from the geometry of human vision and going back to Euclid's optics. He discusses the theory of perspective with the help of elegant experiments devised specially for the purpose and with reference to the historical development of the art of painting. The essentials of physical and physiological optics are simply explained and the whole book is well illustrated. It will appeal to anyone interested in visual communication and in the meaning and evolution of pictorial art; it will be of particular use to artists and photographers, professional and amateur. The book does not tell artists what they ought to do, but rather investigates what has been done in the course of the centuries.

POPPER, K. R., **Logica della scoperta scientifica,** Einaudi, Torino, 1970, XXX+550 pp., L. 3.400.

CONTENTS. - I. Introduzione alla logica della scienza. *I: Sguardo su alcuni problemi fondamentali. - II: Il problema di una teoria del metodo scientifico.* - II. Alcune componenti strutturali di una teoria dell'esperienza. *III: Le teorie. - IV: La falsificabilità. - V: Il problema della base empirica. - VI: Gradi di controllabilità. - VII: Semplicità. - VIII: Probabilità. - IX: Alcune osservazioni sulla teoria dei quanti. - X: La corroborazione, ossia: come una teoria regge ai controlli.* - Appendici. Nuove appendici. Indice analitico. Indice dei nomi.

Progress in Brain Research, VOL. 28: **Anticholinergic Drugs and Brain Functions in Animals and Man,** by P. B. BRADLEY and M. FINK, Elsevier Publishing Company, Amsterdam, 1968, XVI+184 pp., figs. and tables, Dfl. 35.00.

While most treatments useful in psychiatry are effective to the extent they selectively alter the central nervous system, there is no generally useful way of measuring the induced changes or defining the locus of action except measurement of the gross electrical changes seen in the EEG. While the EEG changes have often been related to changes in behaviour, since 1952 there have been frequent disquieting reports of "dissociation". These reports have generally come from animal studies and principally after the use of anticholinergic drugs. This volume presents arguments illustrating a variety of issues that have contributed to the problem, including limited observations, non-quantitative assessments, poor definition of relevant behaviours, lack of appreciation of drug-dose relations, and species specificity. While these problems contribute to the controversy, the lack of testable theories relating brain function to behaviour is also a major factor in the propagation of the controversy. The undue dependence on animal studies in the area of research so uniquely human and the consequent errors is highlighted by the reports of this volume. The volume concludes with the suggestion that the association of EEG and behaviour will become better defined with more studies in man, and the questions may change from the conditions of dissociation to questions of the behaviours which are directly related to specific electrophysiological changes accompanying specific behaviours in man. Of interest to: Pharmacologists, psychiatrists, experimental neurologists, and experimental psychologists.

Progress in Brain Research, VOL. 29: **Brain Barrier Systems,** by A. LAJTHA and D. H. FORD, Elsevier Publishing Company, Amsterdam, 1968, XVI+552 pp., figs. and tables, Dfl. 95.00.

It has been known for some time that the permeability properties of the brain are unique for this organ; indeed this "Blood Brain Barrier" is one of the properties in which the nervous system is different from the rest of the organism. Recently this field has been investigated with new methods of approach, and a conference on this topic was organized by Drs. Ford and Lajtha near Amsterdam, which resulted in the present book. This conference, which showed that the old concept of a single barrier does not explain all observations, had the appropriate title "The Brain Barrier System". It was the first symposium to review critically the latest standing of this complex field in all its aspects. The composition of membranes and their structure was discussed and it was clearly shown, not only that we can explain all the complexities with structures, but, that the structural components can be changed in altering states of the nervous system. Perhaps these factors, (a) the complexity of the system, and (b) the ability of the system to respond to outside stimuli, were the two most important features that emerged from the conference. Of interest to: Neuroanatomists, neurophysiologists, neuropharmacologists, psychiatrists, neurologists, neurosurgeons and medical school libraries.

Progress in Brain Research, VOL. 30: **Cerebral Circulation,** by W. LUYENDIJK, Elsevier Publishing Company, Amsterdam, 1968, XVI+498 pp., figs. and tables, Dfl. 95.00.

Cerebral Circulation was the theme of the Third European Neurosurgical Congress in Madrid, April 1967. The normal and pathological aspects of blood circulation in the brain were dealt with by workers in the basic research areas as well as by clinicians. The purpose of this volume is to present as much material as possible about normal and pathological cerebral circulation. Outstanding authors contribute to the newest developments in areas of basic research - anatomy, physiology, and pathology of cerebral circulation. Excellent clinical surveys are given dealing especially with aneurysms, localisations, arterio-venous anomalies, angiomas of the brain and disorders of vascular obstruction of the main vessels in the neck. The work being done in these basic fields makes possible a better understanding of pathological consequences in patients. The official reports of the congress and the most important free communications on the theme are included in this book. Of interest to: Specialists in clinical neurophysiology, neurology, neurosurgery, radiology, vascular surgery, electroencephalography, neuroanatomy, and neuroanaesthesiology.

QUÉNU, J, **Nouvelle pratique chirurgicale illustrée,** 3 vols., Editions Doin, Deren & C.ie, Paris, 1967-1968-1969, 300 pp., 284 pp. and 270 pp., figs., Vol. I: 73 F., Vol. II: 60 F., Vol. III: 82 F.

Contents. - **Volume I.** - *I: Pose d'un stimulateur cardiaque intra-corporel,* by Ch. Cabrol. - *II: Résection trachéale suivie d'anastomose termino-terminale pour cylindrome du tiers moyen de la trachée,* by Cl. Dubost and Cl. Evrard. - *III: Oesophago-gastroplastie, opération de Graviliu modifiée,* by J. Perrotin and P. Hautefeuille. - *IV: Lipectomie circulaire,* by G. Thomeret. - *V: Adénomectomie prostatique par voie transvésicale large,* by L. Quénu. - *VI: Opération de Colonna: arthroplastie de la hanche congénitalement luxée de l'enfant,* by J. Judet and P. Rigault. - *VII: L'amputation de cuisse par transfixion pour gangrène par artérite,* by C. Frileux and P. Pillot. - **Volume II.** - *I: Resection et anastomose bronchique après lobectomie inférieure gauche pour cylindrome infiltrant,* by Cl. Dubost and P. Chartier. - *II: Plicature de la veine cave inférieure,* by J. Moreaux and J. Testart. - *III: Exérèse d'une tumeur de l'hypochondre gauche,* by M. Arsac. - *IV: Opération de Childs et Phillips après résection iléo-caecale,* by J. Moreaux and J. Testart. - *V: Colpectomie totale avec hystérectomie et lymphadénectomie pour cancer du vagin,* by G. Thomeret. - *VI: Urétérectomie segmentaire pour papillome de l'uretère iliaque,* by L. Quénu. - *VII: Arthroplastie tibio-tarsienne,* by J. Lelièvre. - *VIII: Quintus varus congénital,* by J. Lelièvre. - **Volume III.** - *I: La trachéotomie basse,* by B. Debesse. - *II: Exérèse d'un anévrisme de l'artère sous-clavière droite et prothèse immédiate,* by G. Thomeret. - *III: Résection de la carène, pneumonectomie, anastomose trachéo-bronchique gauche, pour cancer de la bronche souche droite,* by Cl. Dubost. - *IV: Pancréatectomie subtotale pour pancréatite chronique,* by G. Thomeret. - *V: Hémicolectomie gauche et entérectomie pour cancer colique fistulisé dans l'intestin grêle,* by Cl. Dubost. - *VI: Le phéochromocytome, traitement opératoire d'une tumeur surrénale gauche et d'une cholécystite lithiasique associée,* by Cl. Dubost and R. Soyer.

RICHARD, G. (ed.), **Territoire et domaine vital** (Series: *Ecologie et Ethologie,* No. 1), Masson & C.ie, Paris, 1970, 128 pp., figs. and tables, 60 F.

Ce nouveau volume est le second de la série consacrée à la biologie des animaux sauvages. Les organisateurs des réunions annuelles qui se tiennent en forêt de Chizé, au centre d'Etude Biologique des Animaux Sauvages, ont voulu alterner les thèmes physiologiques et les thèmes éco-éthologiques. C'est pourquoi le deuxième volume se trouve consacré aux problèmes du Territorialisme et du domaine vital, alors que le premier traitait des Cycles génitaux saisonniers de mammifères sauvages. Le lecteur trouvera dans ce volume des mises au point modernes sur le centre d'intérêt choisi, des comptes rendus de travaux originaux et le texte intégral des discussions qui ont suivi chaque présentation. Ceci permet d'assurer que les spécialistes, comme les non-spécialistes, pourront faire ample moisson d'informations en consultant cet ouvrage. L'avant-propos du Professeur R. G. Busnel, les conclusions du Professeur G. Richard soulignent l'intérêt des confrontations de ce type et montrent combien il devient important dans la Science actuelle de coordonner les efforts des chercheurs, parfois dispersés. Ceci semble particulièrement vrai pour les questions de vocabulaire qui, dans une grande mesure, se trouvent à la base des difficultés de synthèses.

RIVIER, W., **De l'imaginaire au réel** (Essai sur le temps dans la théorie de la relativité), (Series: *Bibliothèque Scientifique,* No. 39), Dunod, Paris, 1969, 88 pp., 14 F.

La théorie d'Einstein est une construction axiomatique de représentation de l'univers qui se révèle plus conforme, dans l'ensemble, aux données de l'expérience et de l'observation que l'ancienne physique. Dans le présent ouvrage la théorie de la relativité restreinte est d'abord réduite à deux formules qui mettent en évidence les postulats de la théorie d'Einstein. L'essentiel de l'exposé est ensuite consacré au problème du temps qui est ramené aux relations de champs d'horloges animés les uns par rapport aux autres de vitesses constantes. Lorsque ces vitesses changent de direction, il s'introduit des coefficients arbitraires donnant lieu à des conclusions paradoxales. L'auteur reconnaît cependant, à partir de cet exemple, que la théorie d'Einstein est conforme à l'expérience dans des domaines où ses conséquences se différencient suffisamment de celles de la théorie classique. Un appendice spécifie les conditions d'un rayonnement de nature cinétique et devrait rendre intuitive l'idée de champ d'horloges utilisée dans cette étude. Ce livre est par sa nature à recommander aux mathématiciens; mais ses conclusions devraient intéresser tous les chercheurs concernés par cette branche de la physique.

ROHAULT, J., **A System of Natural Philosophy,** Vol. I and Vol. II (a Fac-simile of the Edition and Translation by J. and S. Clarke, Published in 1723), (Series: *The Sources of Science*, No. 50), Johnson Reprint Corporation, New York, 1969, xxx+286 and 292 pp., tables, $35.00.

Rohault lectured publicly on the Cartesian system throughout the 1660's, and it is probable that his wonderfully clear treatment of the Cartesian system prevented an earlier adaption of the more difficult Newtonian explanation of physical laws.

ROUBINE, E. (ed.), **Mathematics Applied to Physics,** Springer-Verlag, Berlin, 1970, xx+610 pp., figs., DM 58.

The essential characteristic of the work in question is that it is the outcome of international co-operation. Physicists need to follow the development of mathematics, even of those kinds of mathematics that do not have an immediate application. Mathematicians, however, do not spontaneously write works designed to meet this desire. The problem is bound up with interdisciplinary teaching at universities. It is to Unesco's credit that it has inspired an attempt to solve it. It is well known that physics and technology are at the origin of certain theories such as: harmonic analysis, the spectral theory, partial differential equations, the theory of information. In other cases, physics has adopted and popularized mathematical theories where these have proved useful long after their first occurrence—such, for instance, as matrices, tensors, spinors.... Today, chemistry uses algebraic topology, coding techniques the Galois fields. These few remarks explain the lines on which the work has been conceived and its fairly classical outlook. To the public for whom it is intended, the chapter titles will have a familiar ring. However, its content is not that of a school textbook and still less of a book for popular reading. The breadth of the subject has necessitated a somewhat concise style, and the work in fact represents an up-to-date statement of the position on a number of points.

Contents. - *I: Functions of Complex Variables*, by F. Sommer. - *II: Theory of Distribution*, by E. M. De Jager. - *III: Exterior Differential Forms*, by G. A. Deschamps. - *IV: Ordinary Differential Equations*, by A. N. Tihonov, A. B. Vasil'eva and V. M. Volosov. - *V: Partial Differential Equations*, by F. John. - *VI: Integral Equations*, by F. John. - *VII: Approximation numérique de la solution des problèmes d'équations aux dérivées partielles*, by J. L. Lions. - *VIII: Optimisation*, by N. Moisseev and V. Tikhomirov. - *IX: Probability Theory and its Applications*, by D. J. A. Welsh. - *X: Quantum Mechanics*, by T. Yamanouchi.

ROURE, F. and BUTERY, A., **Mathématiques pour les sciences sociales,** Vol. I (Series: *Systèmes-Décisions*), Presses Universitaires de France, Paris, 1970, xii+320 pp., figs., 38 F.

Cet ouvrage s'adresse à des étudiants de premier cycle plus spécialement intéressés par les questions d'économie et de gestion. Il est rédigé par deux Maîtres-Assistants du Département de Mathématiques de l'Université de Paris-Dauphine, qui ont déjà une longue expérience pédagogique. Après avoir rappelé les notions de base d'algèbre, les auteurs insistent sur les fonctions numériques de variables réelles. L'ouvrage s'adresse donc aussi bien à des étudiants en sciences économiques qu'aux étudiants des I.U.T. Comme il présente des bases mathématiques, il sera utilisable par des étudiants en sociologie et par ceux du certificat préparatoire aux études médicales.

CONTENTS. - *I: Vocabulaire. - II: Ensembles. - III: Relations binaires. - IV: Applications. - V: Structures algébriques élémentaires. - VI: Nombres réels. - VII: Fonction réelle d'une variable réelle. - VIII: Limites des fonctions réelles d'une variable réelle. - IX: Continuité des fonctions réelles d'une variable réelle. - X: Dérivées. Théorèmes généraux. Primitives. - XI: Etude de la courbe représentative d'une fonction au voisinage d'un point. - XII: Fonctions logarithmes et exponentielles. - XIII: Différentielle. - XIV: Construction des courbes représentatives des fonctions. - XV: L'ensemble R". - XVI: Fonctions réelles de plusieurs variables réelles. - XVII: Dérivées et différentielles des fonctions réelles de plusieurs variables réelles.*

RUNCORN, S. K. (ed.), **The Application of Modern Physics to the Earth and Planetary Interiors,** John Wiley & Sons Ltd., London, 1969, XIV+692 pp., figs. and tables, 260s.

Over the last ten years the concepts and techniques of modern physics have been applied to Geophysics, complementing the classical physics on which the subject had hitherto been based and which has contributed so much to our understanding of the Earth's interior in the last sixty years. Thus, the theory of semi-conduction gives a satisfactory explanation of the distribution of electrical conductivity in the mantle, while modern theory of solids attempts to predict the laws of its mechanical behaviour. The papers presented at this NATO Advanced Study Institute reflect the results of the application of modern physics to problems of the Earth's mantle and interior and give a convenient view of the progress which has been made at the present time.

CONTENTS. - I. Cosmology and Geophysics. *I: Theoretical Considerations Concerning a Change in G. - II: Geophysical Methods of Detecting a Change in G. - III: Theoretical Aspects of the Hypothesis of Earth Expansion. - IV: Geological and Geophysical Evidence Related to the Hypothesis of Earth Expansion.* - II. Solid State Physics and Geophysics. *I: New Developments in the Study of the Electrical Conductivity of the Earth's Mantle. - II: Laboratory Measurements. - III: Theory of Creep Applied to the Earth's Mantle.* - III. High-Pressure Physics and the Earth's Interior. *I: Chemical Composition of the Mantle. - II: Equations of State. - III: Laboratory Data.* - IV. Developments in Techniques. *I: Application of Techniques of Modern Physics to Rock Magnetism. - II: New Applications of Atomic and Nuclear Physics to Geophysical Measurements. - III: Space Technology.* - V. Magnetohydrodynamics. *I: Geomagnetic Reversals. - II: The Dynamo Problem. - III: Aspects of the Geomagnetic Secular Variation.* - VI. The Earth as a Fundamental Physics Laboratory.

RUSSELL, F. S., **The Medusae of the British Isles,** VOL. II: **Pelagic Scyphozoa with a Supplement to the First Volume on Hydromedusae,** Cambridge University Press, London, 1970, XII+284 pp., figs. and plates, £7.

This monograph aims to refer to all research which has been done on the biology of British species, directing the reader to the original accounts and including brief statements of some of the more interesting results. Many of the species included have a wide geographical distribution and, with its full descriptions of their structure and development, the book will be of interest to marine biologists throughout the world. The drawings, mostly by the author, and photographs are again of the very high standard of those in the previous volume and include six which are reproduced in colour.

SARPSKAYA, T. (ed.), **Proceedings of the 1970 Heat Transfer and Fluid Mechanics Institute** (Held at the Naval Postgraduate School, Monterey, California, June 10-12, 1970), Stanford University Press, Stanford, 1970, XII+370 pp., figs. and tables, $15.00.

New and basic advances in fluid dynamics and heat transfer are contained in this collection of papers presented at the June 1970 meeting of the Institute. This was the twenty-second meeting of a series organized annually through 1968, and biennially thereafter, whose purpose is to make available to engineers and scientists the most recent contributions of research and development organizations throughout the United States, in Canada, Great Britain, and Germany. The texts of 23 contributed papers and the summaries of the five invited General Lectures are included. All were selected to enlarge the understanding of heat transfer and fluid mechanics.

SCHEIDEGGER, A. E., **Theoretical Geomorphology,** 2nd rev. ed., Springer-Verlag, Berlin, 1970, XIV+436 pp., figs. and tables, DM 86.

The surface features of the Earth are commonly split into two categories, the first of which comprises those features that are due to processes occurring inside the solid Earth (endogenetic features) and the second those that are due to processes occurring outside the solid Earth (exogenetic features). Specifically the exogenetic features are treated in the science of *geomorphology.* In this book a brief description is first given of the physiographic facts of geomorphology, after which some of the basic physics is reviewed which is necessary for the understanding of the subsequent exposition. Then, the body of the book presents in sequence the pertinent subjects which are (i) the mechanics of slope formation, (ii) the theory of river action, (iii) drainage basin and large scale landscape development, (iv) the theory of subaquatic effects, (v) nivel effects and (vi) some special features. The present edition is the second of this book. A comparison with the first edition will show that some sections have been extensively rewritten. Chapter V on basin development is completely new, inasmuch as the past eight years have brought an entirely new approach to the problem, based upon statistical mechanics.

SCHWARZBACH, M., **Berühmte Stätten geologischer Forschung** (Bücher der Zeitschrift Naturwissenschaftliche Rundschau), Wissenschaftliche Verlagsgesellschaft MBH, Stuttgart, 1970, X+322 pp., figs. and tables, DM 58.

CONTENTS. - *I: Berühmte Felsen und Felsberge. - II: Berühmte Taler. - III: Berühmte Wasserfälle. - IV: Berühmte Höhlen. - V: Berühmte Seen und Sümpfe. - VI: Berühmte Gletscher. - VII Berühmte Korallenriffe. - VIII: Berühmte Kaps. - IX: Berühmte Zeugen Alpiner Gebirgsbildung. - X: Berühmte Erdbeben. - XI: Berühmte Vulkane. - XII: Berühmte Geysire. - XIII: Berühmte Meteoritenkrater. - XIV: Berühmte Bergwerke. - XV: Berühmte Fossilfunde und Museen.*

Selected Lectures of the Royal Society (Reprinted from the Philosophical Transactions or the Proceedings of The Royal Society), VOL. 3, Academic Press, London, 1970, VI+288 pp., figs., tables and plates, 60s.

The Council of The Royal Society has joined with Academic Press to produce this third volume of lectures, selected from those given before the Society by distinguished scientists during the past few years. Although the lectures have already appeared in the Society's *Proceedings* or in *Philosophical Transactions* it is agreed that a far broader readership would be interested in obtaining a selection of carefully prepared lectures written by experts in their particular fields on subjects covering a wide variety of significant scientific advances. This volume contains major lectures selected from those given during the period March 1967 to December 1968.

SERRE, J.-P., **Cours d'arithmétique** (Series: « *Le Mathématicien* », No. 2), Presses Universitaires de France, Paris, 1970, 188 pp., 12 F.

CONTENTS. - I. Méthodes algébriques. *I: Corps finis. - II: Corps p-adiques. - III: Symbole de Hilbert. - IV: Formes quadratiques sur* Q_p *et sur* Q. *- V: Formes quadratiques entières à discriminant* ± 1. - II. Méthodes analytiques. *VI: Le théorème de la progression arithmétique. - VII: Formes modulaires.*

SETTEMBRINI, P. (ed.), **Medicina 3** (Series: *Enciclopedia Feltrinelli Fischer*, No. 26), Feltrinelli Editore, Milano, 1970, 406 pp., figs., L. 1.500.

Questo volumetto rappresenta una visione d'insieme della scienza medica con par-

ticolare riguardo alle relazioni interne tra le sue discipline e ai suoi legami con le scienze naturali e morali e un'esposizione organica e particolareggiata delle varie specializzazioni cliniche della moderna medicina pratica. Le materie trattate sono: Chirurgia - Dermatologia - Medicine interna - Medicina del traffico - Medicina tropicale - Neurologia - Oculistica - Odontoiatria - Ortopedia - Ostetricia e ginecologia - Otorinolaringoiatria - Pediatria - Psichiatria - Psichiatria dell'età evolutiva.

SIMPSON, G. G., **La géographie de l'évolution,** Masson et C.ie, Paris, 1969, 204 pp., figs., 60 F.

Les sept chapitres qui composent ce livre étaient, à l'origine, des essais ou des conférences, élaborés au cours d'une période de plusieurs années et parus séparément dans des publications très dispersées et, dans certains cas, inaccessibles. Ces essais ont été sélectionnés, et quelque peu modifiés, de façon à présenter une suite logique de thèmes. L'ouvrage, ainsi conçu, donne tout d'abord un bref compte rendu de l'explication de l'évolution, suivi d'un exemple de la récolte et de la codification des données d'information: la preuve de la périodicité dans les restes fossiles. Les problèmes actuels d'observation et d'interprétation de l'évolution sont alors considérés d'une manière plus générale pour les Vertébrés, l'un des groupes les mieux utilisables sous ce rapport. Le reste de ce livre est consacré, en grande partie, à une catégorie de faits évolutifs et de problèmes de première importance qui, jusqu'ici, ont rarement été suffisamment soulignés dans les ouvrages généraux ou dans l'enseignement de l'évolution; ces faits sont les facteurs géographiques dans l'évolution. Les organismes (tous ceux que nous connaissons ou que nous pensons connaître) vivent sur la Terre; ils diffèrent d'un endroit à un autre; en tant que populations, ils se déplacent d'un lieu à un autre, même sur des distances de plusieurs milliers de kilomètres. En général leur adaptation et leur histoire impliquent toujours des facteurs géographiques et l'évolution ne peut pas être bien comprise si l'on néglige ces facteurs.

SLETTEBAK, A. (ed.), **Stellar Rotation** (Proceedings of the IAU Colloquium held at the Ohio State University, Columbus, O., U.S.A., September 8-11, 1969), D. Reidel Publishing Company, Dordrecht, 1970, XVI+356 pp., figs. and tables, Dfl. 70.

The subject of stellar and solar rotation has attracted considerable interest during recent years, by astronomers and hydrodynamicists alike. These Proceedings are a collection of 6 review papers and 33 contributed papers, plus the discussion following the papers, on problems of stellar and solar rotation, as presented in the first international conference to be held on this subject. Sponsored by the International Astronomical Union, The Ohio State University, and the National Science Foundation, the Colloquium attracted 44 scientists from 12 different countries. Organization of the Colloquium and Proceedings is along the following lines: effects of rotation on stellar interiors and evolution; effects of rotation on stellar atmospheres; stellar rotation in clusters, binaries, and special objects; the rotation of the sun. Review papers in these areas were presented by H. A. Abt (Kitt Peak National Observatory), G. W. Collins, II (Ohio State University), R. H. Dicke (Princeton University), M. Plavec (Ondrejov Observatory), I. W. Roxburgh (Queen Mary College), and A. Slettebak (Ohio State University).

SMITH, H. M., **Principles of Holography,** John Wiley & Sons Ltd., London, 1969, XII+240 pp., figs., 94s.

This monograph discusses the history, theory, practice, and applications of holography. Holography, invented in 1948, recently underwent a complete renaissance due to some important breakthroughs at The University of Michigan. Because of this renaissance and the resulting proliferation of literature on the subject, this book's prime purpose is to offer a unified, comprehensive treatment under one cover. The author simplifies and clarifies holographic terminology — which remains consistent and accurate throughout the book. The theory of all aspects of holography is treated as simply as possible. A one-dimensional analysis is used wherever possible so that the operations are not needlessly bulky. So that mathematics does not cloud the basic principles being discussed and so that it remains simple and understandable, obvious approximations are

used when possible. Some topics are not treated completely generally just for the sake of generality—rather the most common situations are treated. The simple, clear diagrams and figures leave no doubt about the definition of any symbol. This lucid monograph will enable any photographic or optical engineer to understand holography and, therefore, to deal intelligently with holographic problems—whether in lab work or management level decisions. However, it is assumed that the reader has a knowledge of several aspects of optics, such as modulation transfer functions, spatial frequency, phase coherence, and diffraction.

SOLARI, G. C. and GONZALES, G. C., **Los médicos y el control de la natalidad** (Encuesta sobre la opinión de obstetras y ginecólogos), (*Cuadernos de Discusión*, No. 5), Desal, Santiago de Chile, 1969, 102 pp., tables, $2.20.

Los obstetras y ginecólogos constituyen un factor de influencia importante en el comportamiento anticonceptivo de la población: las tres cuartas partes de los especialistas que trabajan en los principales hospitales de Santiago plantean a sus pacientes el tema de la regulación de la natalidad, y en cuanto a los restantes, sólo lo hacen cuando es solicitado su consejo. En este libro se entregan los resultados de una encuesta realizada por DESAL-CELAP en cinco instituciones hospitalarias de la capital chilena a fin de conocer las opiniones y actitudes de los médicos en torno a las técnicas anticonceptivas.

SOURIAU-HOEBRECHTS, C., **La presse maghrébine** (Libye, Tunisie, Maroc, Algérie. Evolution historique. Situation en 1965. Organisation et problèmes actuels), Centre National de la Recherche Scientifique, Paris, 1969, 370 pp., 45 F.

Ce livre, que Christiane Souriau a écrit, est un très bon résumé de l'histoire des journaux du Maghreb jusqu'en 1965, qu'elle a complété ensuite par un coup d'oeil sur leur évolution de 1966 à 1968. Elle a en outre consacré un chapitre fort pertinent aux conditions matérielles et morales dans lesquelles vit la presse maghrébine et ces deux développements éclairent et étoffent l'analyse de 1965.

CONTENTS. - I. L'évolution historique. - II. La presse maghrébine en 1965. *I: La presse libyenne. - II: La presse tunisienne. - III: La presse algerienne. - IV: La presse marocaine.* - III. Organisation et problèmes actuels. - Conclusion.

SRB, A. M., OWEN, R. D. and EDGAR, R. S. (eds.), **Facets of Genetics** (Readings from Scientific American), W. H. Freeman and Company Ltd., Berkshire, 1970, XII+354 pp., figs., 46s.

This collection is divided into five parts, each beginning with an introduction intended to provide some degree of integration and focus for the articles. Each illustrates how genetics illuminates our study of life at a different level: Part I is concerned with the processes of inheritance; Part II, with the nature of genetic material and how it works in cells; Part III, with the development of individual organisms; Part IV, with the diversity of living things that has occurred in the course of evolution; and Part V, with our efforts toward understanding and serving mankind.

TEMAM, R., **Analyse numérique** (Series: « *Le Mathématicien* », No. 3), Presses Universitaires de France, Paris, 1970, 120 pp., 10 F.

CONTENTS. - *I: Résolution approchée d'équations elliptiques linéaires. - II: Applications. - III: Une équation elliptique non linéaire.*

The Growth Points of Physics (Proceedings of the Florence Inaugural Conference, Florence, Italy, 8-12 April 1969), European Physical Society, Carouge-Geneva (Reprinted from « Nuovo Cimento », Bologna), 1970, XL+568 pp., figs. and tables, No price stated.

Contents. - *I:* G. Bernardini, *Spreech at the Inaugural Session.* - *II:* V. F. Weisskopf, *Physics in the XX-th Century.* - *III:* P. M. S. Blackett, *The Old Days of the Cavendish.* - *IV:* E. Amaldi, *Physics and Education.* - *V:* H. B. G. Casimir, *Physics and Industry.* - *VI:* G. Chiarotti, *Colour Centres and Optical Properties of Alkali Halides.* - *VII:* W. Czyz, *High-Energy Collisions of Hadrons and Nuclei.* - *VIII:* F. J. M. Farley, *The Status of Quantum Electrodynamics.* - *IX:* G. B. Field, *Cosmic Background Radiation and its Interactions with Cosmic Matter.* - *X:* J. Friedel, *On the Plastic Properties of Materials.* - *XI:* A. Hewish, *Pulsars.* - *XII:* H. J. Lipkin, *The Spectrum of Hadrons.* - *XIII:* W. Marshall and S. W. Lovesey, *The Use of Neutrons in the Study of Solids and Liquids.* - *XIV:* N. F. Mott, *Conduction in Solids.* - *XV:* R. S. Pease, *Research on Controlled Nuclear Fusion.* - *XVI:* R. Pellat et G. Laval, *Les instabilités du plasma.* - *XVII:* R. Penrose, *Gravitational Collapse: the Role of General Relativity.* - *XVIII:* E. R. Pike, *Photon Statistics.* - *XIX:* A. Rahman, *Computer Studies of Condensed Matter.* - *XX:* E. E. Salpeter, *Highly Condensed Matter in Stars.* - *XXI:* E. Schatzman, *Physical Processes Related to the Problem of Nucleosynthesis at the Surface of Stars.* - *XXII:* K. Schindler, *Interplanetary and Magnetospheric Plasma.* - *XXIII:* D. W. Sciama, *The Recent Renaissance of Observational Cosmology.* - *XXIV:* M. J. Seaton, *Electron Collisions.* - *XXV:* D. Sette, *Research on Critical Phenomena.* - *XXVI:* V. M. Strutinsky, *The Fission Process.* - *XXVII:* G. Toraldo di Francia, *Some Recent Progress in Classical Optics.* - *XXVIII:* W. F. Vinen, *Macroscopic Quantum Phenomena.* - *XXIX:* H. Zwicker, *Use of Laser Light for Investigations of Atoms, Molecules and Plasmas.* - *XXX:* J. M. Winter, *Multiple Quantum Transitions.* - *XXXI:* L. Woltjer, *Radiogalaxies and Quasi-Stellar Objects.* - *XXXII:* R. V. Khokhlov, *Nonlinear Optics of Ultra-Short Pulses.* - *XXXIII:* M. F. Perutz, *The Atomic Structure of Biological Catalysts.* - *XXXIV:* P. Von Brentano, *Isobaric Analogue Resonances in Nuclei.*

TIMMONS, C. J. (ed.), **Modern Reactions in Organic Synthesis,** Van Nostrand Reinhold Company, London, 1970, vi+312 pp., tables, £ 5.10.

The book covers, in seven sections, the importance and utility of a wide variety of new methods that have been recently developed and applied in synthesis, degradation, rearrangement and structural investigation. Description of each method is backed up by a wide diversity of original ideas, examples and principles, with references to the original literature. The notation and references are international, the presentation and treatment are clear and orthodox, and the contributing authors are all men of high standing in their chosen fields.

Contents. - *I: Introduction.* - *II: Reduction Methods,* by R. F. Evans. - *III: Oxidation Methods,* by R. G. R. Bacon. - *IV: Synthetic Uses of Free Radicals,* by G. B. Gill. - *V: Electrochemical Methods,* by R. Brettle. - *VI: Photochemical Methods,* by E. V. Blackburn and C. J. Timmons. - *VII: Syntheses of Aromatic Compounds,* by J. W. Barton. - *VIII: Recent Methods for Forming Heterocyclic Rings,* by G. W. H. Cheeseman.

VALSON, C.-A., **La vie et les travaux du Baron Cauchy,** Librairie Scientifique et Technique Albert Blanchard, Paris, 1970, xxxvi+178 pp., 32 F.

Cette étude bio-bibliographique demeure la monographie la plus développée que nous possédions sur l'un des mathématiciens les plus éminents du XIX^e siècle, et sa consultation préalable est indispensable avant toute étude d'un aspect quelconque de la vie et de l'oeuvre de Cauchy.

VELIMIROVIC, M., **Atlante Atomico,** Edizioni Capitol, Bologna, 1970, 176 pp., tables, L. 10.000.

Le pubblicazioni specialistiche hanno affrontato l'argomento della scienza atomica con un linguaggio e con proposizioni non accessibili al lettore comune. Il valore di questo *Atlante Atomico* risiede nel rappresentare il microcosmo e i microprocessi con un linguaggio comprensibile a chiunque abbia una conoscenza anche limitata delle leggi fisiche e matematiche, senza formule e discorsi accademici. Inoltre tutte le leggi e i processi

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VERNIN, G., **La chromatographie en couche mince** (Techniques et applications en chimie organique), Dunod, Paris, 1970, XIV+178 pp., figs. and tables, 39 F.

CONTENTS. - *I: Généralités. - II: Techniques expérimentales: appareillage, adsorbants et éluants. - III: Techniques expérimentales: développement, révélation, conservation et reproduction des plaques. - IV: Aspects analytiques de la chromatographie en couche mince. - V: Fondements théoriques de la chromatographie en couche mince. Relations entre le comportement chromatographique et la structure chimique. - VI: Les applications de la chromatographie en couche mince à la chimie organique.*

VON KOVACS, E., **Raketen-Einmaleins** (Einführung in die elementaren Grundlagen der Himmelsmechanik und der Raketentechnik), (Dümmlerbuch 4161), Ferd. Dümmlers Verlag, Bonn, 1970, 276 pp., figs. and tables, DM 24.80.

CONTENTS. - *I: Zur Geschichte der Raumfahrt. - II: Was sind Raketen? - III: Ballistik. - IV: Raketenmotor. - V: Rakete. - VI: Schrifttum. - VII: Berechnungsbeispiele.* - Tafeln im Anhang.

WACKENHEIM, A. and ESCUDERO, L., **Neuroradiologie** (Encephalographie gazeuse fractionnée normale et pathologique), Editions Doin, Deren & C.ie, Paris, 1969, 374 pp., figs., 115 F.

Les auteurs présentent un précis d'encéphalographie gazeuse fractionnée. Après un exposé de la technique « strasbourgeoise » de l'EGF (avec tomographie systématique de la fosse postérieure, 1958), les auteurs décrivent dans plusieurs chapitres l'anatomie radiologique normale des structures ventriculaires et cisternales. Ils consacrent ensuite plusieurs chapitres à la méthodologie analytique des clichés et plusieurs chapitres aux grandes synthèses de diagnostic en matière de tumeurs cérébrales, de traumatismes, de malformations, d'infections, de parasitoses... etc. Nul doute que l'intention des auteurs fut avant tout didactique. Dans les moindres détails l'ouvrage reste schématique avec le souci permanent de l'enseignement, de la démonstration. Ce précis s'adresse à ceux qui débutent dans la spécialité neuro-radiologique, aux élèves des certificats d'électro-radiologie (diagnostic), de neurologie, de neurochirurgie, de psychiatrie et de traumatologie cranio-cérébrale.

WALES, A. P. (ed.), **World Directory of Booksellers** (An International Guide to Booksellers), Alexander P. Wales Publishers, London, 1970, XVI+912 pp., £9.15.6.

Con la pubblicazione di questa nuova opera di consultazione, l'editore A. P. Wales mette in evidenza l'importante settore del commercio dei libri: quello rappresentato dalle librerie. L'opera è di consultazione assai facile ed è stata suddivisa per Paesi. Le varie città sono state quindi elencate in ordine alfabetico e sotto ciascuna di esse, pure in ordine alfabetico, seguono le librerie. Fanno eccezione a questa disposizione le librerie degli Stati Uniti che sono state catalogate per Stati. In generale, i particolari forniti per ciascuna libreria riguardano: Vendita al minuto – Vendita all'ingrosso – Distribuzione – Importazione – Esportazione – Antiquariato – Vendita di paperbacks – Libreria universitaria. Suddivisi in 30 classificazioni, vengono elencati i tipi di libri che la libreria tratta. Le librerie generiche, che trattano libri su qualunque soggetto, sono chiaramente contrassegnate. Se la libreria tratta libri redatti in lingue straniere, oltre che libri di produzione nazionale, queste sono indicate. Introduzione e norme per l'uso dell'Annuario in inglese, Francese, Italiano, Spagnolo e Tedesco. Spiegazione dei simboli e delle abbreviazioni nelle suddette lingue.

WEDEPOHL, K. H. (ed.), **Handbook of Geochemistry,** Vol. II/2, Springer-Verlag, Berlin, 1970, IV+668 pp., figs., DM 212.

Standard Sections of Chapters. (Chapters have the atomic numbers of the elements). A) Crystal chemistry. - B) Isotopes in nature. - C) Abundance in cosmos, meteorites, lunar materials and tektites. - D) Abundance in rock-forming minerals (phase equilibria), minerals. - E) Abundance in common igneous rock types. - F) Behavior in magmatogenic processes (pegmatites, gas transport, ore deposition etc.) - G) Behavior during weathering and alteration of rocks. - H) Solubilities of compounds which control concentrations of the element in natural waters; adsorption processes; valence states in natural environments. - I) Abundance in natural waters and in the atmosphere. - K) Abundance in common sediments and sedimentary rock types. - L) Biogeochemistry. - M) Abundance in common metamorphic rock types. - N) Behavior in metamorphic reactions. - O) Relations to other elements, economic importance etc.

WINAND, J., **Aspects qualitatifs et quantitatifs du métabolisme lipidique de la souris normale et de la souris congénitalement obèse** (Etude des lipides par radiochromatographie gazeuse), Editions Arscia S.A., Bruxelles, 1970, 298 pp., figs. and tables, 550 F.B.

The purpose of this work is shown at the beginning of the first chapter. The introduction outlines the literature treating some of the mechanisms which are capable of controlling the quality and the quantity of lipids accumulated in the adipose tissue and the liver of the normal mouse, and of two types of mice which spontaneously present the triad obesity-hyperinsulinism-diabetes. The hyperinsulinism of the obese-hyperglycemic mouse of Bar Harbor is already highly pronounced at the age of six weeks. The hyperinsulinism of the New Zealand obese mouse is less accentuated. General discussions and relative conclusions of the four principal parts of this work are presented in chapters VII, X, XIV and XVII.

ZÁRUBA, Q. and MENCL, V., **Landslides and their Control,** Elsevier Publishing Company, Amsterdam, 1969, x+206 pp., figs., Dfl. 40.

This monograph presents a concise survey of theoretical and practical aspects of slope movements, and the methods of their investigation, prevention and control. Numerous examples of landslides on Czechoslovak and foreign territories, and of their treatment, are referred to for a better understanding of this phenomenon. Of interest to: Engineering geologists, civil engineers concerned with the problems of landslider, workers of geological surveys, and institutes. The book is also suitable as a textbook for university students.

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Ricorre quest'anno il centesimo anniversario della nascita di Federigo Enriques, che fu tra i fondatori di questa Rivista.

" Scientia " ritiene di assolvere a un obbligo preciso ricordando ai suoi Lettori la figura e l'opera dell'insigne studioso che, nella prima metà di questo secolo, ebbe una parte di non poco rilievo nelle vicende della cultura italiana: non di questa soltanto, del resto, poichè per tutta la vita l'Enriques si studiò di fare udire la voce degli studiosi del nostro Paese nei grandi dibattiti scientifici che si andavano intrecciando nel mondo. Questo fu, anzi, uno dei motivi ispiratori di " Scientia ", che Egli intese come tramite che potesse valicare confini e superare distanze e diffidenze. L'altro, dettato anch'esso da Federigo Enriques, fu l'esigenza di comporre le diverse discipline scientifiche in una unità nella quale ciascuna ritrovasse la propria ragion d'essere e i propri fini; i procedimenti delle discipline specialistiche, infatti, avrebbero potuto acquistare pieno senso soltanto se fosse intervenuta ad illuminarli una visione ben più ampia: il pensiero scientifico, appunto.

Enriques, in tal modo, proponeva una idea nuova della scienza ed una nuova immagine dello scienziato: idea e immagine che si vanno riaffacciando in questi anni, alla luce di più recenti esperienze.

Nelle pagine che seguono il Lettore troverà un articolo nel quale Lucio Lombardo Radice rievoca, proponendone una valutazione critica, le battaglie sostenute dall'illustre matematico e filosofo per far valere le sue concezioni in una età nella quale la cultura italiana era in larghissima parte soggetta all'autorità neo-idealistica.

Teniamo a ringraziare Lucio Lombardo Radice per questa sua lucida analisi, così come ringraziamo l'Ing. Giovanni Enriques, figlio del compianto Maestro, e la Casa Editrice Zanichelli per le fotografie e il materiale biografico fornitoci.

This year marks the hundredth anniversary of the birth of Federigo Enriques, who was one of the founders of this Review.

"Scientia" considers that it is fulfilling a definite obligation in recalling for its readers the personality and work of the eminent scholar who, during the first half of this century, played a part of no small importance in the sphere of Italian culture; but not only in this sphere, since, throughout his life, Enriques strove to make the voice of Italian scholars heard in the great scientific debates in which the world was becoming involved. Indeed, this was one of the motives that inspired "Scientia", which he saw as a way of being able to cross boundaries and overcome both distance and diffidence. The other motive, also dictated by Federigo Enriques, was the need to compound the different scientific disciplines into a unity in which each might find its own raison d'être and its own aims; in fact, the procedures of the specialistic disciplines could attain their full meaning only if they were illuminated through a far broader view: that of scientific thought.

In this way, Enriques proposed a new idea of science and a new image of the scientist: an idea and an image that are being presented once again in these years, in the light of the most recent experiences.

In the pages that follow, the reader will find an article in which Lucio Lombardo Radice recalls and offers a critical appraisal of the battles waged by the eminent mathematician and philosopher for an appreciation of his conceptions in an age in which Italian culture was very largely subject to neo-idealistic authority.

We wish to thank Lucio Lombardo Radice for this clear analysis, just as we also wish to thank the publishing firm of Zanichelli and Ing. Giovanni Enriques, the son of the late Master, for supplying us with photographs and biographical material.

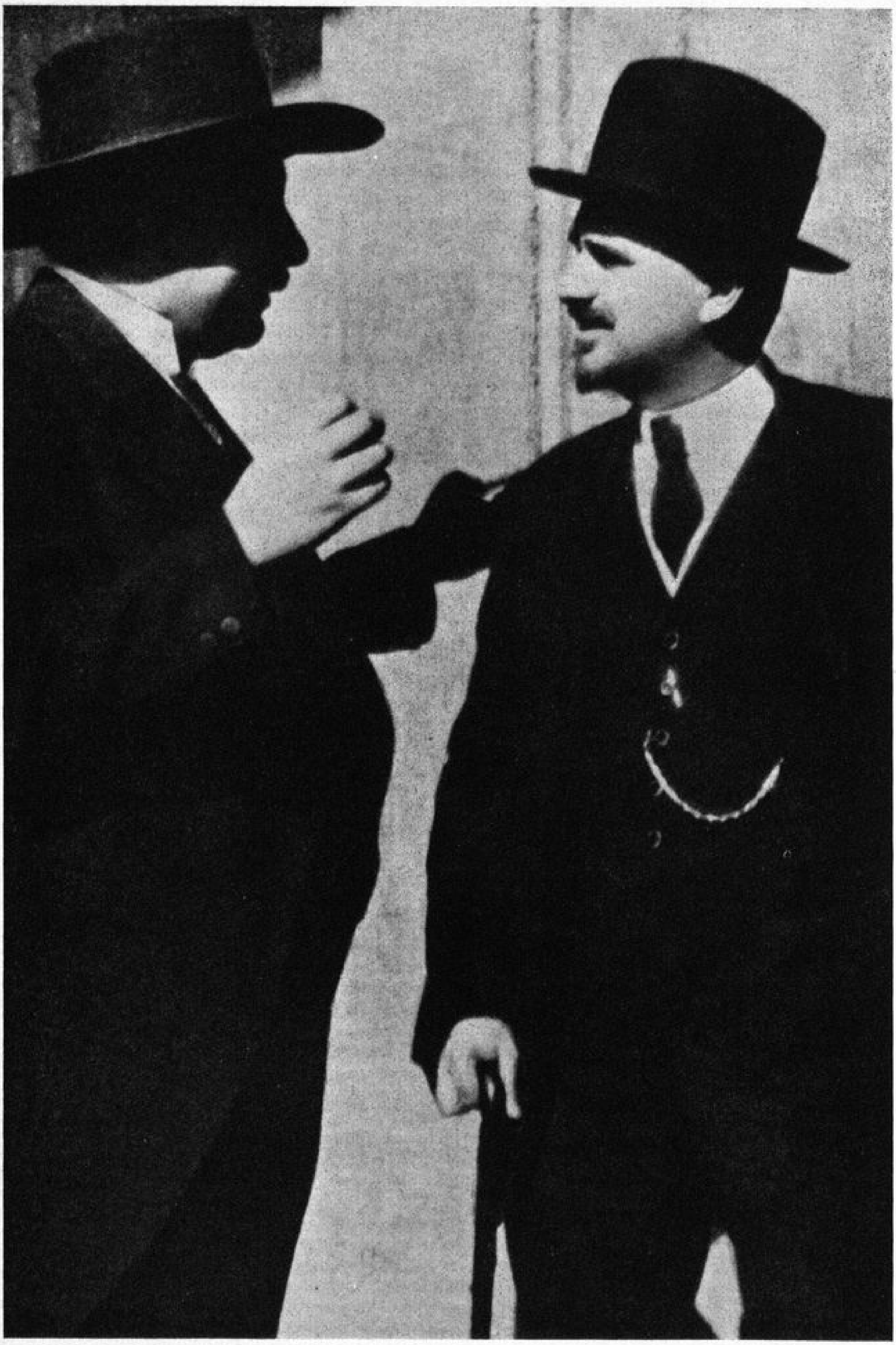

Fig. 1

Federigo Enriques (a destra) fotografato con Albert Einstein nel 1921 (Archivio Zanichelli)
Federigo Enriques (on the right) photographed with Albert Einstein in 1921 (Zanichelli Archives)

PER LA FILOSOFIA E LA DIDATTICA DELLE SCIENZE

BATTAGLIE, SCONFITTA E VITTORIA DI FEDERIGO ENRIQUES

Proprio in vista di questo breve saggio su Enriques filosofo, epistemologo, storico della scienza, didatta e pedagogista, ho dovuto scorrere le schede dei soli *volumi* scritti da Enriques in possesso della Biblioteca dell'Istituto Matematico della Università di Roma, ora intitolato a Guido Castelnuovo, nel quale Enriques insegnò per quasi un quarto di secolo, fino alla morte[1]. Sono rimasto, ancora una volta, pieno di stupore per la mole, e ancora di più per la *estensione*, dell'opera di quel grande uomo di cultura. Ancora una volta, però, nel ripercorrere le pagine delle principali opere di Federigo Enriques, mi sono reso conto che quella quasi stupefacente *molteplicità* di campi di lavoro e di ricerca è, in definitiva, coerente *unità*. Come i filosofi presocratici a lui così congeniali, o come i « filosofi naturali » del Rinascimento, Enriques si occupò in modo originale di 'campi' che siamo abituati a considerare separati da confini netti, o addirittura privi di frontiera comune!, perchè la sua impostazione era unitaria, perchè quei 'campi' non erano per lui discipline specialistiche distinte, ma determinazioni diverse e congiunte di *un* pensiero, di *una* visione del mondo.

La scienza come cultura. — Il pensiero unificante di Enriques è quello di un « concetto più largo della filosofia, come forma di attività implicata in ogni prodotto del pensiero » (1911), è quello della « rilevanza filosofica » della ricerca e della scoperta scientifica (in particolare, matematica e fisica). Questo concetto centrale si determina, nel pensiero di Enriques, come affermazione della inscindibilità del rapporto tra filosofia come *Weltanschauung*, e « filosofia naturale ».

Da un lato, Enriques è convinto - sin dalla prima giovinezza, di poter trovare « soltanto nella filosofia naturale » una « risposta (parziale, d'accordo) ai problemi che lo avevano affascinato negli anni del Liceo » (secondo la testimonianza di un colloquio romano resa da Giuseppe Scorza Dragoni). Enriques afferma instancabilmente, in un mezzo secolo di battaglia filosofica, che il pensiero occidentale è permeato dai temi che ad esso sono stati posti dalla ricerca scientifica, da Pitagora e Zenone fino a Kant e a Hegel. Non a caso il volume nel quale, nel 1938, raccoglie (con l'aiuto di Attilio Frajese) conferenze e lezioni che offrono una sintesi di quaranta anni di ricerca, ha per titolo: *Le Matematiche nella storia e nella cultura*[2].

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[1] Un bellissimo e completo « Elenco cronologico delle pubblicazioni di F.E. » è stato redatto, dopo la morte del Maestro, da Luigi Campedelli e Adriano Barlotti. Lo si può ora trovare in Appendice al volume di F.E., *Natura, regione e storia* (Antologia di scritti filosofici a cura di L. Lombardo Radice) Boringhieri, Torino, 1958.

Le Matematiche sono cultura, sono parte essenziale della cultura, in particolare sottendono tutti i « grandi sistemi metafisici ». Al filosofo, Enriques non chiede di diventare un « tecnico » della scienza; al filosofo egli « domanda molto meno e molto di più »: « la chiara intelligenza delle idee più elementari ch'egli non avrà mai sufficientemente approfondite, e il significato filosofico - che spesso egli crede d'insegnare ed invece deve apprendere da chi ha più lungamente meditato su tale soggetto - dei principii e dei metodi della scienza ».

Dall'altro lato, Enriques è filosoficamente esigente collo scienziato, al quale chiede di essere un « filosofo naturale », un critico dei fondamenti della sua scienza, non un puro tecnico. Insomma, per usare le parole colle quali egli chiude il capitolo, « Matematiche e filosofia » del libro sopra citato, egli chiede all'uno e all'altro, al filosofo e allo scienziato, « che non rompa il nesso delle correnti spirituali che convergono nel movimento del pensiero, e perciò che intenda i motivi diversi onde il movimento stesso si origina ». Chiede, all'uno e all'altro, di non far degenerare in « filosofia dei compartimenti-stagno » l'« universalismo umanistico » proprio della cultura.

COLLOCAZIONE STORICA DEL PENSIERO DI FEDERIGO ENRIQUES. — Poichè questo: l'unità della cultura, il posto occupato dalle scienze nella filosofia, nella storia, nella formazione del singolo - era il centro del suo pensiero, Enriques *doveva* - necessariamente! - condurre una battaglia su molti fronti: filosofico, epistemologico, didattico, storico.

Sarebbe un far torto alla « visione dinamica della scienza » di Enriques, che lo « portò naturalmente sul terreno della storia », non compiere uno sforzo per una collocazione storica del pensiero stesso di Enriques. Enriques, originalissimo, non è però un solitario. Quello che abbiamo definito come l'elemento unificante del suo pensiero - il rapporto scienza-filosofia, scienza-cultura-storia - è tratto caratteristico della società scientifica (o per dir meglio dei suoi esponenti più consapevoli e avanzati) italiana ed europea tra la fine del secolo scorso e l'inizio della prima guerra mondiale.

Possiamo affermare, bibliografia alla mano!, che il suo cammino filosofico Enriques lo compie nel giro di quei quindici-venti anni. Il suo primo scritto di filosofia è del 1901, i *Problemi della scienza* del 1906, *Scienza e razionalismo* del 1912.

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[2] La prima edizione è del 1938, Zanichelli, Bologna. La Casa Editrice Zanichelli ha preannunciato una ristampa entro il 1971.

Ebbene, se consideriamo gli scienziati italiani di primo piano nel primo decennio del secolo, troviamo tutta una serie di uomini molto impegnati in problemi filosofici, sociali, educativi, storici; lo scienziato tecnico-puro è di là da venire. Qualche nome alla rinfusa: i matematici Peano, Padoa, Castelnuovo, Severi, Gaetano Scorza, il fisico Righi, il chimico Ciamiciàn, il medico Murri. *Scientia*, fondata - con altri - da Enriques per facilitare la immissione nella circolazione culturale e filosofica dei risultati e dei metodi nuovi delle scienze, troverà quindi collaboratori congeniali in tutti i campi.

In quel quindicennio, si conclude la grandiosa opera di uno scienziato-filosofo come Henri Poincaré, ha inizio la rivoluzione scientifica che porta il nome di Albert Einstein, « filosofo naturale » e non certo matematico puro.

Se guardiamo poi verso il firmamento filosofico di quella stagione storica, troviamo Bergson e James, Russell e Mach, Croce e Gentile. Ora, in tutte le correnti di pensiero che si fronteggiano: positivisti e idealisti, logicisti e pragmatisti, convenzionalisti e razionalisti, il problema del rapporto scienza-filosofia occupa un posto centrale.

Il fatto è che il progresso scientifico non è, in quegli anni, una avanzata in linea retta; al contrario, giunge a compimento una grande rivoluzione scientifica, vengono messi in questione i *fondamenti* stessi della scienza (in particolare della matematica e della fisica). Lo scienziato, il *vero* scienziato, *deve* fare i conti colla filosofia in una epoca siffatta, proprio mentre affronta i problemi che la scienza gli pone; il filosofo, l'autentico filosofo, *deve* approfondire le « idee elementari » (fondamenti) delle scienze che vengono poste in discussione.

IN CHE COSA CONSISTE L'ORIGINALITÀ DI ENRIQUES? — Anche Enriques - come Russell o Hilbert, come Peano o Poincaré - fa filosofia, innanzitutto, per esigenze interne della scienza.

Ci limitiamo qui a qualche accenno[3]. Nei *Problemi della scienza* (1906), Enriques fa una analisi operativa della misura del tempo assai vicina a quella che contemporaneamente, senza che il nostro autore ne fosse a conoscenza, conduce fino in fondo Albert Einstein. La analisi di Enriques va nella stessa direzione di quella di Einstein, contro la idea di « tempo assoluto »: è la filosofia della relatività (ristretta) che emerge dall'interno della meccanica e della fisica. Un anno dopo, nel 1907, in un altro scritto, Enriques espone idee congeniali a quelle della relatività generale, che Einstein formulerà in modo compiuto e rigoroso un decennio dopo. Enriques considera infatti la meccanica newtoniana come una « scienza locale », vera con grande approssimazione solo se la « durata e la velocità » di un movimento sono « molto piccoli rispetto a certi dati relativi al riferimento ». Al di là di queste ipotesi, « si creeranno le premesse di *meccaniche non* newtoniane che tenderanno a coincidere con la *meccanica newtoniana* col diminuire delle velocità considerate ».

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[3] Ho sviluppato assai più ampiamente l'argomento nella Introduzione al volume citato nella Nota [1] (« F.E. e la filosofia naturale agli inizi del secolo 20-mo »).

Già in queste analisi determinate è implicita una critica del « fatto scientifico » inteso come *dato*, e della scienza come protocollo di dati (che è critica, si badi bene, tanto contro il neo-empirismo quanto contro il neo-idealismo: Croce aveva bene mutuato da Mach la sua concezione della scienza come gnoseoprassi, come catalogazione arbitraria ma utile!) Questa critica occupa tutto un Capitolo (il secondo), dei *Problemi della scienza.*

Fatto e ipotesi, dato e rappresentazione globale sono inscindibili, sostiene Enriques. La verifica sperimentale non è mai soltanto verifica di dati, è verifica delle immagini nelle quali inquadriamo i dati; di qui, la necessità di « modificare la rappresentazione geometrica adottata », quando si palesi « non adeguata ».

« La realtà non è un dato puro, ma qualcosa di costruito mercè l'attività razionale coordinatrice »: ecco il fondamento interno alla scienza del *razionalismo* filosofico di Federigo Enriques.

Enriques diede della sua filosofia almeno tre definizioni: *positivismo critico, razionalismo sperimentale, razionalismo storico.* Queste tre definizioni ci sembrano corrispondere bene a tre successive fasi evolutive del suo pensiero.

La prima ricerca epistemologica, *Sulla spiegazione psicologica dei postulati della geometria* (1901), risente della influenza del positivismo 'classico' per l'insistente richiamo ai sensi; si tratta però già di un positivismo *critico*, perchè il dato sensoriale è organizzato nelle forme assiomatiche. (Enriques riconduce gli assiomi della geometria metrica alla mano, al 'tatto speciale'; quelli della geometria proiettiva all'occhio; quelli della topologia a un 'senso tattile generale'). Il raccordo sensi-assiomi si svilupperà nel nesso geometria-fisica, caratteristico dell'Enriques.

Contro il convenzionalismo, e in definitiva (ci sembra) anche contro il 'formalismo puro', Enriques è fautore di « una geometria che faccia corpo con la fisica e trovi il suo prolungamento naturale in questa disciplina ».

Questa ci sembra una posizione assai originale dell'Enriques. Da un lato, egli accetta fino in fondo il metodo assiomatico, dall'altro ritiene 'interessanti' (scientificamente fecondi) solo quei sistemi di assiomi che caratterizzano spazi geometrici immagini di realtà fisiche. Questo ci sembra il sottosuolo scientifico del suo *razionalismo sperimentale*; la rispondenza, con approssimazione sempre perfettibile, dei fenomeni fisici a quadri formali, puramente intellettuali (come sono i sistemi di assiomi).

Enriques parla della sua « fede nella intelligibilità delle cose »; potremmo dire che egli crede a una *natura secondo ragione.* Questa 'ragione' non è però più per lui un assoluto, come era stata per il suo grande meastro Galileo. È una ragione che si evolve e si determina storicamente: di qui la posizione più matura di Enriques, quella del *razionalismo storico*, Da questo angolo visuale, la interruzione della attività filosofica, possiamo dire dal 1912 al 1938, non appare più una vera e propria interruzione; perchè la ricerca filosofica si prolunga e si incarna nella ricerca storica, che tanto occuperà Enriques negli anni venti e negli anni trenta.

Va aggiunto poi, crediamo, che il rinnovato slancio filosofico dell'ormai anziano studioso, e pensiamo soprattutto a *Causalité et déterminisme dans la philosophie et l'histoire des sciences* (1941), si collega colla presa di coscienza di una nuova crisi dei fondamenti (fisica nucleare, meccanica ondulatoria).

Enriques, in conclusione, non ci appare come un pensatore solitario, distaccato dalla sua epoca; nel suo tempo, e nei temi del suo tempo, egli si colloca pienamente, ma in una posizione molto originale. Egli polemizza con positivisti e idealisti, con logicisti ed empiristi, con pragmatisti e convenzionalisti, e nello stesso tempo realizza una sintesi molto originale di elementi tratti da queste correnti di pensiero. Come ha notato acutamente Guido Zappa, in un suo lungo scritto su « Enriques filosofo » (*Studium*, 1959), « di fronte alle tendenze antirazionali del pragmatismo, di fronte alle tendenze antiscientifiche dell'idealismo, egli reagisce, ma nello stesso tempo assorbe qualche elemento da queste correnti e lo incorpora nel suo pensiero ».

PERCHÈ SCONFITTA, PERCHÈ VITTORIA. — Mi sono proposto di delineare sinteticamente la collocazione storica di Enriques pensatore, e perciò, volutamente, anche se con rincrescimento, mi limito a pochissimi accenni alla sua operosità come storico della scienza e come educatore scientifico. Ho già detto che la storia della scienza (alcuni volumi sono in collaborazione col De Santillana, col Frajese, col Mazziotti) occupa un posto assai notevole negli ultimi venticinque anni della vita di Enriques. Pur manifestando una speciale passione per la storia della scienza greca, per la ricostruzione dall'interno dei procedimenti mentali dei geometri Greci, Enriques è uno dei pochi storici della matematica italiani (stavo per dire il solo) che abbia affrontato l'intero arco di sviluppo della matematica, studiando anche - storicamente - gli scienziati, se non proprio contemporanei a lui, della generazione alla sua precedente.

Il 'filo conduttore', come si è detto, è ancora una volta, l'idea che la matematica (la scienza) è parte integrante della cultura, e della storia *tout court*. Questo è anche il filo conduttore della attività di Enriques nel campo della pedagogia e della didattica della matematica e delle scienze. Attività enorme: testi universitari, che sono veri e propri 'gioielli'; manuali per le scuole medie (in collaborazione con Amaldi), che ancora oggi « fanno testo »; la Società Mathesis, il *Periodico di Matematiche*, e poi conferenze, seminari, articoli, battaglie perchè la scienza non fosse relegata in un ghetto, ma fosse parte integrante degli studi umanistici.

La soluzione data dalla riforma Gentile (1923), almeno per la istruzione secondaria superiore, fu tutt'altra: fu quella corrispondente alla concezione della scienza come non-cultura propria del neo-idealismo tanto del Croce quanto del Gentile (anche se non di tutti i loro discepoli). La prevalenza della tesi idealistica non fu un fatto solo filosofico-teorico; fu una scelta politica, istituzionale, statuale. Per la epistemologia, per la storia della scienza, per la pedagogia e didattica della scienza nessuna cattedra universitaria, quasi nessun istituto speciale; posizione subalterna del liceo (cosiddetto) scientifico rispetto al liceo classico; niente scienze sperimentali per cinque anni di ginnasio, e poche ore di matematica. Le riviste e le case editrici principali seguivano l'indirizzo prevalente, così che ben poco arrivava in Italia anche dei progressi (vertiginosi!) della ricerca epistemologica, storico-scientifica, di didattica e di pedagogia delle scienze che venivano fatti in altri paesi.

In altra occasione[4] ho cercato di analizzare con attenzione lo scontro di Enriques con Croce e Gentile, e sono pervenuto alla conclusione che da tutte e due le parti non vi fu vera e propria capacità di 'egemonia', cioè capacità di far proprie e portare avanti dal proprio punto di vista le ragioni dell'avversario. Croce e Gentile non vollero riconoscere valore di conoscenza e di cultura alla ricerca scientifica; Enriques non seppe far proprio il motivo profondo della dialettica hegeliana.

Comunque, il vincitore fu allora l'idealismo; Enriques fu lo sconfitto.

Venticinque anni dopo la morte di Enriques, la situazione si è profondamente mutata. Non è il caso di chiedersi qui perchè, per merito di chi; vogliamo solo constatare il fatto che le idee di Federigo Enriques della unità dialettica tra scienza e filosofia, della scienza come parte integrante dell'umanesimo, della scienza come aspetto e tessuto fondamentale della storia, hanno oggi, in un certo senso e almeno al livello della alta cultura, il sopravvento. Di ciò vi è consapevolezza, e io sono convinto che, al di là della occasione del centenario della nascita, le idee di Enriques ritorneranno a circolare nella cultura italiana, in cerchie sempre più larghe.

Roma, Università, Istituto Matematico.

L. Lombardo Radice

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[4] Vedi lo scritto citato in [3].

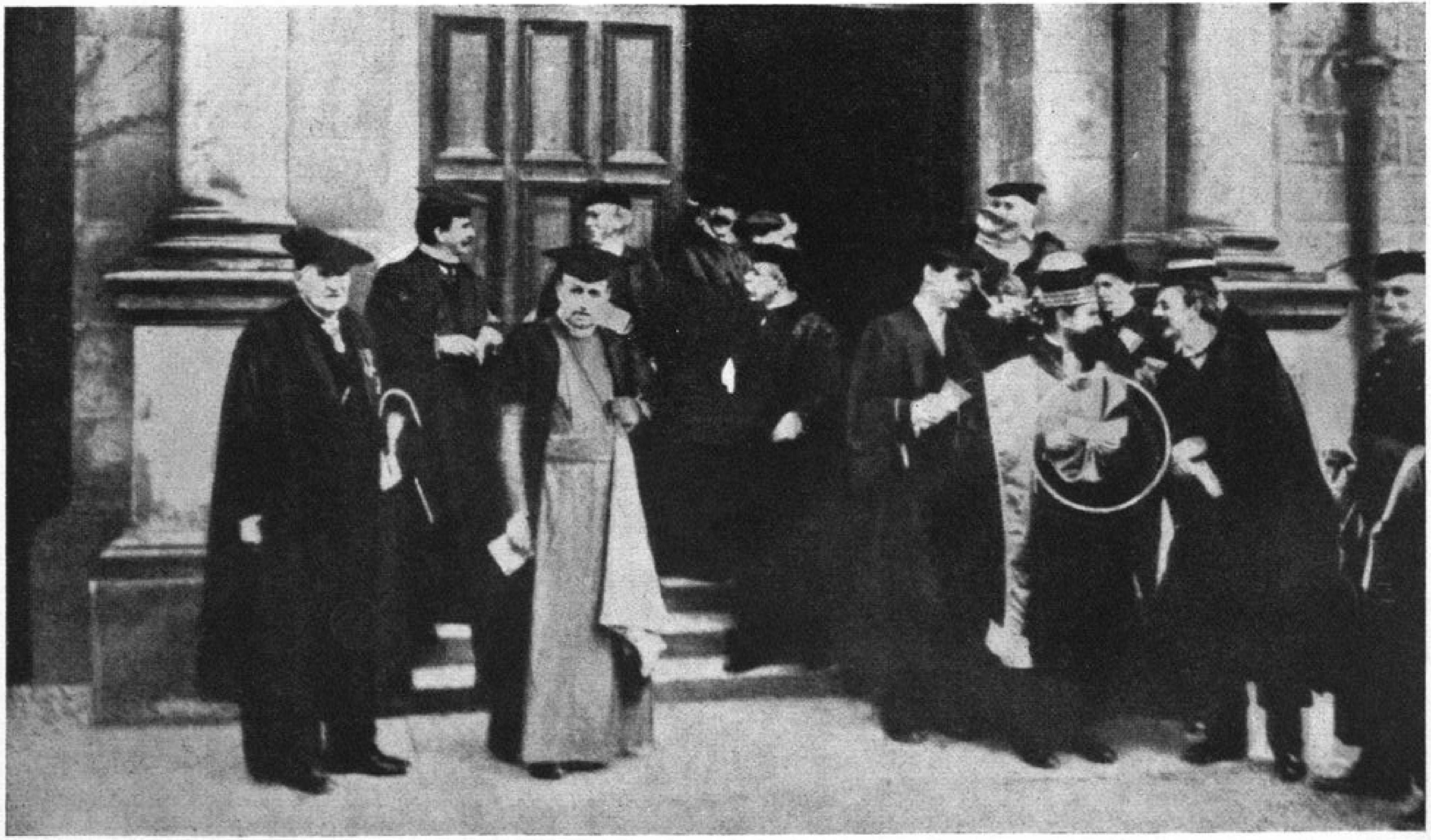

Fig. 2

Federigo Enriques (il terzo da destra) ritratto nel 1911, dopo la cerimonia del conferimento della laurea ad honorem all'Università di St. Andrew (Archivio Zanichelli).

Federigo Enriques (third from the right) photographed in 1911, after ceremony at St. Andrew's University, at which he was given an honorary degree (Zanichelli Archives).

TRANSLATION: J. LUBBOCK - ROME

THE BATTLES, DEFEAT AND VICTORY OF FEDERIGO ENRIQUES

For the very purpose of writing this brief essay on Enriques, epistemologist, science historian, didactic and pedagosist, I have had to look through the files of only the *volumes* written by Enriques in the possession of the Library of the Mathematical Institute of Rome University, now dedicated to Guido Castelnuovo, in which Enriques taught for nearly a quarter of a century, until his death[1]. Once more I was left filled with amazement at the bulk, and even more at the *extent*, of the work of that great man of culture. Once more, however, in rereading the pages of the main works by Federigo Enriques, I realised that the astounding *multiplicity* of the fields of his work and research is, in fact, a consistent *unity*. Like the pre-Socratic philosophers that were so congenial to him, or like the « natural philosophers » of the Renaissance, Enriques concerned himself, in an original way, with « fields » that we are accustomed to consider as separated by sharply-defined boundaries (or even as lacking any common frontier!) because his particular formulation was a unitary one - because those « fields », for him, were not distinct specialistic disciplines but diverse and associated determinations of *one* thought, of *one* view of the world.

SCIENCE AS CULTURE. — Enriques' unifying thought is that of « a wider concept of philosophy, as a form of activity implicit in every product of thought » (1911): it is that of the « philosophical relevance » of research and scientific discovery (in particular, in mathematics and physics). This central concept takes the form, in Enriques' thought, as the affirmation of the indestructibility of the relationship between philosophy as *Weltanschauung* and « natural philosophy ».

On the one hand, Enriques was convinced - even from his early youth - of being able to find « only in natural philosophy » an « answer (only partial, naturally) to the problems that had fascinated him in his High School years » (according to a conversation in Rome, reported by Giuseppe Scorza Dragoni). In half a century of philosophical battles, Enriques tirelessly states that western thought is permeated by the themes that have been presented to it by scientific research, from Pythagoras and Zeno to Kant and Hegel. It is not at random that the volume in which, in 1938, he assembles (with the help of Attilio Frajese) discourses and lectures that offer a synthesis of forty years' research is entitled « *Mathematics in history and culture* »[2]. Mathematics is culture, it is an essential part of culture and, in particular, it embraces all the « great metaphysical systems ». Enriques does not ask the philosopher to become a « technician » of science; he asks of the philosopher « much less and much more »: « the clear intelligence of the more elementary ideas that he has never studied sufficiently thoroughly and the philosophical significance - which he often believes he is teaching but which he has to learn from whoever has meditated on this subject for the longest time - of the principles and methods of science ».

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[1] A very fine and complete « Chronological list of the publications of Federigo Enriques » was compiled, after the death of the Master, by Luigi Campedelli and Adriano Barlotti. It can now be found in the Appendix to Enriques' volume *Natura, ragione e storia* (An anthology of philosophical writings, edited by L. Lombardo Radice), Boringhieri, Turin, 1958.

[2] The first edition dates from 1938. The publisher, Zanichelli, of Bologna, has announced that it will be reprinted during 1971.

On the other hand, Enriques is philosophically exacting with the scientist, whom he asks to be a « natural philosopher », a critic of the foundations of his science, not a pure technician. In short, to use the words with which he concludes the chapter « Mathematics and philosophy » in the above-mentioned book, he asks both of them, the philosopher and the scientist, « not to break the connection of the spiritital currents that converge in the movement of thought, and therefore to understand the different motives from which the movement itself originates ». He asks both of them not to let the « humanistic universalism » proper to culture degenerate into a « philosophy of water-tight compartments ».

THE PLACE IN HISTORY TO BE ASCRIBED TO THE THOUGHT OF FEDERIGO ENRIQUES. — Since his thought was centred upon this - the unity of culture, the place occupied by the sciences in philosophy, history and the training of the individual - Enriques had, *necessarily*, to wage a battle on many fronts: philosophical, epistemological, didactical and historical.

It would be wronging Enriques' « dynamic view of science », which « led him naturally onto the ground of history », not to make an effort to ascribe a place in history to this very thought of his. Enriques is extremely original, but he is not a lone individual. What we have defined as the unifying element in his thought - the science-philosophy, the science-culture-history relationship - is a characteristic feature of Italian and European scientific society (or rather, of its more conscious and advanced exponents) between the end of the last century and the beginning of the first world war. We can state - bibliography in hand! - that Enriques' philosophical activity falls within those fifteen-twenty years. His first writings on philosophy date from 1901, the *Problemi della scienza* from 1906 and *Scienza e razionalismo* from 1912.

If then, we consider the Italian scientists of the first rank in the first decade of the century, we find a whole series of men highly committed in philosophical, social, educational and historical problems; the pure technician scientist is a thing of the future. To mention some names at random: the mathematicians Peano, Padoa, Castelnuovo, Severi, Gaetano Scorza, the physicist Righi, the chemist Ciamician and the doctor Murri. *Scientia,* founded by Enriques and other to make it easier for the new results and methods of the sciences to enter into circulation in the spheres of culture and philosophy, was thus to find congenial contributions in all fields.

That fifteen-year period saw the conclusion of the great work of a philosopher-scientist such as Henri Poincaré and the beginning of the scientific revolution that bears the name of Albert Einstein, a « natural philosopher » and certainly not a pure mathematician.

If we then look towards the philosophical firmament of that historical period, we find Bergson and James, Russell and Mach, Croce and Gentile. Now in all the currents of thought that confront one another - positivist and idealist, logicist and pragmatist, conventionalist and rationalist - the problem of the science-philosophy relationship occupies a central place.

The fact is that, in those years, scientific progress is not an advance in a straight line; on the contrary, a great scientific revolution is achieved and the very *foundations* of science (in particular, of mathematics and physics) are called into question. The scientist - the *true* scientist - must come to terms with philosophy in such an age, at the very same time that he tackles the problems that science poses; the philosopher - the genuine philosopher - *must* make a thorough study of the « elementary ideas » (foundations) of the sciences that are under discussion.

WHAT DOES ENRIQUES' ORIGINALITY CONSIST IN? — Also Enriques - like Russell or Hilbert, Peano or Poincaré - practises philosophy, first and foremost, for the inner needs of science. We will confine ourselves here to a few brief references[3]. In his *Problemi della Scienza* (1906), Enriques makes a working analysis of time measurement that is very close to that which - without our author being aware of it - Albert Einstein, at the same time, is pursuing thoroughly to its end. Enriques' analysis follows the same direction as that of Einstein, against the idea of « absolute time »: it is the philosophy of (special) relativity that emerges from within mechanics and physics. One year later, in 1907, in another report, Enriques expounds ideas that accord with those of general relativity, which Einstein was to formulate in a complete and strict manner one decade later. In fact, Enriques considers Newtonian mechanics as a « local science », true - with great approximation - only if the « duration and velocity » of a movement are « very small compared with certain data relative to the system of reference ». Beyond these hypotheses, « the premises of *non-Newtonian mechanics* will be created, tending to coincide with *Newtonian mechanics* as the velocities considered are reduced ».

Already in these determinate analyses there is implied a criticism of the « scientific fact » understood as a *datum* and of science as a record of data (which is a criticism - it must be noted - as much of neo-empiricism as of neo-idealism: rightly had Croce borrowed from Mach his conception of science as gnoseopraxis, as arbitrary but useful cataloguing!). This criticism takes up a whole chapter (the second) of *Problemi della scienza.* Fact and hypothesis, datum and overall representation are inseparable, maintains Enriques. Experimental proof is never merely a proof of data but of the images within which we frame the data; hence the necessity to « modify the geometrical representation adopted » when it is clearly « inadequate ». « Reality is not a pure datum but something constructed thanks to rational co-ordinating activity »: this is the inner foundation to Federigo Enriques' science of philosophical *rationalism.* Enriques gave his philosophy at least three definitions: *critical positivism, experimental rationalism* and *historical rationalism.* These three definitions would seem to correspond well to three successive evolutional stages in his thought.

His first epistemological study, *Sulla spiegazione psicologica dei postulati della geometria* (1901), shows traces of the influence of « classical » positivism through the instant call to the senses; however, it is already a *critical* positivism, because the sensory datum is organised in axiomatic forms. (Enriques relates the axioms of metrical geometry to the hand, the « special touch », those of projective geometry to the eye and those of topology to a « general tactile sense »). The connection between senses and axioms is to be developed in the geometry-physics union, which is characteristic of Enriques. Against conventionalism and, in short - as it seems to us - also against « pure formalism », Enriques is the upholder of a « geometry that forms an integral part of physics and finds its natural extension in this discipline ».

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[3] I have developed the subject far more fully in the Introduction to the volume mentioned in Note [1] («Federigo Enriques and natural philosophy at the beginning of the 20th century»).

This attitude of Enriques seems to us highly original. On the one hand, he wholly accepts the axiomatic method; on the other hand, he considers « interesting » (scientifically fruitful) only those systems of axioms that characterize geometrical spaces that are images of physical realities. This seems to us to be the scientific subsoil of his *experimental rationalism*: the correspondence - with an approximation that can always be perfected - of physical phenomena with purely intellectual formal pictures (such as are systems of axioms).

Enriques speaks of his « faith in the intelligibility of things »; we might say that he believes in a *nature according to reason*. This « reason », however, is no longer something absolute for him, as it had been for his great master Galileo. It is a reason that is evolved and determined historically: hence Enriques' more mature attitude, that of *historical rationalism*. From this point of view, the interruption in his philosophical activity - one might say, from 1912 to 1938 - no longer appears as an actual interruption, since his philosophical research extends and is embodied in his historical research, which so much occupied Enriques during the twenties and the thirties. It may also be added, we believe, that the renewed philosophical drive of the now elderly scholar - and we are mainly thinking of *Causalité et déterminisme dans la philosophie et l'histoire des sciences* (1941) - is connected with his attitude towards a new crisis in the foundations (nuclear physics, wave mechanics).

In conclusion, Enriques does not appear to us as a solitary thinker, detached from his age; in his time, and in the themes of his time, he occupies a definite position, but a very original position. He argues with positivists and idealists, logicians and empiricists, pragmatists and conventionalists, and at the same time he achieves a very original synthesis of elements taken from these currents of thought. As Guido Zappa has shrewdly observed, in a long article on « Enriques the philosopher » (*Studium*, 1959), « he reacts towards the antirational tendencies of pragmatism and the anti-scientific tendencies of idealism, but at the same time he absorbs certain elements from these currents and incorporates them in his thought ».

WHY DEFEAT, WHY VICTORY? — I have set out to give a synthetical outline of Enriques' place in history as a thinker. Therefore, intentionally, even if with regret, I confine myself to a very few brief notes on his activity as a science historian and as scientific educator. I have already said that the history of science (in some volumes he collaborated with De Santillana, Frajese and Mazziotti) occupies a very considerable place in the last twenty-five years of Enriques' life. While revealing a special passion for the history of Greek science and for a reconstruction from within of the mental processes of the Greek geometricians, Enriques is one of the few Italian historians of mathematics (I was about to say, the *only* one) that has undertaken to assess the entire field of the development of mathematics, by studying also - historically -

the scientists that, if not his actual contemporaries, belonged to the previous generation.

As has been said, the « thread » is once more the idea that mathematics (science) is an integrating part of culture and of history: nothing more. This is also the thread that guided Enriques' activity in the field of pedagogy and didactics in mathematics and the sciences. This activity was enormous: university texts that are real « jewels »; manuals for secondary schools (in collaboration with Amaldi) that are still taken as authoritative; the Mathesis Society, the *Periodico di Matematiche*, and then lectures, seminars, articles etc., all fighting to ensure that science was not relegated to a « ghetto » but was an integrating part of humanistic studies.

The solution provided by the Gentile reform (1923), at least for higher secondary education, was quite different: it was that corresponding to the conception of science as non-science, that belonged to the neo-idealism both of Croce and of Gentile (even if not of all their disciples). The prevalence of the idealistic thesis was not merely a philosophico-theoretical fact, it was a political, institutional, statuary choice. For epistemology, for the history of science, for the pedagogy and didactics of science, there was no university chair and practically no special institute; the so-called scientific high school occupied an inferior position compared with the classical high school, there was no experimental science for five years in the grammar school and few hours of mathematics. The main periodicals and publishing firms followed the prevalent trend, so that very little news reached Italy of the astounding progress being made in other countries in epistemological and historico-scientific research and studies in the didactics and pedagogy of the sciences.

On another occasion[4] I attempted a careful analysis of Enriques' clash with Croce and Gentile and I reached the conclusion that on neither side was there a real capacity for « hegemony »: that is, a capacity to appropriate the motives of the opponent and pursue them, each from his own point of view. Croce and Gentile were unvilling to recognise scientific research as knowledge and culture; Enriques was unable to appropriate the deeper motive of Hegelian dialectics.

In any case, the victor at that time was idealism; Enriques was the defeated.

Twenty-five years after the death of Enriques, the situation has radically changed. It is not a case here of asking oneself why, or who is responsible; we merely wish to show that the ideas of Federigo Enriques - on the dialectical unity between science and philosophy, on science as an integrating part of humanism, and on science as a fundamental aspect and texture of history - in a certain sense, and at least at the level of the higher forms of culture, have the upper hand. There is an awareness of this, and I am convinced that, after and beyond this centenary of the birth of Enriques, his ideas will return into circulation in Italian culture, extending to ever wider spheres.

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[4] See the Introduction cited in [3] above.

MODERN ASPECTS OF THE PROBLEM OF THE ORIGIN OF LIFE

According to modern concepts, the origin of life on earth is based on the building up of complex carbon compounds, from which multimolecular systems developed. The initial steps of this process were definitely of a chemical nature, but at a certain stage of development of matter the transition from chemical to biological evolution occurred. The new objective laws appeared determining the principal features of life on our planet. However, life as we know it today did not appear as a result of any purposeful process regulated by a plan established beforehand. We conceive of its present manifestation as inevitable only because we are not familiar with any manifestation of life, other than life on earth.

Man everywhere, although in varying manner, has striven to define his place on earth and in the vast universe. Just now we find ourselves at a stage when a radical revision is taking place of our basic concepts of science and human knowledge.

According to legend the ancients, with an application of colossal resources and energies, attempted to erect the grandiose tower of Babel in order to reach the firmament, considered to be supported from within by a rigid dome extending down to the earth. At present, in an era when man is already travelling in the cosmos, we look at the sky with a sensation altogether different from that experienced by our grandfathers and even our fathers. We are passionately aware of the fact that we are not alone in the universe. We are destined to spare no effort or material to obtain direct proof of this, because man does not live « by bread alone ». The solution of this problem is an absolute necessity to him.

One hundred to two-hundred years ago the world appeared to a man of science as a grandiose mechanism, created in accordance with the exact law of conservation of matter and energy. In that scheme, all is unchangeable and all phenomena occur in a closed cycle, unchanged on returning to the original position. It was, therefore, said that every event in the world could be easily calculated and predicted in the same manner as we predict the course of day and night, or winter and summer - phenomena due to the revolution of the earth or its orbiting around the sun.

However, in modern times, belief in the unchangeability of all that exists has been replaced, in the minds of people, by another principle previously proclaimed by Heraclitus, the great dialectic of ancient Greece - « everything is in a state of flux ».

We live in a world that perpetually changes; undergoes evolution. This process of development has a progressive character and leads to the emergence of each new and more complex form of existence. Striving to catch only a glimpse of the universal progress from evolution of elements to the emergence of conscious beings and of the human community, it is nevertheless clear to us that this evolution does not appear to proceed in a straight line. At different times and on different cosmic objects, it may have been accomplished by different routes. Therefore, we can visualise evolution as a system of parallel or divergent lines some branches of which can bring about the organisation and motion of matter in complex and perfect form. About many of these forms we still know nothing, in fact we cannot even guess at their existence. However, independently of the degree of perfection they may have attained in the process of their development, they may have no relation with life in the majority of cases.

Life is the result of only one of many such branches of the evolution of matter. Characteristic of it are the specific routes of appearance and perfection. In the present day, we only know life by one singular example, i.e. terrestrial life; from it, we must formulate our opinions about other possible forms of biological organisation.

Hence, the earth and events which have taken place on it must to a considerable degree serve as a model for our paramount ideas concerning life in the universe.

Earlier, the emergence of life on earth was considered to be the result of a lucky accident. This could contribute nothing to a theory concerning life outside our planet, apart from calculation of the probability of such an event, which gives little satisfaction to the mind.

However, we now have a basis for looking at the emergence of life not as « lucky happenstance », but as a phenomenon entirely regulated by laws and an unalienable and integral part of the general evolutionary development of our planet. If we accept the laws of nature as universal, we should expect that an analogous evolution of matter would result in the emergence of life on other planets of our universe.

The search for life outside the earth is only a part of the more fundamental question as to the origin of life in the universe. The study of the origin of life on earth presents itself as the investigation of just one example of events that must have occurred in the cosmos an innumerable number of times. Therefore, the explanation of the manner in which life sprang up on earth must of necessity give strong proof in favour of the theory of its existence elesewhere in the universe.

According to contemporary theory, the onset of the rise of life on earth was preceded by a process in which carbon compounds became successively more complex, and the formation from these of a polymolecular systems. It is possible to break down this process into several stages:

1. Appearance of hydrocarbons and cyanides and their immediate derivatives in cosmic space, and during the formation of the earth and

the subsequent development of its crust, atmosphere and hydrosphere.

2. Conversion on the earth's surface of the initial carbon-containing compounds into more and more complex organic substances - monomers and polymers; appearance of the so-called « primordial soup ».

3. The self-formation, in this soup, of polymolecular open systems capable of mutually interacting with the environment and capable of growth and multiplication on the basis of this interaction - appearance of probionts.

4. The further evolution of « probionts », the development of more perfect metabolism, more perfect molecular and super-molecular structures accomplished through the basis of prebiological selection - the appearance of the primordial organisms.

We see that the two first stages are chemical in character. They proceed within the frameworks of the laws of physics and chemistry, which are common for all things in the universe.

However, at a certain stage of development of matter the transition from chemical to biological evolution occurred. The system which then appeared had to acquire some features of organisation specific to the organic world.

Among those we should mention is firstly, the ability to oppose an increase of entropy, that is characteristic of living organisms. It is known that in contrast to processes in inorganic nature, generally resulting in the growth of entropy, the living systems are marked by a decrease of entropy and an appearance of order from the chaotic thermal movement of molecules.

Secondly, it is the so-called « purposiveness » of organisation of living beings, adaptability of the whole organism to its permanent self-preservation and self-reproduction under the given environment, as well as the fitness of its integral parts (the molecules, cell organoides and organs), to their special functions. And, thirdly, it is a special and very perfect mode of storage and transfer of information based on intramolecular organisation of high molecular weight compounds, the nucleic code.

Simultaneously with the appearance of the new features specific for living systems, some new biological laws, based on interaction of organism and environment and on the Darwinian natural selection, have arisen in the very making of life.

The appearance of the compounds necessary for initial steps of prebiotic evolution: hydrocarbons, cyanides and their close derivatives, occurred in the extremely far epochs, many billions of years ago, long even before the formation of the solar system. It was caused by a relative abundance of hydrogen in space, as well as by the fact that carbon preceded the appearance of the heavy elements necessary for planetary systems formation, and resulted from the stable process of stellar radiation, independently of flashes of supernovae. The presence of carbon has been established by a study of spectra of all classes of stars, including the oldest ones, 15 - 20 billion years old.

Therefore, carbon and hydrogen compounds are very widely distributed in space. We find them both on the surfaces of stars with temperatures of several thousands of degrees and very strong gravitational fields as well as in interstellar gas and dust at an extremely low gravitational gradient and at temperature close to absolute zero. This is proved by the study of contemporary clouds of interstellar material and by the spectral investigation of comets, the celestial bodies formed under conditions which are close to those of interstellar space.

These studies have shown that comets are abundant with light hydro-carbons and cyanide.

The data, derived from the meteorite studies, are of special interest in this respect, because until recently when the samples of lunar rocks have been brought back, they were the only extraterrestrial bodies available for direct analysis, and also because their composition is very similar to that of dust material concentrations, planetesimals, from which the earth and earth-like planets have been formed.

Not only the simplest carbon and hydrogen compounds, but various complicated organic substances of abiogenic origin have been found in some meteorites, the so-called « carbonaceous chondrites ».

Therefore, as suggested by J. Bernal, the second stage of evolution of carbon compounds, their transformation to produce more and more complicated organic substances, had begun not only before the origin of life, but even long before the earth was formed.

According to J. Bernal (1967) those transformations occurred abiogenetically under the influence of short-wave ultraviolet and cosmic radiation on the surface of the interstellar dust particles and on planetesimals.

Thus, in the course of its formation as a planet, the earth has acquired some organic compounds formed beforehand.

Later, the earth's surface gained further organic substances, introduced with meteorite and comet material.

However, the bulk of the organic substance necessary for the origin of life was probably formed endogenously on the earth during formation of its crust, hydrosphere and secondary atmosphere.

From this viewpoint, numerous recent attempts to find abiogenic organic compounds in the earth's rocks seem to be of great interest. However, the interpretation of such geological data is sometimes rather complicated by possible contamination.

According to the most accepted views, our planet was formed by the accumulation of cold solid bodies, planetesimals.

Such gases as diatomic hydrogen, helium etc., could be retained by the earth only if they were adsorbed by solid rocks and then became part of its primary atmosphere. However, this atmosphere could not last very long, since the gases composing it were not retained by terrestrial gravitation.

The compact mass of the earth continued to evolve and this evolution was mainly determined by its thermal history. The primary rocks were partially melted out by heat energy resulting from gravitation and radioactive decay, and the crust, hydrosphere and atmosphere were thus formed.

The bulk of the water was originally bound by hydrated rocks. Therefore initially the water content of the earth's surface was much lower than now, and formation of the oceans proceeded gradually, in step with the crust formation.

Appearance of the secondary atmosphere was also closely related to the formation of the crust. This atmosphere was principally different from the contemporary one; it had a reducing character, was free of oxygen (O_2), and in addition to water vapour contained some hydrogen compounds, like gaseous hydrocarbons, ammonia, and hydrogen sulphide (H_2S).

It was entirely transparent to short wave ultraviolet radiation, since the absence of molecular oxygen made impossible the formation of an ozone screen, which now bars access of this radiation to the earth's surface.

Many scientists working in different countries, S. Miller (1959), C. Ponnamperuma (1963, 1968), J. Oro (1965), T. Pavlovskaja, A. Pasynskii (1959), when reproducing the conditions of primordial earth, have convincingly demonstrated that under the conditions of the secondary atmosphere and hydrosphere various organic substances should necessarily be synthesized. In some cases these compounds were very similar to the ones we are isolating now from contemporary organisms, e.g. amino-acids, sugars, purine and pyrimidine bases, nucleotides, organic acids, as well as their polymers including substances which resemble proteins and nucleic acids (R. Lemmon, 1969). S. Fox (1969), on the grounds of his modelling experiments, has concluded that the protein-like polymers, formed abiogenetically, should have a certain intramolecular organisation, non-random distribution of amino-acid residues in the polypeptide chain, that would ensure them some specific catalytic activity.

The organic compounds formed on the earth in such a way could certainly be decomposed by short-wave ultraviolet radiation or some other factors. Some of them could, however, escape decomposition, if their polymers or their complexes were removed from the surface to deeper water layers or were adsorbed by soil particles, etc.

Thus, various organic substances accumulated in the hydrosphere during its formation and a complex aqueous solution of those substances, the so-called « primordial soup », was formed.

We may now suggest with some confidence the probable character of chemical transformations proceeding in the « primordial soup », and even the order of appearance of various organic substances. It was in general similar to the phenomena taking place in a simple aqueous solution of those substances and was, certainly, radically different from all that we observe in living systems.

The « primordial soup » lacked both complex order of metabolic chemical reactions and any kind of biological structure.

Even protein-like polymers of the « primordial soup », possessing an organised intramolecular structure and catalytic activity, must have been essentially different from modern proteins, because the structure and enzymatic activity of the latter are always excellently and « appropriately » adjusted to the functions they perform in the organism.

Such fitness, « purposiveness » of their structure, even at the molecular level, was certainly absent from a simple aqueous solution of chemical compounds.

It could only arise in the very process of establishment of life, when chemical evolution was changing to biological evolution through the formation of integral prebiotic systems and their development by natural selection.

We know that life is not dispersed throughout space like the substances in the « primordial soup ». It is represented by organisms, discrete systems, spatially separated from the environment, but interacting with it as open systems, i.e., ones whose stability and duration of life are determined not by their invariability nor by state of rest, but on the contrary, by permanent transformation of substances. The thermodynamics of such systems distinctly differ from classical thermodynamics (I. Prigogine, 1955) as conventionally defined. This fact conditions the first feature mentioned above, which is characteristic for all organisms, that a decrease, not an increase of entropy occurs in them.

Therefore, in the course of evolution of matter an appearance of organisms should be preceded by a self-formation of multimolecular open systems, much more primitive than the organism. Only by their gradual development could the ancestors of all living beings on the earth appear.

It is easy not only to suggest, but to reproduce in modelling experiments a great number of such systems initiating the biological evolution: vesicles Goldacre's (1958), Fox's microspheres (1959), Bungenberg-de-Young's coacervates (1932) etc.

It was important for the further evolution of these systems that they could interact as open systems with the solution surrounding them and their stability had not a static character but a dynamic one. Coacervate drops seem to be the most suitable but, certainly, not the only possible models for reproducing the phenomena having taken place in remote epochs (T. Evreinova, 1966).

Under conditions similar to the « primordial soup » the coacervate drops are easily formed through aggregation of various, even non-specific polymers (e.g. polypeptides and polynucleotides) to form microscope-visible systems, separated from the external solution by a pronounced surface layer, but capable of selective uptake of various substances (amino-acids, sugars, mononucleotides, various salts) and of excretion to the solution the products of chemical reactions proceeding inside the drops (A. Oparin, 1963).

The rates of these reactions may be sufficiently enhanced by incorporation of various catalysts (mineral, organic or enzymic ones) into the drops.

In this case both decomposition processes and the synthesis of the polymers, forming the drops, at the expense of energy-rich compounds entering the system, may take place. If the rate of the latter processes exceeds the rate of decomposition, the drops not only become dynamically stable, but also capable of growth. Their volume and mass growth rates are just perceptible. Thus, the flows of substance and energy appear in the drops, modelling in the most simple way metabolism of living organisms. We have reproduced in coacervate drops some rather complicated patterns of metabolic activity, combining several mutually coordinated reactions.

In all these cases the growth rates of the drops are a direct function of their individual organisation, especially, of the ratio of velocities of various reactions occurring in them. In this respect different drops might be considered as more and less perfect ones.

We have demonstrated that under the conditions of the same environment, i.e. in the solution of the same compounds, more perfect drops grow faster than less perfect ones, which may even show a decomposition (A. Oparin, 1968).

Such modelling experiments reproduce initial steps of the new law which should appear at the transformation of chemical to biological evolution, i.e. the primitive form of prebiotic natural selection, which determines the second feature of all living systems, that is the « purposiveness » of their organisation.

The prebiotic natural selection underlay the further evolution of the systems, which gave rise to organisms. We may designate them as « probionts ». Their further development may be discussed only in the frameworks of hypotheses mostly based on the data of comparative study of metabolism and structure of contemporary primitive organisms (A. Oparin, 1969).

The evolution of probionts was mainly connected with the development of their catalytic machinery, the most important factor for organisation of metabolism, based on the ratio between the rates of reactions.

Certainly, at the stage of evolution discussed here, only mineral salts and organic substances with a very low catalytic activity, adsorbed from the solution, were accessible as catalysts for probionts. Their activity, however, could be increased by several thousand times if some of them were combined in a suitable way (A. W. Langenbeck, 1953).

We may imagine a colossal number of various atom groupings and their combinations, which to some extent were capable of catalysing reactions necessary for probionts. However, since natural selection eliminated less perfect complexes, killing the probionts possessing them, only some of them, the most perfect ones, can be found in contemporary organisms; those are coenzymes well known in biochemistry. The number of them is small, but they are inherent in all living beings without exception, indicating their early formation in the process of the origin of life.

As the probionts expanded and divided under the influence of mechanical force (for example the shock of surf) the necessary constant concentration of coenzymes could be maintained thanks to adsorption of them or their constituents from the external medium.

A rather similar situation may be observed in contemporary organisms, which have to obtain from the environment vitamins serving as coënzymes in their metabolism. But, certainly, capability to synthesize the coenzymes should be developed in probionts, which would release the systems from too great dependence on the environment.

Gradual complication of the probiont metabolism demanded, however, very accurate combination of a large number of metabolic reactions to a coordinated network of biochemical processes. Low catalytic activity and specificity of the coenzymes were insufficient to resolve this problem and in the progressive evolution the coenzymes were complemented with a whole arsenal of much more powerful catalysts - enzymes, that is, proteins with a secondary and tertiary structure extremely well adapted to the fulfilment of their catalytic functions. Thus, the epoch of coenzymes was replaced by a new one when the proteins with intramolecular organisation acquired a decisive role (A. Oparin, 1965).

Initially it appeared that protein-like polymers with a random distribution of amino-acid residues could be a material for coacervate drops and probionts formation, but they had very low catalytic activity or were absolutely devoid of it.

Certainly, the amino-acid sequences capable of performing the role of enzymes, could arise in the course of polymerisations in probionts. But in the course of common non-arranged polymerisation this advantage could be rapidly lost, levelled in the expanding probiont.

Therefore, the appearance of an organisation which could provide a constancy of primary structure of newly synthesized polymers was very important.

An extremely important role in this organisation fell upon polynucleotides.

The synthesis of enzyme proteins in contemporary organisms is carried out through a very complicated and perfect mechanism, by which amino-acids are subsequently joined to the polypeptide chain, in an order governed by a specific, strictly regular nucleotide sequence of DNA and RNA molecules.

Such mechanism could certainly be born only by the long evolution of probionts and later, of living systems. But even at the early stages of development, polynucleotides, associated with probionts, could effect amino-acid polymerisation in these systems. The intramolecular structure of the most primitive polynucleotides was still very imperfect. It could dramatically vary during the growth of the probionts. Each variant appearing in this way could be fixed to some extent in the given expanding system, as a result of polynucleotide complementarity, and at the same time it affected the order of amino-acid residues in polypeptides, synthesized in the system.

An increased level of catalytic activity in the newly-formed amino-acid sequence would favour the faster growth and multiplication of the system which produced it.

In the opposite case, natural selection would destroy it. Thus, intramolecular structure of protein-like polypeptides and in parallel with them, polynucleotides, participating in their synthesis, gradually became more and more organised and better adjusted to the functions they performed in the expanding and multiplying systems.

But we should remember that neither the polynucleotides capable of replication, nor even polypeptides possessing certain specific amino-acid sequences formed under the influence of the polynucleotides, but whole systems of probionts with a primitive, but more or less perfect metabolism, corresponding or not to the given conditions, were screened by the natural selection. The role of nucleic acids in this process was to consolidate spatially in expanding and multiplying systems a continual synthesis of catalytically more favourable amino-acid sequences and to serve as a stabilising factor in their evolution.

Thus, at the rather late stage of evolution a new epoch began, and the living systems reached an unprecedented level of exact self-reproduction, which is characteristic of all contemporary organisms. As we have already pointed out, the general picture of gradual evolution of carbon compounds and the systems formed from them to the origin of life is strongly supported by the data of astronomy, geology, physics, chemistry and biology.

But the evolution should not be considered as a straightforward chain of events, with a strictly obligatory sequence of links, that is governed by a special plan worked out beforehand and directed to a definite goal - the origin of the identical mode of life which exists now. At each stage of prebiotic evolution the further course of events in various sites of our planet might have proceeded in different ways. Only since most of the modes of organisation disappear in the process of natural selection never to return, the highly uniform organisation of the earth's life which exists now, has been formed.

The considerations mentioned above allow us to suggest here the following scheme, still rather a hypothetical one, of prebiotic evolution.

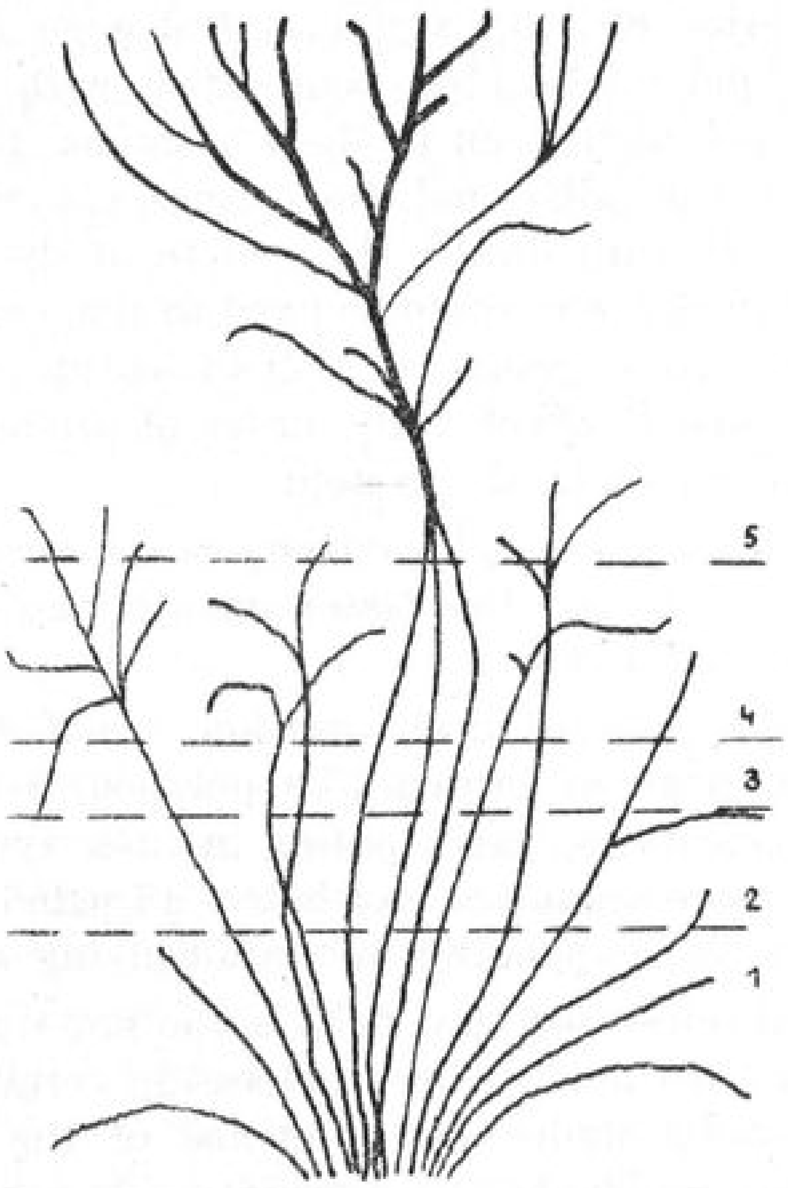

Fig. 1

Schema of prebiotic evolution

1 - Zone of synthesis and decomposition of carbon compounds; 2 - Zone of the origin of polymers; 3 - Zone of the origin of open systems; 4 - Zone of the origin of natural selection of « probionts »; 5 - Zone of the nucleic code.

On the left side of our scheme we depicted the development of systems formed only from proteins or proteinoids and in the right side proteolipids, and carbohydrate-lipids. On the center of the scheme are presented systems composed of protein and nuclear-like polymers. Fourth zone as a result of the origin of natural selection only nucleo-proteid systems could have survived and give rise of mdern life.

Even in referring to the very beginning of the evolution of carbon compounds, one ought not to speak of some single object which gave rise to others or of a general path of development. The formation and primary complexity of organic compounds must have occurred in the cosmos and on our planet under very varying conditions and under the influence of greatly varying sources of energy. Because of this, in the basis of our schematic diagram we do not have a single trunk, but a bush of various pathways. Most of them come to a dead end - a total disintegration and a diffusion into interstellar space (which is probably what we have in the top layer of the moon), or on the contrary, buried in the form of high molecular weight molecules, but very stable ones, not changing from the original form (not unlike the bitumen of meteorites).

However, in the varying conditions of the earth's surface, some of the original organic substance must necessarily have evolved into more complex forms, representing increasingly larger multimolecular compounds. This event cannot be regarded as being some sort of « fortunate accident ». Of course, in this or that specific place, in this or that specified time, it could or could not have originated, just as in a particular area it might or might not rain.

But in the framework of the entire surface of the earth and during long periods of time, the basic organic matter must have arisen to a considerably higher level of organisation. We do not necessarily have to think of them as being endowed with such perfect internal molecular structure as contemporary proteins or nucleic acids.

The notion that the origin of some whole systems must have preceded the formation of complete, highly adapted parts of this whole, appear as but an echo of the old mechanical notions of assembling a machine and its parts which have been fabricated previously. The nucleic acids and proteins which are familiar to us have intermolecular structure, which is adapted to the execution of definite funtions in the living body, for example enzymes, and could have arisen only in the process of evolution of that body or its intact fore-runner probionts. Because of this, in the «primordial soup», there could and had to originate randomly-constructed polypeptides or polynucleotides and also carbohydrates or lipid compounds and even high molecular-weight compounds which we do not find now in nature.

In order that organic substances could enter into a new stage of evolution all that was necessary was their ability to stimulate the origin of phase 3 of the relationships in the previously homogeneous « primordial soup ».

Only on this basis could arise individual multimolecular systems separated from the external surrounding by a surface, but capable of interaction with this environment in the manner of open systems. The formation of this type of system is found at the junction of the preceding chemical evolution and the biological evolution which originated from its foundation. The first step in the latter was the surmounting of the growth of entropy in new systems.

In general, in the inorganic world, all processes are directed to an increase in entropy; for living systems, on the contrary, the diminishing of entropy is characteristic. This is stimulated by the fact that living organisms are open systems, the very existence of which is dependent on their continuous interaction with their surrounding external medium. Through them continually sweeps a stream of matter, energy and information, and they (according to the expression of Schroedinger) seem to feed on negative entropy received from the external surroundings.

Therefore, the formation in the primal homogeneous aqueous solution of individual open systems becomes a necessary stage on the path of the origin of living beings. But this formation had to occur every time the molecules of organic matter reached a specific size.

On earth's surface these open systems could and should have formed in great quantity and variety, differing in their composition as well as the characteristics of intermolecular bonds and structures.

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PLATONE E RUSSELL NELLA DEFINIZIONE DELLA MATEMATICA

L'articolo mostra l'esistenza di analoghi giudizi sulla matematica fatti da Platone e da Russell. Sia nella coscienza che la matematica, come scienza ipotetico-deduttiva, è una costruzione basata su postulati non dimostrabili ed è quindi basata su convenzioni e sia nel reputarla una fonte di bellezza e di piacere intellettuale oltre che, apparentemente in contrasto col primo giudizio, una verità esistente e rassicurante, Platone e Russell si esprimono quasi con le stesse parole. L'articolo mostra inoltre il giudizio di Russell su l'amore di Platone per la matematica e uno sviluppo successivo della concezione di Russell su tale scienza, sviluppo che potrebbe essere però più formale che sostanziale.

È noto che ancora non è stata data una definizione completa e soddisfacente della matematica. Se ci si limita ad alcuni particolari argomenti, si può dare una definizione in relazione ad essi; quando però si vuole abbracciare il vastissimo campo su cui essa si estende, ci troviamo di fronte a difficoltà insolubili o per lo meno non ancora risolte.

Non c'è però matematico e non c'è filosofo che non abbia tentato di dare una personale definizione, si tratta però in generale di visioni particolari di questa scienza il cui sviluppo non ha precedenti.

In questa nota vogliamo mostrare appunto alcune definizioni della matematica e alcuni giudizi sulla funzione e sul valore di essa, date da due filosofi che furono anche appassionati di matematica: Platone e Russell.

Non è nelle nostre intenzioni estendere, oltre a quello che abbiamo detto, il parallelo tra due personalità tanto diverse e così distanti nel tempo. Un confronto tra due studiosi non è mai possibile e le analogie molto spesso rimangono tali solo in superficie; inoltre, senza offesa per il grande studioso inglese, le due personalità non mi sembra che siano confrontabili perchè di dimensioni diverse. Riguardo alla matematica, però, si può tentare quell'accostamento che abbiamo già dichiarato, essendo simili l'interesse e il giudizio (almeno sino ad un certo punto, come vedremo) nei suoi confronti.

Platone ebbe chiaro il concetto di matematica, quale scienza ipotetico-deduttiva, di una scienza cioè che basandosi su certi enti quali il « punto », la « retta », il « numero » (per Platone comunque esistenti in sè) e su certe loro proprietà accettate ma indimostrabili (ma probabilmente « vere » per Platone), costruiva, per così dire, sul nulla, su un nulla logico, il suo edificio.

Ecco infatti un significativo brano della 'Repubblica':[1]

« Quanto alle altre discipline, delle quali abbiamo detto che colgono qualcosa dell'essere, cioè la geometria e le discipline che ad essa fan seguito, vediamo che esse sognano rispetto all'essere, che è impossibile ad esse di vedere nello stato di veglia, fino a che si servono di ipotesi che lasciano immutabili, dal momento che non ne sanno render ragione. Veramente qual mezzo può esservi perchè diventi scienza una tale convenzione (ὁμολογία) che ha un principio che non conosce, una fine e un procedimento concatenati con ciò che non conosce? »

Questo giudizio va a grande merito di Platone che ha visto chiaro nell'essenza logica e relativa della scienza ipotetico-deduttiva nonostante il concetto pitagorico di assoluta realtà della matematica intesa anzi come essenza razionale dell'universo, che doveva dominare, a parte le critiche dei sofisti, gli ambienti colti a lui contemporanei. Platone d'altra parte non limita il suo giudizio sulla matematica a questa visione scettica anche se logica, poichè nella sua concezione della conoscenza l'ufficio di questa è assai importante. La matematica infatti è d'ausilio alla dialettica, collabora con essa per raggiungere, secondo Platone, la vera intelligenza, la conoscenza delle idee.

La conoscenza per Platone si suddivide infatti in opinione e intelligenza, la prima a sua volta in congettura, rivolta alle immagini degli oggetti visibili, e in credenza, rivolta agli oggetti materiali veri e propri; la seconda nella « diànoia » propria degli enti matematici e nella vera intelligenza che è la contemplazione delle idee. Con la «diànoia» si deve intendere una sorta di « passaggio verso » la conoscenza ed è questo appunto l'ufficio della matematica per Platone: un « argano che tira l'anima verso la verità, atta a produrre lo spirito (διάνοια) filosofico ed a rivolgere verso l'alto lo sguardo invece di volgerlo malamente verso il basso »[2].

D'altronde poco prima Platone aveva affermato che « la geometria è conoscenza di ciò che sempre è (τοῦ ἀεὶ ὄντος) ».

Se dunque la matematica deve apparire al logico Platone come una convenzione, un'opinione, al mistico Platone essa appare come una realtà in grado di elevare l'anima e portarla verso la verità, la vera conoscenza.

Innumerevoli sono i brani che potrebbero citarsi per mostrare l'atteggiamento di Platone nei riguardi della matematica: essa non solo dà un profondo piacere intellettuale [3] ma come abbiamo già mostrato, permette di « uscire dal divenire e raggiungere l'essere » [4], aguzza l'ingegno anche dei meno dotati [5] cosicchè il suo insegnamento dovrebbe imporsi per legge [6].

Platone si può considerare in effetti il fondatore della didattica matematica, colui che per primo ne vide l'ufficio formativo [7] e che esplicitamente mostrò un esempio di insegnamento attivo [8]. Ma questo non riguarda le intenzioni del presente lavoro. Interessanti sono, per quanto ci riguarda, altri due brani; nel primo, tratto dalla Repubblica [9], si intuisce, in un certo senso, il significato relativo della matematica che già abbiamo visto in un brano più esplicito ma che, nell'opera di Platone, viene poco dopo:

« Credo che tu sappia che coloro che si occupano di geometria, di calcoli e di simili discipline, ammettono per ipotesi il dispari e il pari e le figure e tre specie di angoli e altri enti analoghi secondo la materia che trattano. Ed essi procedono come se conoscessero davvero questi enti, sui quali hanno (soltanto) formulato ipotesi, e sui quali non ritengono di dover dare alcun conto nè a se stessi nè agli altri... ».

Nell'altro brano [10] viene esplicitamente affermata l'esistenza oggettiva della matematica, come d'altronde appare non solo da quanto abbiamo già riportato (« la geometria è conoscenza di ciò che sempre è »), ma anche dalla teoria generale delle idee:

« Così i geometri, gli astronomi e i calcolatori sono anch'essi cacciatori: non creano essi le figure ma trovano quelle *esistenti*. E poichè non sanno servirsene ma soltanto scovarle, le consegnano ai dialettici perchè si servano di ciò che essi hanno trovato: almeno quelli tra loro che non son del tutto privi di senno ».

In realtà bisognerebbe esaminare un altro significativo brano di Platone la cui interpretazione però è assai discussa, almeno nella sua parte iniziale [10a]:

« Ecco dunque che cosa intendevo per specie intelligibile, e dicevo che, ricercandola, l'anima è costretta e ricorrere ad ipotesi, senza arrivare al principio, perchè non può trascendere le ipotesi; essa si serve come d'immagini, di quegli oggetti stessi di cui quelli della classe inferiore sono copie e che in confronto a questi ultimi sono ritenuti e stimati evidenti realtà. Comprendo, disse, che ti riferisci al mondo della geometria e delle arti che le sono sorelle. Allora comprendi che per secondo segmento dell'intelligibile io intendo quello cui il discorso attinge con il potere dialettico, considerando le ipotesi non principi, ma ipotesi nel senso reale della parola, punti di appoggio e di slancio per arrivare a ciò che è immune da ipotesi, al principio del tutto; e, dopo averlo raggiunto, ripiegare attenendosi rigorosamente alle conseguenze che ne derivano, e così discendere alla conclusione senza assolutamente ricorrere a niente di sensibile, ma le sole idee, mediante le idee passando alle idee; e nelle idee termina tutto il processo. Comprendo, rispose, ma non abbastanza. Mi sembra che tu parli di una operazione complessa. Comprendo però il tuo desiderio di precisare che quella parte dell'essere e dell'intelligibile che è contemplata dalla scienza dialettica è più chiara di quella contemplata dalle cosiddette arti, per le quali le ipotesi sono principi; e coloro che osservano gli oggetti delle arti sono costretti, sì, a osservarli con il pensiero senza ricorrere ai sensi, ma poichè li esaminano senza risalire al principio, bensì per via d'ipotesi, a te sembrano incapaci d'intenderli, anche se questi oggetti sono intelligibili con un principio. E, a mio avviso, tu chiami pensiero dianoetico, ma non intelletto, la condizione degli studiosi di geometria e di simili dotti, come se il pensiero dianoetico venisse ad essere qualcosa di intermedio (μεταξύ) tra l'opinione (δόξα) e l'intelletto (νοῦς) ».

Sembra chiaro che la conoscenza matematica (tralasciando le varie interpretazioni tra cui assai penetrante e dotta quella di Federigo Enriques [10b]) basandosi su principi che deve ammettere senza conoscerne l'essenza vera (al contrario della dialettica) e servendosi per la sua comprensione di oggetti materiali (anche se estrapolandone le proprietà attraverso un processo razionale) non può essere dello stesso tipo, della stessa classe, della conoscenza dialettica a cui serve invece come stadio intermedio, passaggio, argano.

Senza proseguire oltre con ulteriori citazioni è dunque chiara la posizione di Platone: la matematica, come costruzione logica si deve basare su alcune premesse indimostrabili e quindi ingiustificabili, in questo senso apparirebbe una semplice convenzione; d'altra parte la matematica, oltre al piacere che arreca, all'uso didattico che se ne può trarre, può condurre l'anima verso la verità e la contemplazione dell'essere trattando di enti razionali, di cui le figure sono solo immagini imperfette), *esistenti* in sè.

Ebbene, analogo atteggiamento possiamo notare in Bertrand Russell. L'aforisma con cui egli definisce la matematica si può dire che ricalca, dopo più di duemila anni, quanto Platone aveva già pensato ed espresso. Dopo aver mostrato la natura ipotetico-deduttiva della matematica, formata da una catena di enunciati in cui « è essenziale non discutere se il primo enunciato è realmente vero » [11] cosicchè per fare della matematica « assumiamo dunque un'ipotesi che ci sembra attraente, e deduciamo le sue conseguenze [12] » purchè tale ipotesi riguardi « *qualcosa* » e non cose particolari, Bertrand Russell infatti afferma, poco dopo:

« Così la matematica può essere definita come la materia nella quale non sappiamo mai di che cosa stiamo parlando, nè se ciò che stiamo dicendo è vero ».

Come si può notare vi è un'aderenza stupefacente con il primo brano di Platone da noi citato [12a]. È chiaro che Russell doveva ignorare che il filosofo ateniese aveva espresso pressochè lo stesso pensiero, altrimenti, come fa in tante altre occasioni, avrebbe accennato a questo analogo atteggiamento.

La frase di Russell è stata presa a modello da tutti coloro che vollero riassumere apoditticamente, e forse con una certa intenzione dispregiativa, il significato della matematica quale scienza ipotetico-deduttiva, oppure viene presentata come una « boutade » del filosofo inglese anche se non priva di una certa verità.

In realtà, senza entrare in merito alla questione, notiamo che in senso realmente logico per una scienza ipotetico-deduttiva quale può essere la matematica, bisogna convenire che la frase di Russell è giustificata. Il senso di insoddisfazione che essa però lascia (o perlomeno *ci* lascia) può mostrare se non altro che la definizione della matematica è lungi dall'essere esaurita con tale frase [13].

D'altronde questo è l'atteggiamento dello stesso Russell che in un'altra occasione [14] mostra esplicitamente di considerare la matematica, come abbiamo visto anche in Platone, una verità oggettiva, una tavola di salvezza addirittura contro lo scetticismo:

« Sarebbe assai azzardato dilungarsi sugli effetti che probabilmente la matematica avrà in futuro; ma da un determinato punto di vista appare probabile un risultato positivo. Contro quel genere di scetticismo che rinuncia a perseguire gli ideali perchè la strada è ardua e la mèta non sicuramente raggiungibile, la matematica rappresenta, nella propria sfera, una risposta esauriente. Si dice troppo spesso che non esiste alcuna verità assoluta ma soltanto opinioni e giudizi personali; che ciascuno di noi, nel suo modo di vedere il mondo, è condizionato dalle proprie peculiarità, dai propri gusti e dalle proprie inclinazioni; che non esiste alcun regno eterno della verità al quale con la pazienza e la disciplina, si possa infine ottenere d'essere ammessi, ma soltanto la verità per me, per te, per ciascuna singola persona. Uno degli scopi principali degli sforzi umani è negato da questo atteggiamento mentale, e dalla nostra visione morale scompare la virtù suprema del candore, dell'accettazione impavida di ciò che è. La matematica è una perpetua smentita di questo scetticismo, il suo edificio di verità resiste infatti, incrollabile e inespugnabile, a tutte le armi del dubitante cinismo ».

Ed ecco come Russell conclude tale lavoro, dopo aver ulteriormente esaltato l'ufficio della matematica [15]:

« Nell'elevare la vita morale, nel nobilitare il tono di un'epoca o di una nazione, le virtù più austere rivelano una forza singolare, superiore alla forza delle facoltà non ispirate e non purificate dal pensiero. Tra queste austere virtù, la principale è l'amore per la verità, e nella matematica, più che in qualsiasi altra disciplina, l'amore per la verità trova incoraggiamento anche quando la fede vacilla. Ogni nobile ricerca non è soltanto fine a se stessa, ma è anche un mezzo per creare e rafforzare un magnanimo atteggiamento mentale; e questo obiettivo va sempre tenuto presente nell'insegnare e nell'apprendere la matematica ».

La matematica è dunque, anche per Russell, un « argano » verso la verità oltre a rappresentare una gioia di per sè [16].

È interessante osservare, a questo punto, come Russell giudica Platone: ebbene, all'inizio del lavoro da cui abbiamo tratto le ultime citazioni [17], ecco cosa scrive Russell: « Platone, come sappiamo, considerava la contemplazione delle verità matematiche degne della divinità; e Platone aveva compreso, forse più di chiunque altro, quali elementi della vita umana meritassero un posto nel cielo. Vi è nella matematica, egli dice, qualcosa di *necessario* 'Pare che pensasse a questo l'autore del proverbio secondo cui nemmeno Dio può combattere la necessità, quella necessità cui anche la divinità è tenuta; intendere le necessità umane cui pensano i più quando citano il proverbio, è pensare nel modo più stolto che ci sia. *Clinia* - Quali sono dunque, ospite, queste nozioni necessarie e divine? *Ateniese* - Io penso che siano tali che chi non le conosce e non le usa in pratica, non apparirà mai agli uomini quale Dio o dèmone o eroe capaci di dirigere al bene gli uomini stessi' [18] - Questo era il giudizio di Platone sulla matematica; ma i matematici non leggono Platone, mentre coloro che lo leggono non conoscono la matematica, e pensano che la sua opinione sia soltanto una curiosa aberrazione » [19].

Siamo quindi in perfetta identità di vedute con Platone nei confronti della matematica, si pensi che, proseguendo, Russell aggiunge, è il caso di dire platonicamente: « La matematica, giustamente considerata, non contiene soltanto la verità, ma la bellezza suprema, una bellezza fredda e austera... sublimemente pura, capace di quell'alta perfezione che soltanto la grandissima arte esprime ». Non solo quindi vi è identità di vedute ma si osserva una stessa evoluzione: dalla definizione insoddisfacente di matematica come logica opinabile, si passa con fervore all'ammirazione incondizionata per la matematica quale verità assoluta, capace di elevare lo spirito e di combattere i dubbi.

A questo punto dobbiamo presentare un ulteriore sviluppo del pensiero di Russell. Questo nuovo sviluppo (che potrebbe essere più apparente che effettivo, come vedremo) nulla toglie alla straordinaria identità che abbiamo descritta tra Platone e Russell, solo che le strade che erano state per un tratto parallele ora divergono.

Nell'introduzione alla seconda edizione de « I Principi della matematica » (scritta trentaquattro anni dopo la stampa delle prima che avvenne nel 1903), Russell, commentando alcune differenze tra quanto aveva scritto nella prima edizione e quanto in quegli anni era mutato grazie allo studio che da più parti aveva contribuito a risolvere alcuni argomenti e ad impostarne altri, pur decidendo di lasciare invariata l'opera, mette in chiaro quali sono gli argomenti rimasti validi e quali invece gli sembrano invecchiati [20]. Tra quest'ultimi, oltre alla definizione di matematica pura (quale classe di tutte le proposizioni della forma « p implica q ») vi è il problema dell'*esistenza* delle costanti logiche e dei numeri che viene concluso con la frase: « Al tempo in cui scrivevo i Principi, dividevo col Frege la credenza nella realtà platonica dei numeri, i quali, nella mia immaginazione, popolavano il regno senza tempo dell'essere. Era una fede consolante, che in seguito abbandonai con rimpianto. Dovrò ora accennare ai vari passi attraverso i quali fui condotto ad abbandonarla ».

Senza addentrarci nell'esposizione dei passi che portarono Russell a mutare la sua opinione, e l'analisi di essi, notiamo solo che lo stesso distacco dalla concezione platonica sulla realtà dei numeri (che per Russell ora sono solo finzioni logiche) la troviamo nella sua « Storia della filosofia occidentale » [21] e precisamente nel capitolo « Percezione e conoscenza in Platone » [22]. Al termine della sua argomentazione sull'esistenza dei numeri, Russell scrive « Le considerazioni suesposte hanno dimostrato che, mentre vi è un tipo formale di conoscenza, la logica e la matematica, che non deriva dalla percezione, i ragionamenti di Platone intorno a tutto il resto della conoscenza sono erronei. Questo non prova, naturalmente, che la sua conclusione sia sbagliata; prova soltanto che egli non ha dato ragioni valide per poterla giudicare vera ».

Quindi si può forse notare che, mentre Russell critica la concezione platonica dell'esistenza dei numeri nel suo mondo iperuranio, egli non faccia che spostare il problema alla logica. Non si tratterebbe cioè di una concezione del tutto diversa ma solo di uno spostamento dell'oggetto da considerare esistente. Può darsi. Certamente è sorprendente da un logico formalista quale si mostra in varie occasioni, leggere che « il problema dell'« irrealtà » che affrontiamo a questo punto è assai importante. Fuorviati dalla grammatica, i logici, nella grande maggioranza, quando se ne sono occupati, ne hanno discusso sul piano sbagliato. Hanno considerato la forma grammaticale come una guida nell'analisi più sicura di quel che in realtà non sia » [23]. Così le frasi « Ho incontrato Jones » e « Ho incontrato un unicorno » differiscono, prosegue Russell, per un fattore di realtà: « La logica direi, non deve ammettere un unicorno più di quanto lo faccia la zoologia; infatti la logica ha a che fare col mondo reale quanto la zoologia, sebbene in termini più astratti e generali » [24] e poco dopo: « C'è un unico mondo, il mondo 'reale' ».

Come si vede questo realismo confina con l'idea di una verità assoluta, non tanto in zoologia, il che è più evidente, quanto nella logica.

Come avverte Gödel [25] questo atteggiamento si è successivamente attenuato, tuttavia Russell in uno dei suoi primi scritti « paragona », scrive Gödel, « gli assiomi della logica e della matematica alle leggi della natura, e l'evidenza logica alla percezione sensoriale... ».

Quale sia stato però il successivo atteggiamento di Russell nei confronti della matematica, e quali altre mutazioni esso possa aver subito negli ultimi anni della lunga vita del filosofo inglese (Russell è vissuto 98 anni); non sarebbe agevole trovare nella sua vastissima produzione [26]. Come abbiamo già detto, l'intenzione di questa nota è solo quello di mostrare quanto di analogo vi è o vi è stato nell'atteggiamento di Russell e di Platone nei confronti della matematica e come il giudizio di quest'ultimo sia ancora oggi una delle possibili posizioni dell'uomo nei confronti di essa.

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NOTE

[1] *Repubblica* 533b, trad. di Attilio Frajese (come per tutti gli altri brani di cui non citeremo il traduttore) cfr. « *Platone e la matematica nel mondo antico* » Universale Studium, Roma 1963 p. 141. Si tratta della raccolta ragionata e commentata di oltre cento brani tratti dalle opere di Platone che hanno attinenza con la matematica, un prezioso aiuto per chi voglia studiare la matematica di Platone e quella dei suoi tempi.

[2] *Repubblica* 527b.

[3] Nel *Filebo* ad esempio (51c) Socrate parla della bellezza delle figure geometriche « queste figure infatti io non le dico belle in relazione a qualche cosa come le altre, ma sono belle in sè, per la loro natura, secondo se stesse e la loro bellezza non vien mai meno e sono naturalmente portatrici di un certo piacere... » (trad. di A. Zadro).
Così nell'*Epinomide*, Platone afferma che senza la matematica non si può essere completamente felici (cfr. 977c-d).

[4] *Repubblica* 525b.

[5] Cfr. *Repubblica* 526b e '*Le Leggi*' 747b.

[6] '*Le Leggi*' 747c; '*Repubblica*' 525b.

[7] Forse anche Pitagora ebbe questo concetto per la matematica oltre all'analogo piacere intellettuale per essa.

[8] Si tratta del famoso brano del *Menone* (82a-85b) in cui Socrate insegna ad un ragazzo come può raddoppiarsi un quadrato assegnato.

[9] *Repubblica* 510c.

[10] *Eutidemo* 290b-c.

[10a] *Repubblica* 511a-d (trad. di F. Sartori).

[10b] Cfr. F. Enriques « *Per la storia della logica* » Zanichelli, Bologna 1922 pp. 10-14.

[11] Cfr. Bertrand Russell « *Misticismo e logica* » Longanesi, Milano 1964 trad. di Luca Pavolini p. 99. Il volume è la raccolta di vari saggi; quello da cui abbiamo tolta la frase è « *La matematica e i metafisici* » p. 98 sgg. scritto nel 1901, come avverte Russell nella prefazione, scritta nel 1917, al libro.

[12] Op. cit. p. 99. Anche il brano seguente si trova nella stessa pagina.

[12a] Questa osservazione l'abbiamo trovata in un commento al corrispondente brano della '*Repubblica*' nell'op.cit. « *Platone e la matematica nel mondo antico* » di A. Frajese.

[13] Molti studiosi, abbiamo detto, parlano di questa definizione di Russell. Chi ha esaminato a fondo il significato di essa è per esempio Giovanni Vailati ne « *La più recente definizione della matematica* » in 'Leonardo' 1905 a cui rimandiamo chi volesse maggiormente approfondire l'argomento. (L'articolo dello studioso italiano è apparso anche recentemente in una piccola antologia di suoi scritti a cura di F. Rossi-Landi: « *Vailati, il metodo della filosofia. Saggi critici del linguaggio* » Laterza, Bari ristampa del 1967).

[14] Cfr. « *Lo studio della matematica* » tratto sempre dal volume cit. « *Misticismo e logica* » p. 77 sgg., scritto nel 1902 come afferma Russell nella prefazione.

[15] op. cit. p. 97.

[16] op. cit. p. 96 « In un mondo così pieno di male e di sofferenza, il ritirarsi nel chiostro della contemplazione, per godere gioie le quali, quantunque nobili, devono essere riservate soltanto a pochi, non può non apparire un rifiuto alquanto egoistico... ».

[17] op. cit. p. 81.

[18] Qui termina la citazione che Russell fa di Platone (« *Le Leggi* » 818b-c). Il Passo di Platone prosegue così (trad. di A. Zadro): « Sarebbe molto lontano dal divenire divino un uomo se non è in grado di conoscere il valore dell'unità, del due e del tre, e complessivamente dei numeri pari e dispari, se non sa affatto contare, se non sa calcolare i giorni e le notti, se ignora le rivoluzioni della luna e degli altri astri ».

[19] Notiamo a questo punto una curiosità: terminata la citazione di Platone, Russell, avvertendo che si tratta delle '*Leggi*', pone una nota in cui vi è scritto: 'Questo passo mi è stato segnalato dal professor Gilbert Murray'. Poco dopo, come abbiamo visto, egli lamenta che i matematici non leggono Platone e coloro che lo leggono non sono matematici. Ebbene, data la nota di Russell è chiaro che lui stesso non aveva letto Platone (o non lo aveva letto tutto). D'altra parte l'osservazione che abbiamo già fatto e che cioè Russell dà una definizione della matematica simile ad una di Platone, lungi dal mostrare una derivazione di cui non si cita la fonte (dato che in altre occasioni Russell si mostra scrupoloso), mostra, anche in questo caso, una non completa conoscenza di Platone.

[20] Cfr. « *I principi della matematica* » di B. Russell, trad. di Ludovico Geymonat, Longanesi, Milano 1963. La citazione che segue si trova a pag. 19.

[21] B. Russell « *Storia della filosofia occidentale e dei suoi rapporti con le vicende politiche e sociali dall'antichità ad oggi* » Longanesi, Milano 1963, trad. di L. Pavolini.

[22] Op. cit. in nota prec. p. 242 sgg. Si noti che il brano citato si trova a p. 254.

[23] Cfr. B. Russell « *Introduzione alla filosofia matematica* » Longanesi, Milano 1947 p. 188 (trad. di L. Pavolini). L'opera fu pubblicata a Londra per la prima volta nel 1919.

[24] B. Russell « *Introduzione alla filosofia matematica* » op. cit. p. 189.

[25] Kurt Gödel « *La logica matematica di Russell* » cfr. l'antologia curata da Carlo Cellucci « *La filosofia della matematica* » Laterza, Bari 1967 che riporta l'intero lavoro (trad. da F. Gana) pp. 81-112. La citazione si trova a p. 84.
Si noti che Gödel è uno dei maggiori studiosi viventi di logica dei principi che riallacciandosi ai « *Principia Mathematica* » di Russell e Whitehead dimostrò il famoso teorema che va sotto il suo nome, sulla autocompatibilità e autosufficienza del sistema assiomatico dell'aritmetica (cfr. ad esempio E. Nagel e J. R. Newman « *La prova di Gödel* », Boringhieri, Torino 1961). Gödel inoltre viene considerato il più valido sostenitore della posizione « platonica » della matematica (cfr. ad esempio l'introduzione di C. Cellucci all'op. cit. e specialmente la p. 17).

[26] Le opere di Russell da noi consultate sono quelle però che vengono considerate le più importanti e indicative agli effetti del suo pensiero sulla matematica.

TRANSLATION: J. E. HOLMSTROM - FOLKESTONE

PLATO AND RUSSELL ON THE DEFINITION OF MATHEMATICS

The article shows the existence of similarities between Plato's and Russell's appreciations of mathematics. It brings out that Plato and Russell used almost identical words in their recognition of the fact that mathematics, being a hypothetical and deductive science, is founded upon postulates which cannot be proved and therefore upon conventions; also that it provides a source of beauty and intellectual delight which reassuringly - though in apparent contrast to this view - conveys underlying truths. The article further displays Russell's view of Plato's love for mathematics and the progressive development, perhaps formal rather than substantial, which Russell's conception of that science has undergone.

It is a notable fact that no completely satisfactory definition of mathematics has been put forward up to now. By limiting its scope to a few particular aspects it is possible to define mathematics in relation to those, but any attempt to make the definition cover the whole of the vast field over which mathematics extends runs up against difficulties which are insoluble or which at any rate have not hitherto been solved.

Personal definitions have been attempted by every mathematician and every philosopher but generally speaking all of these are expressions of individual views on a science whose developments are without any precedent. That being so, the purpose of this article is to juxtapose a few definitions of mathematics, and a few opinions as to its functions and value, that have been advanced by two philosophers who had also a passion for mathematics: namely Plato and Russell.

It is not intended, however, to draw any closer parallel than this between these two persons so different from one another and so separated in time. No comparison between two scholars is ever possible, and whatever analogies can in fact be drawn are very often no more than superficial. Moreover it may be felt without any disparagement of the great English scholar that the two are not comparable, because of different stature. As regards mathematics, however, the intended juxtaposition may be in order, seeing that both have a similar interest and (up to a certain point as will be seen) a similar point of view.

Plato, evidently thought of mathematics as being a hypothetical and deductive science, that is to say a science based on certain concepts such as those of a « point » and a « straight line » and « numbers » (existing, for him, in themselves) and on certain properties thereof which must be accepted as incapable of proof (although probably « true » so far as Plato was concerned); it was on this nonentity, this logical nothingness so to speak, that he erected his structure. In this context the following is a significant quotation from the « Republic » [1]:

« The other arts are nearly all concerned with human opinions and desires, or with the production of natural and artificial things; or with the care of them when produced.... (whereas) geometry and allied studies.... cannot yield anything clearer than a dream-like vision of the real so long as they leave the assumptions they employ unquestioned and can give no account of them. If your premiss is something you do not really know and the intermediate steps are a tissue of things you do not really know, your reasoning may be consistent with itself but how can it ever amount to knowledge? ».

This judgment of the position demonstrates Plato's admirable insight into the logical although relativistic validity of hypothetical and deductive knowledge, as opposed to the Pythagorean concept that mathematics has an absolute reality and is to be regarded as the rational essence of the universe a concept which, except for criticism by the sophists, was to dominate thought among his educated contemporaries. On the other hand, Plato did not limit his judgment of mathematics to this sceptical (although logical) standpoint; he regarded mathematics as a very important element in his conception of what constitutes knowledge. According to Plato, mathematics is the handmaiden of dialectics, collaborating therewith as a means of attaining true intelligence, which means understanding of ideas.

Plato in fact regarded knowledge as having two divisions: opinion and intelligence. He thought of opinion as being further subdivisible into conjecture (directed towards images of visible objects) and beliefs (directed towards true material objects), intelligence as being subdivisible into the «dianoia», which is a peculiarity of mathematical entities, and intelligence properly so called, which consists in the contemplation of ideas. This « dianoia » is to be understood as a kind of intermediate step towards knowledge and, according to Plato, the purpose of mathematics is to provide it, being itself « in the nature of a windlass for hoisting the soul to a higher level of truth, of promoting the philosophical spirit (διάνοια) and of directing the gaze upwards towards the good instead of downwards towards evil » [2].

On the other hand Plato had, a little before this, referred to geometry as « apprehension of something that has always existed (τοῦ ἀεὶ ὄντος) ». Thus while mathematics appears to the logical Plato a mere convention or opinion it appears to the mystical Plato as a reality capable of elevating the soul and of drawing it upwards towards truth in the proper sense of that word.

Countless passages might be cited to indicate the attachment of Plato to mathematics not only as being productive of great intellectual pleasure [3] but also, as has already been shown, a means of « escape from becoming and attainment of being » [4] and a means of sharpening even the less gifted minds [5], so much so that its teaching ought to be made compulsory by law [6].

Plato can, indeed, be considered the founder of mathematical teaching, as being the first to have perceived its educational efficacy [7] and as having given an explicit example of how mathematics may actually be taught [8]. These matters are not relevant here but two other quotations are of interest in so far as they bear upon what is being discussed. The first, taken from the Republic [9], explains in a particular way a point to which a more explicit reference has already been made here but which comes a little later in Plato's work, namely that the significance of mathematics is relative:

« You know, of course, how students of subjects like geometry and arithmetic begin by postulating odd and even numbers, or the various figures and three kinds of angles, and other such data in each subject. These data they take as known; and having adopted them as assumptions, they do not feel called upon to give any account of them to themselves or to anyone else, but treat them as self-evident.... ».

In the other quotation [10], the objective existence of mathematics is explicity asserted - an assertion which, moreover, is implicit in what has already been quoted here (« geometry is apprehension of something that has always existed ») as well as in the general theory of ideas:

« So too with geometers, astronomers and calculators; these also, in their way are hunters, since none of them is a mere maker of diagrams, but discovers abstract - *existing* - truths. Not knowing how to use their prey, but only how to hunt, they hand over their discoveries to competent dialecticians - if any are to be found who are not complete nitwits - ...(just as quail-trappers, I imagine, hand over their birds to quail-keepers) ».

We ought in fact to consider also another significant passage in a dialogue of Plato, or at least the beginning of it, though its interpretation is very controversial [10a]:

« This, then, is the class of things I spoke of as intelligible, but with two qualifications: first, that the mind, in studying them, is compelled to employ assumptions, and, because it cannot rise above these, does not travel upwards to a first principle; and second, that it uses as images those actual things which have images of their own in the section below them and which, in comparison with those shadows and reflections are reputed to be more palpable and valued accordingly. - I understand: you mean the procedure of geometry and of the kindred arts. - Then by the second section of the intelligible world you may understand me to mean all that unaided reasoning apprehends by the power of dialectic, when it treats its assumptions not as first principles but as *hypotheses* in the literal sense, things 'laid down' like a flight of steps up which it may mount all the way to someting that is not hypothetical, the first principle of all; and having grasped this, may turn back and, holding on to the consequences which depend upon it, descend at last to a conclusion, never making use of any sensible object but only of Forms, moving through Forms from one to another, and ending with Forms. - I understand, he said, though not perfectly; for the procedure you describe sounds like an enormous undertaking. But I see that you mean to distinguish the field of intelligible reality studied by dialectic as having a greater certainty and truth than the subject-matter of the 'arts', as they are called, which treat their assumptions as first principles. The students of these arts are, it is true, compelled to exercise thought in contemplating objects which the senses cannot perceive; but because they start from assumptions without going back to a first principle you do not regard them as gaining true understanding about those objects, although the objects themselves when connected with a first principle are intelligible. And I think you would call the state of mind of the students of geometry and other such arts, not intelligence, but thinking, as being somethbing intermediate (μεταξύ) between intelligence (δόξα) and mere acceptance of appearances (νοῦς) ».

Subject to its various interpretations (such as the very penetrating and learned one given by Federigo Enriques [10b]) it seems clear, then, that mathematical knowledge cannot be of the same type, or belong to the same class as dialectical knowledge, but can provide only an intermediate stage, a windlass by means of which the latter may be hoisted up. This is because the former, unlike the latter, is based on principles, needing to be assumed without understanding their true essence and can itself be understood only by making use of material objects (though the properties of these can be extrapolated by a rational process).

Plato's position is, therefore, clear without any need for further citations: namely that mathematics as a logical structure has to be founded upon certain premisses which are incapable of proof or justification and in this sense it appears to be a mere convention; but on the other hand, mathematics, quite apart from the pleasure it excites and the dialectical use that can be made of it, provides a road whereby the mind may travel towards truth and be enabled to contemplate the *existence* of rational concepts as such (whereas pictures can never be more than imperfect images of them).

Here we may turn to consider the similar position taken up by Bertrand Russell: the aphorism whereby he defines mathematics may be described as a new edition of what Plato had already thought out and expressed more than two thousand years earlier. The hypothetical and deductive nature of the chain of propositions that constitute mathematics being recognised, he states [11] that « pure mathematics » consists entirely of assertions to the effect that if such and such a proposition is true of *anything* then such and such another proposition is true of *that* thing. It is essential not to discuss whether the first proposition is really true, and not to mention what the anything is, of which it is supposed to be true ». Starting, he goes on to say, from certain rules of inference (which are those that constitute the major part of formal logic) « we then take any hypothesis that seems amusing, and deduce its consequences » - provided only that « our hypothesis is about *anything*, and not about some one or more particular things ». Thus [12] pure mathematics « may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true ».

This, it will be noted, is an astonishing coincidence with our first quotation from Plato [12a]. Russell cannot have known that the Athenian philosopher had expressed almost the same idea, for had he known it he would have mentioned this like so many other references.

Russell's saying has been taken as a model by everyone wanting to summarise aptly, or perhaps somewhat to disparage, the significance of mathematics as a hypothesis-propounding deductive science; or else it has been repeated as a « boutade » uttered by the English philosopher not without some truth.

Actually, without going into the merits of the question, it is impossible not to agree that in a strictly logical sense this saying of Russell's is justified in its applications to a hypothetical-deductive science such as mathematics may be. If it leaves in its wake a feeling of dissatisfaction (or if it leaves *us*, at least, with such a feeling) this may signify merely that the problem of defining mathematics is far from being soluble by such a phrase [13].

Such, moreover, is the attitude of Russell himself who explicitly shows, in another of his essays [14], that he, like Plato as already cited here, regards mathematics as objective truth, a life-raft for rescue from scepticism:

« Of the effects which it - mathematics - is likely to have in the future it would be very rash to say much; but in one respect a good result seems probable. Against that kind of scepticism which abandons the pursuit of ideals because the road is arduous and the goal not certainly attainable, mathematics, within its own sphere, is a complete answer. Too often it is said that there is no absolute truth but only opinion and private judgment; that each of us is conditioned, in his view of the world, by his own peculiarities, his own taste and bias; that there is no external kingdom of truth to which, by patience and discipline, we may at last obtain admittance, but only truth for me, for you, for every separate person. By this habit of mind one of the chief ends of human effort is denied, and the supreme virtue of candour, of fearless acknowledgment of what is, disappears from our moral vision. Of such scepticism mathematics is a perpetual reproof; for its edifice of truths is unshakable and inexpugnable to all weapons of doubting cynicism ».

And here is the conclusion of this essay by Russell, following his exaltation of the function that mathematics performs [15]:

« For the health of the moral life, for ennobling the tone of an age or a nation, the austere virtues have a strange power, exceeding the power of those not informed or purified by thought. Of these austerer virtues the love of truth is the chief, and in mathematics, more than elsewhere, the love of truth may find encouragement for wandering faith. Every great study is not only an end in itself, but also a means of creating and sustaining a lofty habit of mind; and this purpose should be kept always in view throughout the teaching and learning of mathematics ».

Thus, for Russell too, mathematics is a « windlass » for the attainment of truth as well as a joy in itself [16]:

At this point it is of interest to note Russell's opinion of Plato, which is brought out by the following words at the beginning of the paper wherefrom the later of these quotations have been taken [17]. « Plato, we know, regarded the contemplation of mathematical truths as worthy of the Deity; and Plato realised, more perhaps than any other single man, what those elements are in human life merit a place in heaven. There is in mathematics, he says, « something which is *necessary* and cannot be set aside.... and, if I mistake not, of divine necessity; for as to the human necessity of which the Many talk in this connection, nothing can be more ridiculous than such an application of the words. *Cleinias;* And what are these necessities of knowledge, Stranger, which are divine and not human? *Athenian:* Those things without some use or knowledge of which a man cannot become a God to the world, nor a spirit, nor yet a hero, nor able earnestly to think and care for man. (*Laws*, p. 818). Such was Plato's judgment of mathematics; but the mathematicians do not read Plato, while those who read him know no mathematics, and regard his opinion upon this question as merely a curious aberration » [18].

Being, therefore, in perfect agreement with Plato's views on mathematics, Russell himself is moved to add platonically [19]:

« Mathematics, rightly viewed, possesses not only truth, but supreme beauty - a beauty cold and austere, like that of sculpture, without appeal to any part of our weaker nature, without the gorgeous trappings of painting or music, yet sublimely pure, and capable of a stern perfection such as only the greatest art can show ». Evidently, then, there is coincidence not only of the sentiment but also of the evolution undergone in passing from dissatisfaction with the definition of mathematics in terms of subjectively conditioned logic to fervent and unconditional admiration of mathematics as being absolute truth, able to lift the human spirit and conquer its hesitations.

Here a later development of Russell's thinking calls for mention - one which (as will be seen) may be more apparent than real, and which removes none of the extraordinary sameness of Plato's and Russell's respective positions save that their paths which at one stage ran parallel are now diverging.

In the introduction to the second edition of his *Principles of Mathematics* (written thirty-four years after the first edition was printed in 1903) Russell commented on certain differences between what he had said in the first edition and what he had been led to change in the intervening years. In this situation he had decided to leave the text unaltered but to distinguish clearly between the arguments still valid and those he deemed out of date [20]. Among the latter, besides the definition of pure mathematics (comprising all propositions in the form « p implies q ») there is the problem of the *existence* of logical constants and of numbers, a problem on which he now remarked as follows: « At the time when I wrote the *Principles* I shared with Frege a belief in the Platonic reality of numbers, which, in my imagination, peopled the timeless realm of 'Being'. It was a comforting faith which was later abandoned with regret. Something must now be said of the steps by which I was led to abandon it ».

Without entering into detail of the steps that led Russell to change his position or attempting to analyse them, let us merely note that this same departure from the Platonic concept of the reality of numbers (which Russell now regards merely as logical fictions) is recorded in his *History of Western Philosophy* [21] under the chapter heading « Knowledge and perception in Plato ». Russell concludes his argument about the existence of numbers as follows [22]: « The above considerations have shown that, while there is a formal kind of knowledge, namely logic and mathematics, which is not derived from perception, Plato's arguments as regards all other knowledge are fallacious. This does not, of course, prove that his conclusion is false; it proves only that he has given no valid reason for supposing it true ».

It may therefore be considered that Russell, in criticising Plato's conception of numbers existing in the latter's « hyper-uranian » world, has merely transferred the problem to the field of logic. In other words the conceptualisation of the problem as a whole has not been changed but only the object to be considered as existing. Maybe so. It is indeed surprising to find a formal logician, such as Russell in several contexts shows himself to be, declaring that « the concept of 'unreality', which confronts us at this point is a very important one. Misled by grammar, the great majority of logicians who have dealt with this question have dealt with it on mistaken lines. They have regarded grammatical form as a surer guide in analysis than, in fact, it is » [23]. Thus, Russell continues, the statement « I met Jones » differs by a factor of reality from the statement « I met a unicorn ». « Logic, I should maintain, must no more admit a unicorn than zoology can; for logic is concerned with the real world just as truly as zoology, though with its more abstract and general features » [24] - and, a little later, « there is only one world, the 'real' world ».

It will be noted that this idea of realism is bound up with that of absolute. truth not only in respect of zoology, where it is more obvious, but also in logic.

As Gödel has pointed out [25], this conclusion was later moderated although Russell in one of his earlier writings had compared the axioms of logic and mathematics with the laws of nature and compared logical evidence with sensual perception.

However, Russell's productivity was so abundant that whatever conclusion he may later have reached as regards mathematics, and whatever other changes this may have undergone during the last years of the English philosopher's long life (Russell lived to be 98), would not be easy to discover [26].

As already stated, the only purpose of this note is to point out the similarity that exists, or has existed, between Russell's and Plato's attitudes to mathematics, and that Plato's judgement of the question remains today one of the possible standpoints it is possible for man to maintain.

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REFERENCES

(*Translator's note: - The references to Italian publications cited in the original article are replaced here by references to corresponding authoritative English versions of Plato's and Russell's works from which the extracts are quoted*).

[1] *Republic* 533b, at p. 248 in F. M. Cornford, *The Republic of Plato.* Oxford: Clarendon Press, reprinted 1951.

[2] *Republic* 527b,

[3] For instance, Socrates is made to refer, in *Philebus* (51c), to the beauty of geometrical shapes contending that these « are not merely relatively beautiful, like other shapes; they are so always and in their very nature, and the pleasures they occasion are of a specific kind ». - A. E. Taylor, *Plato: Philebus and Epinomis.* London. T. Nelson & Sons Ltd., 1956, p. 173. Likewise, in *Epinomis* (977 c-d), Plato asserts that without mathematics it is impossible to be completely happy. Idem. p. 227.

[4] *Republic,* 525b.

[5] Cf. *Republic,* 526b, and *Laws,* 747b.

[6] *Laws,* 747c; *Republic,* 525b.

[7] Pythagoras too may perhaps have thought in this way of mathematics and of the intellectual pleasure it affords.

[8] This refers to the famous passage in *Menon* (82a-85b) where Socrates is teaching a boy how to double a given square.

[9] *Republic* 510c, at p. 220 in Cornford, op. cit.

[10] *Euthydemus* 290b-c, at p. 173 in translation by J. Warrington included in *Plato: Symposium and other dialogues.* Everyman's Library, London. J. M. Dent & Sons Ltd. 1964.

[10a] *Republic,* 511 a-d, at p. 220-1 in Cornford, op. cit.

[10b] Cf. F. Enriques, *Per la storia della logica.* Zanichelli, Bologna, 1922, pp. 10-14.

[11] B. Russell, *Mysticism and Logic* (*and other essays*). London: Longmans, Green & Co., 1918. This volume is a collection of various papers including that quoted here which appears under the title « Mathematics and the metaphysicians » starting at p. 74 of the English edition and, as stated in the author's preface, originally written in 1901.

[12] Russell at p. 75 in ref. 11.

[12a] This observation was discovered in a comment on the corresponding passage of the *Republic* contained in « *Platone e la matematica nel mondo antico* », by A. Frajese.

[13] As already mentioned, this definition by Russell has been discussed by many scholars. As an example of one who has discussed it exhaustively, to whom the reader may be referred if he wishes to go into the question more deeply, the paper by Giovanni Vailati entitled « *La più recente definizione della matematica* » which appeared in *Leonardo,* 1905 may be cited. (This Italian scholar's article has recently been included also in an anthology of his work compiled by F. Rossi-Landi under the title « *Vailati, il metodo della filosofia. Saggi critici del linguaggio* » reprinted in Bari by Laterza, 1967).

[14] Russell at p. 71 in « *The study of mathematics* », another of the essays in op. cit., written in 1902 according to Russell in his preface.

[15] Russell at pp. 72-3 in ref. (14).

[16] Russell at p. 72 in ref. (14). «In a world so full of evil and suffering, retirement into the cloister of contemplation, to the enjoyment of delights which, however noble, must always be for the few only, cannot but appear as a somewhat selfish refusal of the burden imposed upon others by accident in which justice plays no part... ».

[17] Russell, at p. 59 in ref. (16).

[18] Here ends Russell's quotation from Plato (« *The Laws* » 818b-c). The passage from Plato continues as follows: « A man would be very far from becoming divine if he is not able to know the value of unity, of two and of three, and of all the even and odd numbers, if he cannot count at all, if he cannot calculate the days and the nights, if he is ignorant of the revolutions of the moon and of the other astral bodies ».

[19] At this point, let us note a curiosity: after concluding the quotation from Plato, Russell, mentioning that it is from the « *Laws* », adds a note in which he says: « This passage was pointed out to me by Prof. Gilbert Murray ». A little later, as we have seen, he complains that mathematicians do not read Plato and that those that read him are not mathemati-

cians. Now, in view of Russell's note, it is clear that he himself had not read Plato (or had not read all of him). On the other hand, the observation that we have already made - namely, that Russell gives a definition of mathematics that is similar to one of Plato's - far from showing a derivation of which the source is not quoted (seeing that, on other occasions, Russell appears scrupulous), shows, also in this case, an incomplete knowledge of Plato.

[20] Russell, B. «*Principles of Mathematics*». London: Allen & Unwin, second edition, 1967.

[21] Russel, B. «*History of Western Philosophy and its connection with political and social circumstances from the earliest times to the present day*». London: Allen & Unwin Ltd. 1940.

[22] The quotation is from p. 179 of ref. (21).

[23] Russel, B. «*Introduction to Mathematical Philosophy*». London: Allen & Unwin Ltd. 1919, twelfth impression 1967 (the first quotation here is at p. 168).

[24] Russell at p. 169 in ref. (23).

[25] In « *La logica matematica di Russell* » by Kurt Gödel reference is made to the anthology compiled by Carlo Cellucci, « *La filosofia della matematica* » (published by Laterza, Bari, 1967) wherein the whole work is reproduced in an Italian translation made by F. Gana, pp. 81-112. The present citation is at p. 84.

It should be noted that Gödel is one of the most eminent living authorities on the logical principles that derive from the « *Principia Mathematica* » in which Russell and Whitehead demonstrated the famous theorem, known by his name, on the self-consistency and self-sufficiency of the axiomatic system of arithmetic (see, for example, « *La prova di Gödel* » by E. Nagel and J. R. Newman, published by Boringhieri, Turin, 1961). Gödel moreover is considered to be the most convincing maintainer of « Platonic » position in mathematics, (see, for instance, the introduction to op. cit. by C. Cellucci, especially p. 17).

[26] However, Russell's works consulted by the present writer are those considered to be the most important in their bearing upon mathematical thought.

REFLEXIONS ON SCIENTIFIC RESEARCH

Scientific research is regarded as sequences of transformations of complexes composed of knowledge, problems and instruments. Instruments are either hardware laboratory equipment or software techniques used in problem solving. The transformation sequences are embedded in human settings in which they are given directions. Problems and solutions of them are arranged as follows.

Problems of order	*Solutions*
0	*Data*
1	*Single pieces of knowledge*
2	*Explanatory patterns*
3	*Systems of knowledge*

Information theory is applied to procedures starting from problems of orders 1 and 2 and ending with solutions of them which are single pieces of knowledge and explanatory patterns respectively. Empirical work in a strict sense is concerned with problems of orders 0 and 1 and theoretical work is concerned with problems of orders 2 and 3. Empirical and theoretical work are interrelated by means of flows of problem generating information. Residuals of knowledge which cannot be systematized at the same rate as knowledge is produced, and clashes between old and new knowledge are potential starting points of new lines of research.

1. One way of looking at research. — There are many ways of looking at science. In this paper scientific research is conceived of as sequences of transformations of knowledge, problems and instruments. Instruments are either hard-ware laboratory equipment or soft-ware intellectual techniques used in problem solving activities.

These transformation sequences are embedded in human settings which in various ways determine the directions of research.

Figures 1a) and 1b) illustrate transformations of knowledge K, problems P and instruments I.

In fig. 1a) S P designates soluble problems. A scientist in possession

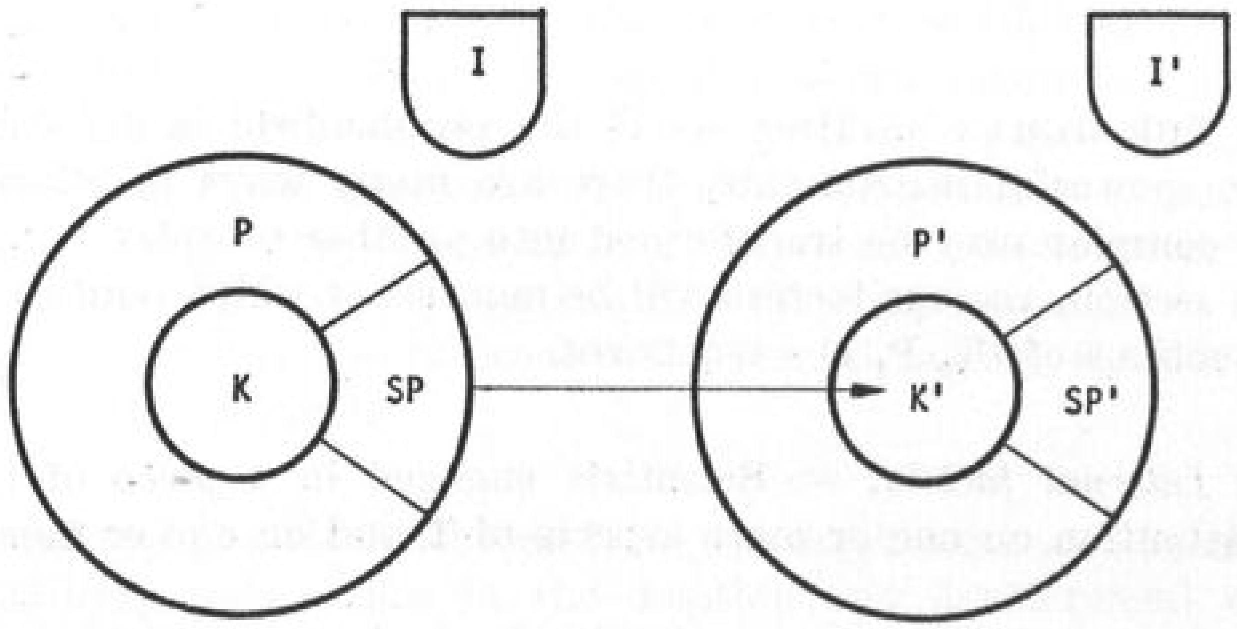


Fig. 1a

of knowledge K and instruments I is capble of solving problems inside S P. When he solves one or more of the problems, K will be changed into K'. The changes K → K' give rise to a transformation of the problemfield P into the problemfield P'. New problems occurring in P' may give rise to a transformation of I into I'. This transformation may be endogeneous, the scientist creates new instruments, or exogeneous, the scientist imports instruments from another field of studies. S P' consists of those problems in P' which the scientist can solve given K' and I'.

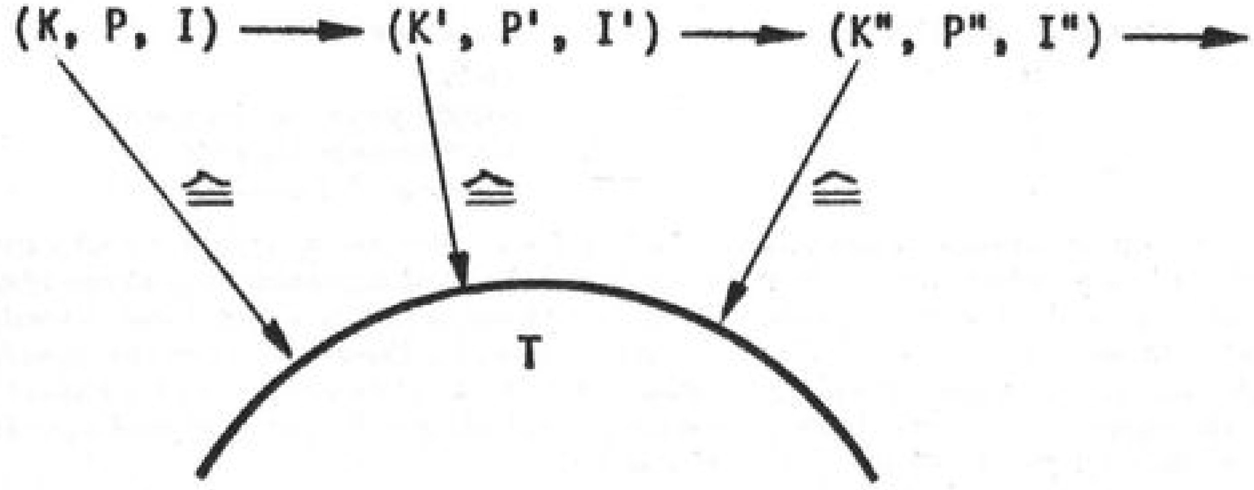


Fig. 1b

T in fig. 1b) designates a part of the real world which the scientist investigates. 'T' stands for « territory ». The sequence K, K', K".... is growing knowledge about T. The arrow ≙ designates epistemic mapping relations between K, (K', K" etc.) and T.

Research is progressive inso far as the epistemic mapping relation is improving. This is the case if K' maps more features than K, and K" more than K' etc. and if furthermore the degree of truth increases, i.e. if K' contains a larger proportion of true information than K, and K" a larger proportion than K' etc.

2. THE HUMAN SETTING. — If the problemfield is extensive and if I are powerful instruments, there are many ways in which a (K, P, I) - complex may be transformed into another complex (K', P', I'). In this section, various factors will be mentioned which tend to restrict the directions of (K, P, I) - sequences.

a) *Internal factors.* — Scientists engaged in a piece of research focus attention on one or more aspects of T and on one or more levels of T.

One example of a perspective is Newton's way of looking at the solar system as composed of molar bodies in empty space under the influence of gravitational forces acting at a distance. Radiations are entirely outside this perspective.

Ideals of science act as a directing factor. Non-physicists may regard research in physics as an ideal. They prefer hard data (outputs of measurements) to soft data (verbal messages from human beings). They wish to make use of mathematical models in their theoretical work in place of verbal ones. They prefer experimental procedures to other methods of obtaining information concerning their territory etc.

In transformations of (K, P, I) - complexes, scientists expose the complexes to critical examinations. Such criticism plays a major role in research procedures. Discovered imperfections in a growing body of knowledge or in an inventory of instruments give rise to new problems which enter the problemfield and affect thereby later (K, P, I) - complexes.

In their criticism, scientists use explicit as well as implicit criteria of excellence concerning concepts used in forming a body of knowledge, concerning claims made on behalf of hypotheses that these deserve to be accepted as knowledge, concerning explanations offered and concerning larger bodies of systematized knowledge than explanatory patterns.

Systems of criteria used by scientists are related to their ideals of knowledge. If for example scientists have a physicalistic ideal of science, they will accept explanations only if patterned as deductive arguments, and they will accept theories only if they have the form of deductive systems etc.

In a list of internal steering factors, one should also incorporate the organizational forms in which research takes place. This is a particularly important factor when many scientists take part in the research procedures. The interest of the scientists as well as their previous research experience is also of importance.

b) *External factors.* — The interests and evaluations of others outside the research group may also affect research procedures. Knowledge and instruments produced elsewhere may be utilized. (K, P, I) - sequences found outside may ge regarded as paradigms and affect the ideals of science. Indeed they, or rather their perception of them, may be regarded as ideals of research by a group of scientists thus affecting the ways in which they frame their problems and criticize their results. These determinants are external factors directing research.

Fig. 2 exhibits the internal and external factors that determine a direction of research.

Empirical and theoretical work. — Research work can be classified into *auxiliary work* aiming at the creation and development of techniques and hardware tools used in the production of knowledge, *empirical work* in a restricted sense, and *theoretical work.*

Empirical work. — The empirical work may be experimental work or consist of systematic observations as in astronomy, or may be conducted in other forms. In the humanistic branches of science it often consists in consultations of written documents and other traces of the past. In empirical work, data problems are very important. What kind of data must be gathered? How shall data be interpreted?

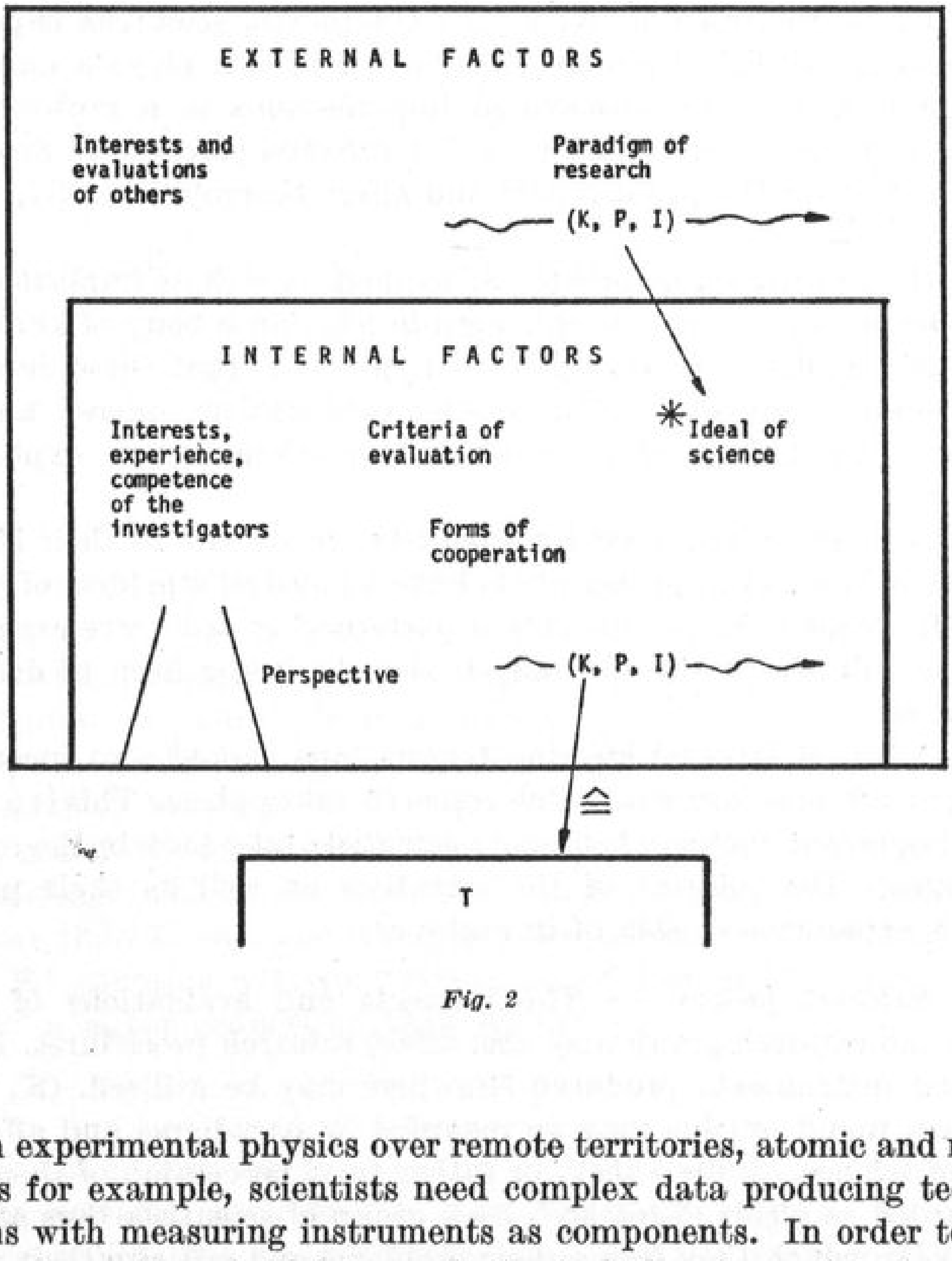

EXTERNAL FACTORS
Interests and evaluations of others
Paradigm of research
(K, P, I)
INTERNAL FACTORS
Interests, experience, competence of the investigators
Criteria of evaluation
Ideal of science
Forms of cooperation
Perspective
(K, P, I)
≙
T

Fig. 2

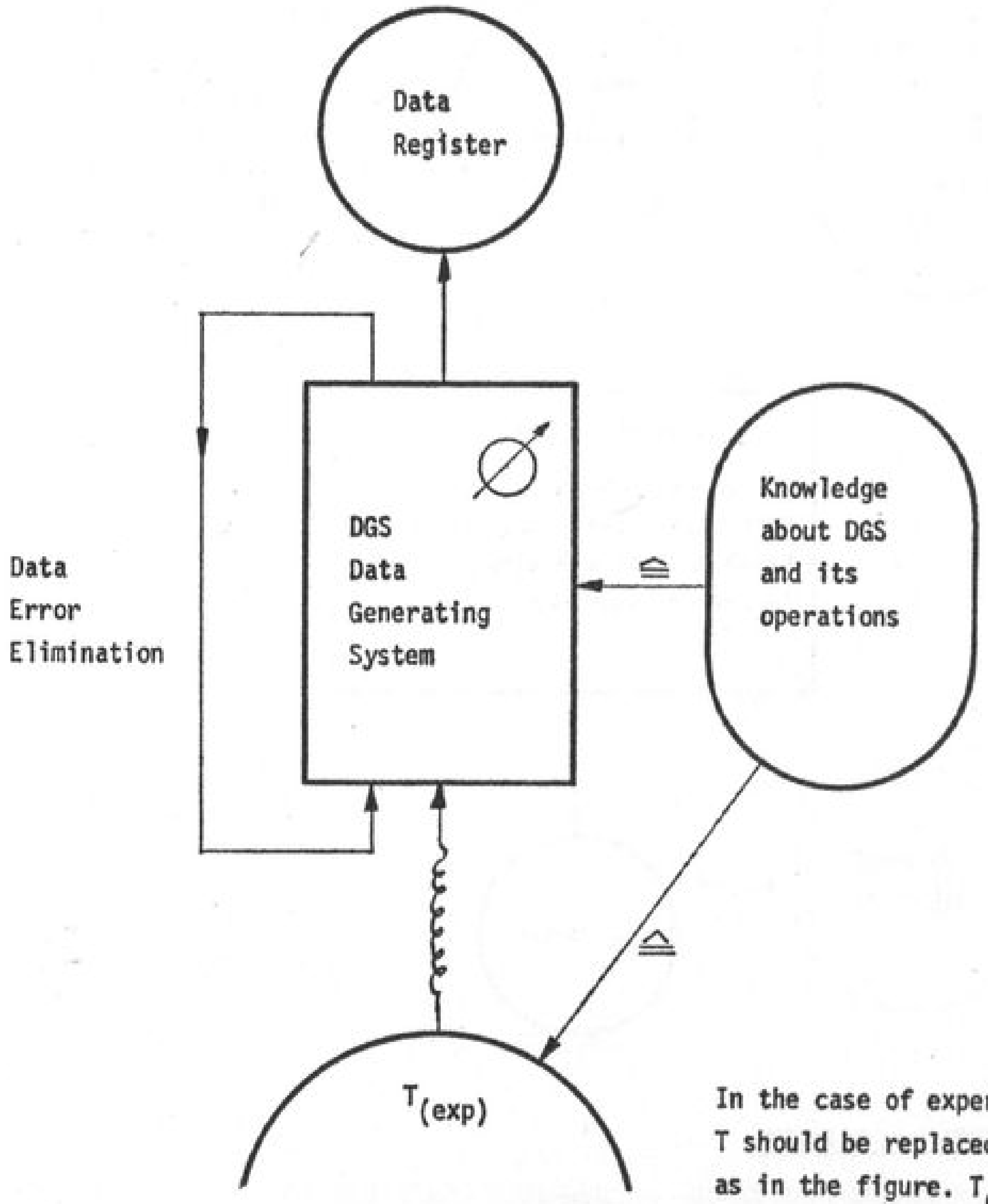
In experimental physics over remote territories, atomic and nuclear physics for example, scientists need complex data producing technical systems with measuring instruments as components. In order to build such systems, the physicists need physical theories and technological know-how. They also need auxiliary techniques in order to control the output of the data generating systems so as to ensure that data provide trustworthy information on features of interest in the territory under investigation.

Data and previous knowledge are used as rawmaterials in the production of knowledge about features of the territory under investigation. Historical examples of such knowledge are the gas laws, which describe possible states of gases, Ohms law and laws describing the photo electric effect, the Compton effect, the Zeeman and Stark effects etc.

Data are also used as evidence for or against hypotheses framed as tentative answers to problems: « How are state variables related to each other? » « How do states of a system change in time? » etc.

It is useful to distinguish between two kinds of problems, two kinds of solutions and two kinds of procedures in empirical work.

1) Problems of order 0, p_0, knowledge of order 0, k_0 and knowledge formation of order 0, kf_0.
2) Problems of order 1, p_1, knowledge of order 1, k_1, and knowledge formation of order 1, kf_1.

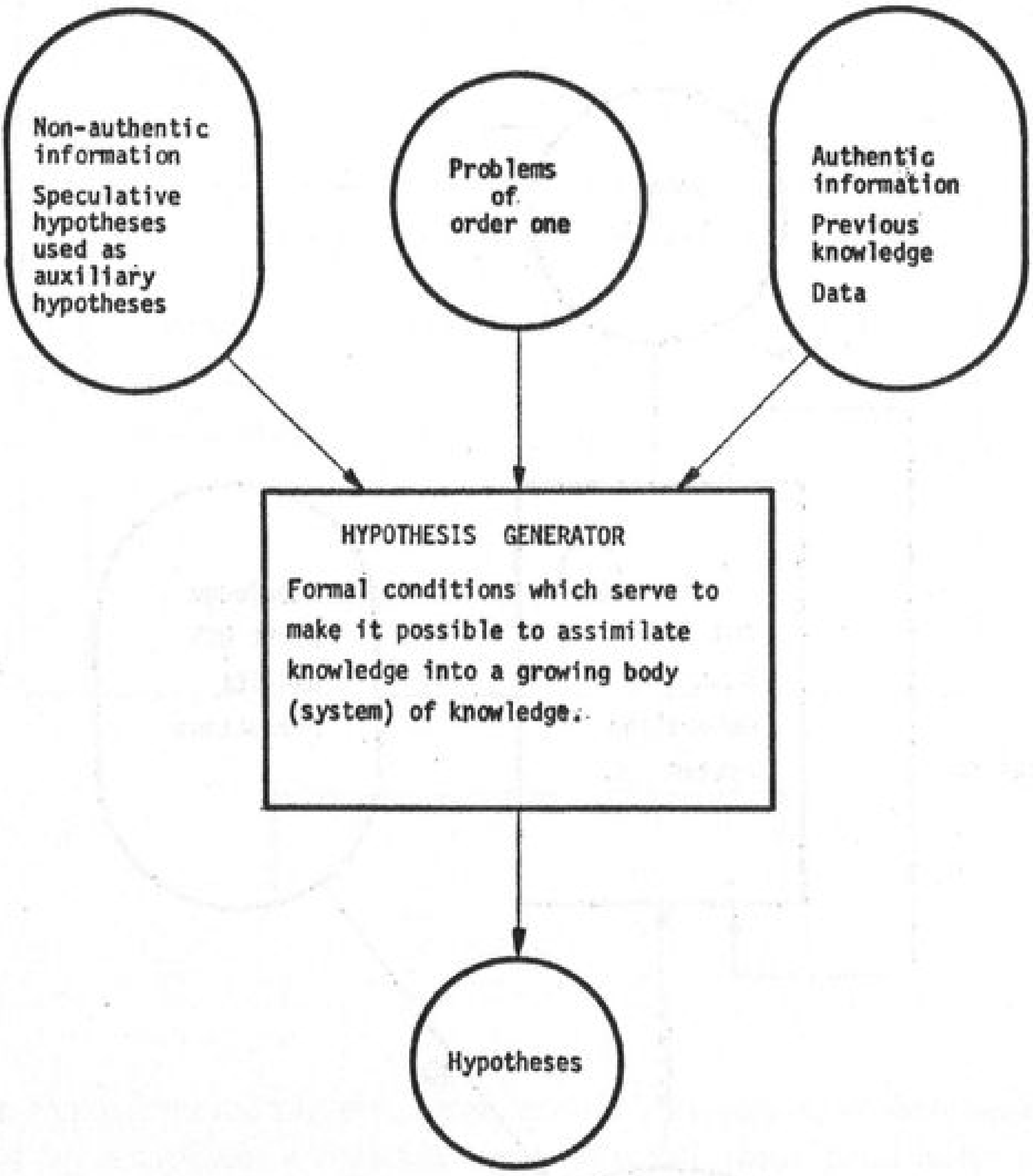

Data
Register
Data
Error
Elimination
DGS
Data
Generating
System
≙
Knowledge
about DGS
and its
operations
≙
$T_{(exp)}$

In the case of experiments T should be replaced by $T_{(exp)}$ as in the figure. $T_{(exp)}$ is a real system prepared in special ways so as to be analogous to a territory T under investigation.

DATA GENERATION

Fig. 3a

Authentic data are examples of knowledge of order zero. Data provide answers to problems of order 0. In order to answer such problems, (for example the question: « What is the pressure of this gas? »), it may be necessary first to construct suitable measuring instruments and more extensive data generating systems. It may be necessary also to invent procedures of using this equipment and to control the output. We have here problems related to problems of order 0. These instrumental and procedural problems will be regarded as parts of the problems of order 0. Knowledge formation of order 0 thus includes all procedures which lead to veridical data.


Non-authentic information
Speculative hypotheses used as auxiliary hypotheses
Problems of order one
Authentic information
Previous knowledge
Data
HYPOTHESIS GENERATOR
Formal conditions which serve to make it possible to assimilate knowledge into a growing body (system) of knowledge.
Hypotheses

HYPOTHESIS GENERATION

Fig. 3b

Problems of order 1 are concerned with components, structures, states and processes of systems under investigation. Examples of such problems are: « How is the period of a pendulum related to its length? » « How is pressure, volume and temperature related to each other in the case of a gas in equilibrium? » « How do planets move in the solar system? » « How are the spectral lines of a gas effected by electrical and magnetic fields? » etc.

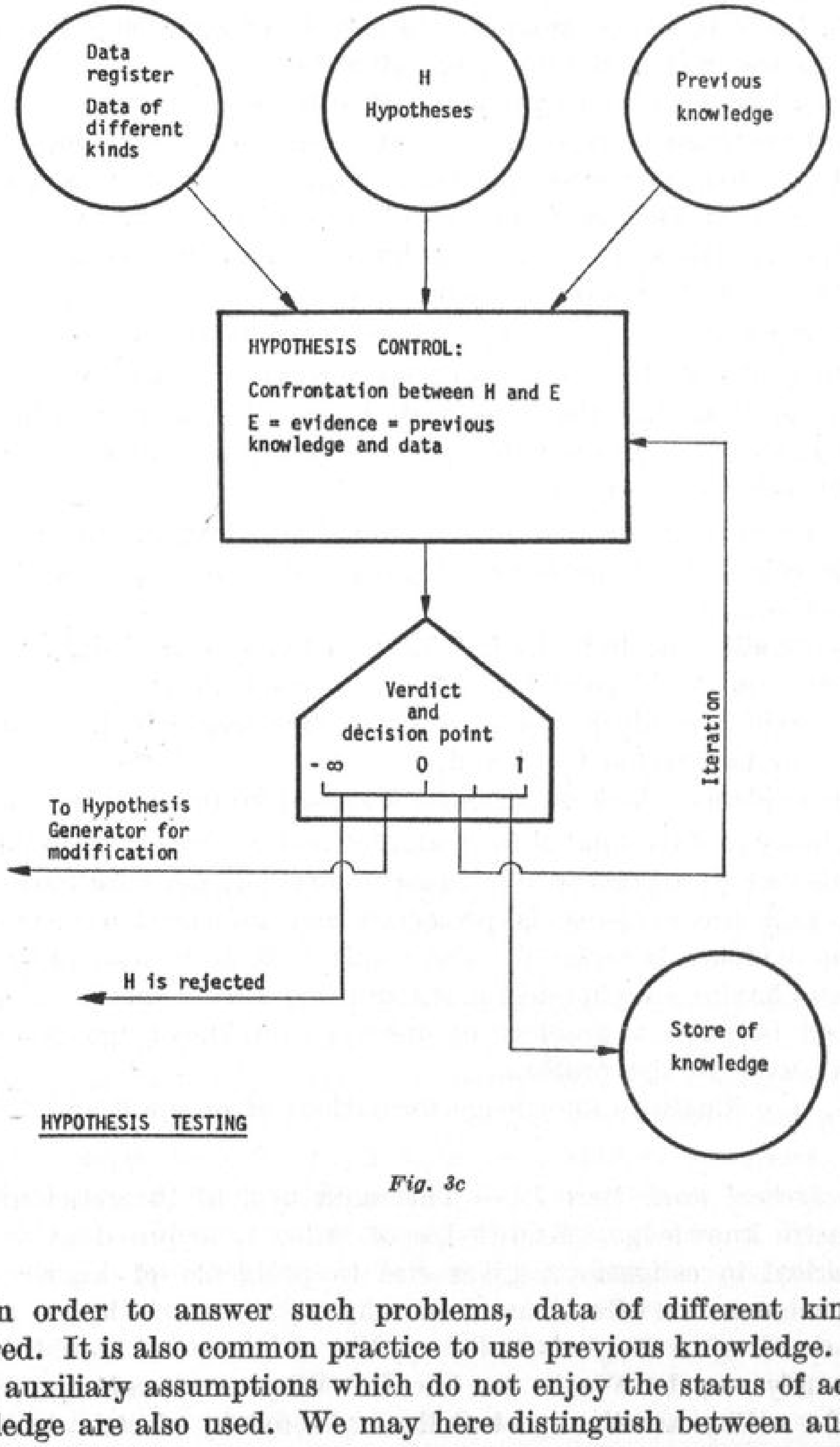

Data register
Data of different kinds
H
Hypotheses
Previous knowledge
HYPOTHESIS CONTROL:
Confrontation between H and E
E = evidence = previous knowledge and data
Verdict and décision point
- ∞
0
1
Iteration
To Hypothesis Generator for modification
H is rejected
Store of knowledge
HYPOTHESIS TESTING

Fig. 3c

In order to answer such problems, data of different kinds are required. It is also common practice to use previous knowledge. Sometimes auxiliary assumptions which do not enjoy the status of accepted knowledge are also used. We may here distinguish between authentic information, A I, data and previous knowledge, and non-authentic information, A' I.

Knowledge formation of order 1 is a complex of operations.

Authentic, and perhaps also non-authentic, information provides clues to the framing of hypotheses which are possible answers to a problem of order 1.

These hypotheses are often required to satisfy formal desiderata: an hypothesis ought to be expressed in a language which will make it possible for it to be assimilated into a body of knowledge if it is promoted to the status of knowledge after testing.

Tentative answers to problems of order 1 are tested.

An hypothesis to be tested is confronted with other information of type A I (authentic information = data and previous knowledge). The outcome of such confrontations are verdicts: « The evidence supports the hypothesis » or « The evidence is unfavourable ».

Verdicts give rise to decisions.

1) It is decided in the case of favourable evidence, either
   a) to confront the hypothesis with further evidence, or
   b) to propose that the hypothesis is accepted as knowledge. The hypothesis becomes public knowledge if this proposal is accepted by other experts.
2) It is decided in the case of unfavourable evidence, either
   a) to reject the hypothesis altogether and to frame another hypothesis, or
   b) to modify the hypothesis h into an hypothesis h' slightly different from h. h' should satisfy these conditions:
      α) evidence which has been found favourable to h should also be favourable to h' and
      β) evidence which undermines h should be favourable to h'.

If decision of the kind 2b) are adopted and implemented, hypotheses are subjected to a process of gradual refinement, i.e. information consistent with the evidence is preserved and information inconsistent with the evidence is replaced. The result is likely to lead to the later hypltheses having a higher degree of truth than earlier ones in a sequence interlaced between a problem of order 1 and knowledge of order 1 being answers to the problem.

Fig. 3a, b, c illustrate knowledge formations of orders 0 and 1.

Theoretical work Part 1. — The main task of theoreticians is to systematize knowledge. Knowledge of order 1, acquired as a result of empirical investigations, gives rise to problems of higher order. These problems are often framed as why-questions. « Why do planets move according to Kepler's laws? » « Why do states of equilibrium in gases satisfy van der Waal's gas laws? » « Why are atomic spectra line spectra? » « Why are the spectral lines affected by electric fields and magnetic fields in accordance with Starks and Zeeman's laws? » etc. Answers to such why-questions have the form of explanatory patterns. In physics, acceptable explanatory patterns are deductive arguments.

The permisses include virtual (not yet accepted) or genuine knowledge of a kind which is produced by theoreticians in the process of solving problems of order 2.

I will refer to such « knowledge », genuine or not genuine, as star-hypotheses. Examples of star-hypotheses are: Archimedes' principles which serve, among other things, to account for the behaviour of solids in liquids, Newton's principles of motion and his law of gravitation which serve to account for the motions of the planets and the behaviour of falling bodies etc., Planck's quantum hypothesis, Einstein's photon hypothesis, Bohr's quantization rules etc., which have served to account for blackbody radiation, the photo-electric effect, the Compton effect, the line spectra of hydrogen and hydrogenlike atoms etc.

There are different kinds of research situations in which problems of order 2 arise.

1. When a territory is little known beforehand, a body of not yet systematized knowledge is built up. A problem of higher order is to expand this body by means of additional hypotheses (star-hypotheses) thus enabling components of the body to be deduced from premisses in the expanded body.
2. Systematized knowledge, K S, has already been produced as a result of previous research. Knowledge of order 1 has been formed as a result of empirical investigations. We consider the following three cases:
   a. It is possible to deduce k from previous knowledge K i.e. $K \rightarrow k$.
   b. k is consistent with K but it is not possible to deduce k from K.
   c. k is inconsistent with K.

Case c is an anomaly which we will not consider in this section.

In case a, p_2 is a mathematical problem, i.e. the problem of deducing k from K. In order to solve this kind of problem it may be necessary to invent or import mathematical techniques.

A solution of p_2 in case a does not increase the amount of information carried by K. (In fact k does not add information to K, since k is not *new* knowledge except possibly in a psychological sense). The effect of a successful solution of p_2 in case a is that k helps to consolidate those parts of K which have been used as premisses.

In case b, in order to deduce k, at least one new hypothesis will be needed in addition to premisses which are pieces of knowledge.

Deductive patterns in which star hypotheses are present are not genuine explanatory patterns until the star hypotheses have been promoted to the status of knowledge. They may therefore be described as virtual explanatory patterns.

It is often not possible to test star hypotheses in the same way as hypotheses offered as virtual answer to problems of order 1.

Another procedure of testing is adopted. Problems of order 1 are framed.

Star-hypotheses, which carry non-authentic information, and previous knowledge are used as input information to an hypothesis generator.

Answer to problems of order 1 are obtained by means of deductions from h^x, the star-hypotheses and from previous knowledge. The conclusions are tested. If the latter are promoted to the status of knowledge, they provide favourable evidence for the star-hypotheses. The star-hypotheses are thus tested by proxy as it were. As all « daughter hypotheses » are deducible from premisses consisting of star-hypotheses and previous knowledge, a whole family of virtual explanatory patterns is generated. All explanatory patterns of a family share common premisses which are star-hypotheses. The conclusions in these patterns support the common premisses.

The support becomes stronger, the more the family grows. Finally, the star-hypotheses will be accepted as public knowledge by the concensus of experts. Such knowledge is knowledge of a higher order than order 1.

A formal theory of knowledge formation. — In order to combine the above primarily descriptive account of knowledge-formations of orders 1 and 2 with a normative one it is useful to adopt a formal approach. When historians test a narrative, they compare statements in the narrative with information contained in authentic documents, data, and with accepted narratives. If the information contained in the evidence agrees with the information in the narrative, the historians judge that the evidence supports the narrative. If, on the other hand, the two bodies of information are inconsistent with one another, the evidence is judged to be unfavourable.

The practice of hypothesis testing in historiography suggests that a measure of information might be useful in a formal study of knowledge-formation.

Let p, q and r be carriers of information. Sentences, mathematical formulas in suitable contexts, data registers, curves and diagrams are carriers of information, as well as complexes, composed of carriers of information, such as historical narratives, theoretical models etc.

Let P be a probability measure on carriers of information. The amount of information carried by p is defined as follows:

Def. 1. $I(p) = - \log P(p)$.

Given two carriers of information, p and q, the amount of information that p adds to the information that is carried by q is defined as follows:

Def. 2. $I(p/q) = - \log P(p/q)$.

How to interpret the probability measures is a question which will not be discussed in this paper. For out purposes, it si sufficient to note that standard requirements on monadic and binary measures of probability ensure that the monadic and binary measures of amount of information have the following properties:

I. a. $I(p) \geqslant 0$.
  b. $I(p) = 0$, if p is logically true.
  c. $I(p) = \infty$, if p is logically false.
  d. $I(p) \geqslant I(q)$, if p logically implies q.
  e. $I(pq) = I(p) + I(q)$, if p and q are independent,
  f. $I(p/q) \geqslant 0$.
  g. $I(p/q) = 0$, if q logically implies p.
  h. $I(p/q) = \infty$, if p contradicts q.
  i. $I(p/q) = I(p)$, if p and q are independent.
  j. $I(p/q) = I(pq) - I(q)$.

Using the two measures of amount of information, we offer the following definition of a matching relation between carriers of information.

Def. 3. If p contains some information, so that $I(p) \geqslant 0$, then $M(p/q) = \dfrac{I(p) - I(p/q)}{I(p)}$.

The following conditions are satisfied by this measure:

M. a. $M(p/q) \geqslant 1$.
  b. $M(p/q) = 1$, if q logically implies p.
  c. $M(p/q) = 0$, if q and p are independent.
  d. $M(p/q) = -\infty$, if p contradicts q.
  e. If $M(p/q) < 0$, then $M(p'/q) > 0$, if p' is incompatible with p given q.
  f. $M(p/q) = \dfrac{I(q)}{I(p)}$, if p logically implies q.

A formula which will prove useful in our formal theory of knowledge formation is the following:

$$\text{g. } M(p/qr) = M(p/q) + \frac{I(r)}{I(p)}(I - M(r/q)) - \frac{I(r/rq)}{I(p)}. \qquad ^{1)}$$

Interpretation of M(p/q)

a. If $M(p/q) > 0$ then $I(p) - I(p/q)(>0)$ is a measure of the amount of shared information. $M(p/q)$ (if > 0) is thus a measure of the degree to which the information carried by p is covered by the information that is carried by q. In other words, M is a degree of information overlap, if $M < 0$.

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[1] *Mg.* is proved as follows:
$I(p)M(p/qr) = I(p) - I(pqr) + I(qr) = I(p) - I(p/q) + I(pq) - I(q) - I(pqr) + I(qr) = I(p)M(p/q) - I(r/pq) + I(r/q) - I(r) + I(r) = I(p)M(p/q) + I(r)\ (I - M(r/q)) - I(r/pq)$.
Mg. is obtained from this equation, if every term is divided by I(p).

b. If $M(p/q) < 0$, then $M(p'/q)(>0)$ is the degree to which q covers p', where p' is a carrier of information which is incompatible with p, given q. Suppose that p is an hypothesis and q is evidence. If $M(p/q) > 0$ then q covers a part of p. q *agrees* with p. Therefore, q is favourable to p. If $M(p/q) < 0$, then q is favourable to p' which, given q, is incompatible with p. Hence q *disagrees* with p. Therefore, q is unfavourable to p. In view of these considerations, it is plausible to accept M as a measure of evidential support.

Another useful concept is that of degree of truth. The degree of truth, $T(p)$, of a carrier of information p is defined in two steps.

Def. 4. The *truthcore* p^x of an information carrier p, is a carrier of information, containing all the true information in p, and nothing more.

Remark. Is p is an historical narrative, then the truthcore p^x of the narrative is a carrier of information containing all true information in p and nothing more.

Def. 5. The *degree of truth*, $T(p)$, of an information carrier p, is $T(p) = M(p/p^x)$ where p^x is the truthcore of p. It is easy to show that $M(p/p^x) = \frac{I(p^x)}{I(p)}$.

We will now make use of this formal apparatus to account for knowledge formation of orders 1 and 2.

Knowledge formation of order 1. — A new hypothesis h is likely to contain part of the input information. In so far as h is partly covered by the authentic input information, h has some evidential support from the beginning. It may therefore happen that h is, not without justification, accepted as knowledge without having been tested.

If h is put to a test, however, h is confronted with previous knowledge, k, and with data d.

We will use formula Mg in an account of hypothesis testing.

According to this formula:

$$M(h/d'k) = M(h/k) + \frac{I(d')}{I(h)} (I - M(d',k)) - \frac{I(d',/hk)}{I(h)} \quad \text{where}$$

d' stands for data, which may not yet have been assembled, i.e. d' are virtual data.

We are interested in conditions which would ensure that the degree of covering is large. The formula shows that $M(h/d'k)$ is large under the following four conditions:

a) $M(h/k)$ is large. This will be the case if k is selected from previous knowledge in such a way that the degree to which h is covered by k is large.

b) $I(d'/hk) = 0$. One should look for data which are entailed by the hypothesis h in conjunction with k.

c) d' contains a large amount of information. The more precise the data are, the more information do they carry.

d) $M(d'/k)$ is small. $M(d'/k) < 0$ holds only if d' is inconsistent with k, the latter being by hypothesis a piece of authentic information. To require that $M(d'/k) < 0$ is therefore not compatible with a reasonable wish that d' should be consistent with veridical data. « $M(d'/k)$ is small » therefore means that d' to a large extent is independent of k.

The next step is to obtain data d of the same kind as d' and to compare them vith d'.

If d agrees with d', then $M(h/kd)$ is also large. If d disagrees with d', then $M(h/kd)$ is < 0. Hence d in conjunction with k is unfavourable evidence of h[1].

We have now reached the verdict point (see fig. 3 c.) which is also a decision point.

Case 1. The verdict is negative. If the hypothesis h is rejected, the evidence which has been used in order to test h may not be used any more. Moreover, there is little guarantee that a new hypothesis h' will fare better than h. On the other hand, if h is changed into another hypothesis h', which is supported by the evidence which supports h and also by the evidence which is unfavourable to h, it is likely that h' has a higher degree of truth than that of h.

There is to be sure no warrant that the evidence e is true, but as evidence is authentic information, that part of the hypothesis h which is covered by e is likely to have a high degree of truth. As h' is covered by more evidence than h, it follows that a larger part of h' is authentic than is the case with h.

Furthermore as h has been undermined by some evidence which supports h', it is likely that the amount of false information carried by h is larger than that which is carried by h'. It is therefore likely that the degree of truth of h' is at least as large as that of h. We conclude that a decision to modify an hypothesis in the face of negative evidence will lead to an increase of the degree of truth.

Case 2. The verdict on h is positive. Either h is promoted to the status of knowledge (authorized information) or h is submitted to continued testing.

It is plausible to assume that a positive correlation holds between degree of truth and degree of covering in the case of high graded evidence.

If this is so, authorization of an hypothesis h, that has been supported by favourable evidence e, is more reasonable the more h is covered by e and the more credible e is.

---

[1] In order to make more precise in what sense there would be agreement or disagreement between d and d', we note to begin with that $P(d'/hk) \leqq P(dyd'/hk) = P(d/hk) + P(d'/hk) - P(dd'/hk)$ according to the rules of probability calculus. By hypothesis $I(d'/hk) = 0$. Hence $P(d'/hk) = 1$. We conclude that $P(d/hk) = P(dd'/hk)$ so that $I(d/hk) = I(dd'/hk)$. d agrees with d' if d' entails d, in which case $I(dd'/hk) = I(d'/hk) = 0$. It follows that $I(d/hk) = 0$. d disagrees with d', given h and k if dd' cannot be true given h and k. In such a case $P(dd'/hk) = 0$ so that $I(dd'/hk) = I(d/hk) = \infty$. Then $M(h/dk) = -\infty$.

If the procedure of testing ends too early however, there is a risk that an hypothesis with a low degree of truth will be accepted as knowledge. If it ends very late, the rate of growth of knowledge is slow. If a low graded hypothesis is accepted as knowledge it is likely that sooner or later it will clash with other knowledge. If this should happen the hypothesis will be submitted to renewed testing and the likelihood is that it will be replaced by a better hypothesis. It is in any case only provisionally that hypothesis testing is terminated, when an hypothesis has been promoted to the status of knowledge.

The thesis that testing is non-terminable is correlated to the epistemological thesis that knowledge is improvable (can be improved), an antithesis of the older view that knowledge is provable (can be proved, in fact has been proved).

Knowledge formation of order 2. — From the information theoretical point of view adopted in this section, why-questions are problems of how to cover new knowledge. If old knowledge does not provide sufficient covering, star-hypotheses are invented which will supply further covering.

In our account of hypothesis formation, we pointed out that formal conditions are imposed on hypotheses which are offered as tentative answers to questions.

A star-hypothesis should satisfy the condition $M(k/h^x K) = 1$ where K is a selection of old knowledge and k is the new knowledge which has given rise to a problem of order 2.

Before testing, h^x is credible to the extent that h^x is covered by K an k. It is favourable that the degree of covering, $M(h^x/Kk)$, is fairly large. According to M g, it holds that

$$M(h^x/Kk) = M(h^x/K) + \frac{I(k)}{I(h^x)}\,(I - M(k/K)) - \frac{I(h^x/Kk)}{I(h^x)}.$$

I. $M(k/K)$ should be small. This is the case if h^x completes a very incomplete covering.

II. $M(h^x/K)$ should be large. In other words, the star-hypothesis should not be too far-fetched. It should not carry very much more information than K.

III. $\frac{I(k)}{I(h^x)}$ should not be very small.

IV. Moreover, only a small part of the information that is carried by h^x should be additional to k and K, so that $\frac{I(h^x/Kk)}{I(h^x)}$ is small.

In order to test h^x, h^x and a selection of previous knowledge K is used as input information to an hypothesis generator. The output is a selection of the input information. (Deductions from premisses are selections of information). Using M g we have

$$M(h^x/Kh) = M(h^x/K) + \frac{I(h)}{I(h^x)} (I - M(h/K)) - \frac{I(h/Kh^x)}{I(h)}$$

This formula provides a justification of the practise of deducing virtual evidence h from h^x and K. Then the third term on the right side vanishes.

The first two terms on the right side are essentially non-negative. We conclude that the daughter hypothesis h together with K is favourable evidence for h^x, if h becomes accepted as knowledge after testing.

As $I(h/Kh^x) = 0$ it holds that $M(h/Kh^x) = 1$. Therefore, if h is promoted to the status of knowledge, what is asserted by h is explained by means of K and h^x. The explanatory pattern is virtual until h^x itself becomes knowledge, whereby it becomes a genuine explanatory pattern.

Theoretical work, Part 2. — Solutions of problems of order 2 are families of explanatory patterns. They give rise to a new order of problems which we will refer to as problems of order 3[1]. Their solutions are systems of knowledge in which certain components are axioms or postulates while other components are theorems. We will refer to the complex of axioms as a center of a knowledge system and accordingly to the axioms as central components of the system.

An essential property of a centric system of knowledge, K S, is that it is completely covered by its center C. ($M(KS/C) = 1$).

Not all knowledge systems are centric ones however. An explanatory historical narrative for example does not have a center but a common feature of all knowledge systems is the presence of information overlap within them.

Star-hypotheses in families of explanatory patterns, Newton's law of gravitation for example, occupy central positions in a knowledge system. Other central components may have their origin in a world-picture. They enter the system after transformations, including translations into a technical language. If centric systems of knowledge already have been created, it may also happen that central components in them are grafted onto new ones under development. It is no accident, therefore, that classical mechanics contains many knowledge systems with homologous central components. Systems of knowledge about machines contain principles of motion which are homologous to the principles of motion in Newton's theory about the planetary system.

The principles of motion in the mechanics of rigid bodies are homologous to those in particle mechanics in the sense that the former are obtained by applying the latter ones to models composed of a large number of mass-points at constant distances from each other. Such configurations of mass-points are rigid bodies mapping solid bodies. The principles of motion in a knowledge system within classical mechanics are special causal principles. Some of them specify general properties of causal relations between components of a system. Others connect causal relations to changes of state. Newton's third principle and the law of force parallellograms are principles of the first kind, whereas Newton's second principle, stating that change of momentum is equal to a force acting on a part of a system, belongs to the second kind of causal principles.

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[1] Such problems would ordinarily be called « theoretical research programs ».

The causal principles in knowledge systems belonging to classical mechanics are combined with chrono-geometrical assumptions which also are ingredients of centers of knowledge systems. Newton's third law of motion for instance is not valid unless simultaneity is an absolute relation.

Classical mechanics, therefore, is a system of knowledge systems, each one of which contains central components mapping space, time, matter and causality in similar ways. Relativistic quantum mechanics is a system of knowledge systems in which space, time, matter and causality are mirrored quite differently from classical mechanics.

As further discussions of knowledge formation of the third order would require extensive case studies in order to be profitable, I refrain from proceeding beyond this point. It would be of special interest in such case studies to examine how world-pictures affect and are affected by the development of systems of knowledge systems. A world-picture which has had beneficial effects on the development of one branch of science becomes entrenched in the minds of scientists to such an extent that they are unwilling to abandon it even when new knowledge clashes with it.

Positivistic philosophies, rejecting all world-pictures, are useful in such situations, but only in them. Positivistic attitudes prevent scientists from building up world-pictures which will make it possible for them to generate systems of knowledge systems and thus to enhance the progress of science.

Relations between empirical and theoretical work. — Knowledge formations of orders 0 and 1 is empirical work in a strict sense, whereas knowledge formation of higher order is the concern of the theoreticians.

Theoreticians receive information about a territory T from laboratory physicists in the form of knowledge of order 1. They build this information into explanatory patterns and into systems of knowledge which are models mapping T. In the process of knowledge formation of orders 2 and 3, hypotheses are grnerated which are delivered to the experimentalists. They design experimental equipment and procedures in order to test the hypotheses. The experimentalists also receive forms of description which are imposed as conditions on their hypothesis formations.

Both theoretical and experimental physicists make use of auxiliary disciplines in order to solve their problems. Those disciplines are workshops in which the instruments in the (K, P, I) - sequences are made.

Laboratory physicists use statistics in order to correct random errors in data-generation and in order to test hypotheses. They use older theories in order to build data generating systems, to correct systematic errors and to interpret data as information about.

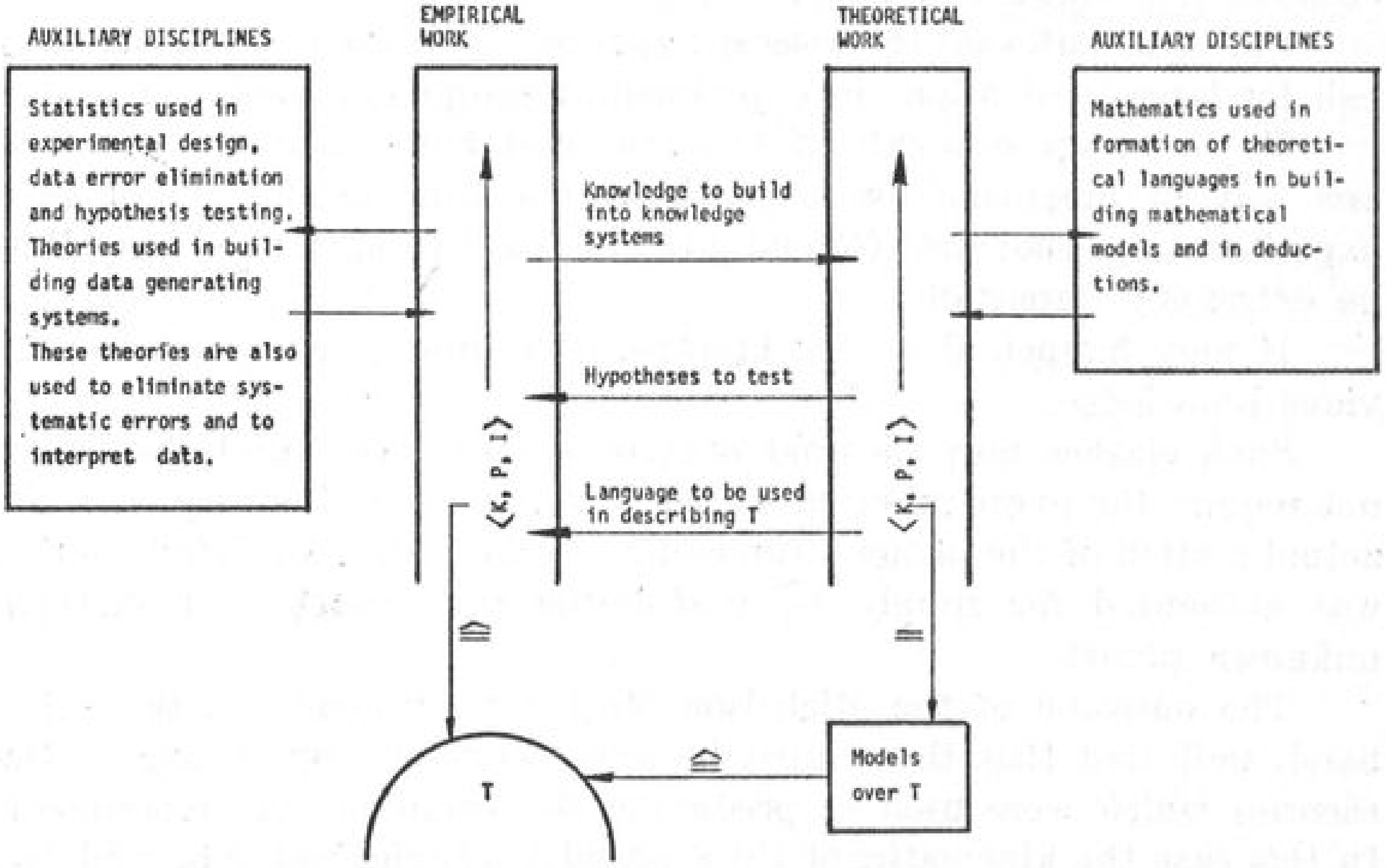
AUXILIARY DISCIPLINES
EMPIRICAL WORK
THEORETICAL WORK
AUXILIARY DISCIPLINES
Statistics used in experimental design, data error elimination and hypothesis testing. Theories used in building data generating systems. These theories are also used to eliminate systematic errors and to interpret data.
Knowledge to build into knowledge systems
Mathematics used in formation of theoretical languages in building mathematical models and in deductions.
Hypotheses to test
⟨K, P, I⟩
⟨K, P, I⟩
Language to be used in describing T
≙
=
≙
T
Models over T

Fig. 4

Theoreticians use mathematics in constructing technical languages, in building models over T, and in drawing conclusions from their own hypotheses in conjunction with established knowledge.

The relations between empirical and theoretical work as well as the relations between these kinds of work and the corresponding auxiliary disciplines, are shown in fig. 4.

It may occur that experimental physicists produce knowledge which the theoreticians are unable to systematize. Work in spectroscopy during the 19th and the beginning of the 20th century generated a vast body of knowledge about spectra which theoreticians were unable to account for. It may, on the other hand, also happen that theoreticians frame hypotheses which experimentalists are unable to test for technical reasons. Einstein's unified field theories contain such hypotheses. There are thus two stores of hypotheses, one store filled by knowledge of the first order which has not been assimilated into theories, and one store of hypotheses framed by theoreticians which have not as yet been tested. These two stores are surrounded by problemfields. The problemfield around the first store is composed of problems of orders 2 and 3, whereas problems of orders 0 and 1 are generated by stores of the second kind.

The two kinds of stores are residuals. If a residual of knowledge of order 1 becomes very extensive, entirely new types of theory developments may arise which may clash with veteran theories at least at an early stage.

The older quantum theories about spectra are inconsistent with classical mechanics and electrodynamics.

Stores of untested theoretical hypotheses, on the other hand, may call for large innovations in experimental equipments and techniques.

To build up residuals of unassimilated knowledge of order 1 is one way of preparing for large theoretical innovations. Unplanned experimental discoveries (serendipity) fill such residuals and may thus be extremely important.

It may happen that new knowledge of order 1 clashes with previous knowledge.

Such clashes may be mild ones or severe ones. Mild clashes do not require the overhauling of earlier theories. The discovery that the actual motion of the planet Uranus differed from the calculated motion was accounted for simply by postulating the existence of hitherto unknown planet.

The outcome of the Michelson Morley experiment, on the other hand, indicated that there must be something seriously wrong in the theories which were used in predicting the result of the experiment. In this case the kinematics of the classical mechanics had to be replaced by a new kinematics.

Clashes or anomalies, like residuals, can thus be regarded as potential sources of great innovations in science, giving rise to new lines of research.

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LE TEORIE FISICHE ED IL CRITERIO DI NORMALITÀ

L'analisi metodologica qui stesa segue fedelmente lo sviluppo storico delle teorie fisiche, prendendo in particolare esame lo status *dei modelli (operativo-concreti), che costituiscono la struttura interpretativa in termini di operazioni fisiche. Un tale modello (fisico) è detto* normalizzato *se le sue diverse, singole, concretizzazioni (effettuate da diversi operatori) sono coordinabili tra di loro mediante il criterio di invarianza caratterizzante e la classe di osservatori-operatori interessati e il sistema relazionale così interpretato. Le teorie si possono allora distinguere in tre tipi, senza voler porre con ciò una gerarchia: a) con modelli operativo-concreti* diretti, *b) con modelli in parte* indiretti, *c) con modelli diretti* mediati. *Il criterio di normalità, infine, sembra quello più soddisfacente per la scelta tra teorie rivali.*

1. - Una definizione di teoria scientifica, in particolare fisica, che si è ormai alquanto affermata, è quella che la vede come un sistema relazionale (sviluppato deduttivamente partendo da un appropriato gruppo di ipotesi) *interpretato* in termini usati, direttamente o indirettamente, nelle descrizioni di particolari componenti, o momenti, dell'esperienza; termini dunque che svolgono un ben definito ruolo nel *controllo* del nostro operare tra le concrete cose e nel susseguirsi degli eventi.

Il *sistema formale* di relazioni, sempre espresse, nel caso delle scienze cosiddette esatte, o esprimibili in forma matematica, è caratterizzato da una sua interna coerenza, anche se è ben lungi dal presentare il requisito logico della completezza; carattere questo che d'altronde, chiudendo il sistema, lo renderebbe non suscettibile di autentici sviluppi, tali da poter comprendere nuove componenti dell'esperienza.

Il *controllo* dianzi invocato è inteso ad assicurare, ad osservatori operanti nel rispetto di definite condizioni ambientali ed operative, una soddisfacente *riproducibilità* dei caratteri intrinseci e del susseguirsi delle componenti dell'esperienza, specialmente nei riguardi dei fini propostisi. L'aggettivo « soddisfacente » testé usato è legato appunto al raggiungimento di tali fini; ed il termine « riproducibilità » va qui considerato definibile soltanto con mezzi ostensivi, così che lo scrivente deve confidare nella collaborazione del lettore.

L'*interpretazione* prima invocata, di una espressione formale del sistema relazionale, consiste nel porre una corrispondenza tra i segni, che compaiono nell'espressione stessa, ed alcuni termini rilevanti usati (in base ad esplicito accordo tra tutti gli interessati) nella descrizione dell'esperienza, ai quali è assegnato - con particolari convenzioni che si dicono « metriche » - lo *status* di quantità nel senso matematico; la rilevanza testè chiamata in causa essendo dettata dalla *ripetibilità* dell'operare in vista dei fini propostisi dagli osservatori interessati. E si nota che per il termine « ripetibilità » vale quanto detto per l'altro « riproducibilità ».

In logica le interpretazioni soddisfacenti una forma si dicono *modelli*; questi danno alla forma stessa significato entro un certo campo dell'agire e del riflettere (sull'azione). Le interpretazioni, che qui interessano, danno significato nel campo dell'operare tra le concrete cose; esse dunque originano dei *modelli* (del sistema formale) che possiamo dire *operativo-concreti*.

I modelli messi a punto dai fisici, e loro collaboratori, costituiscono una *struttura funzionale*, caratterizzata cioè da un ordine orientato verso un complesso ben precisato di fini; ordine che ovviamente trova corrispondenza nel sistema formale. Questo dunque contiene espressioni che, interpretate come sommariamente detto, si traducono in criteri di ordinamento della nostra esperienza in un definito campo dell'operare; anche se essi siano formulati in un linguaggio che non fa esplicito riferimento alle componenti della nostra attività concreta (V. in seguito).

In breve: una teoria scientifica (in particolare fisica) è costituita da un sistema normativo di relazioni interpretate da una struttura funzionale di modelli operativo-concreti.

Dopo le suddette precisazioni appariranno del tutto accettabili due definizioni già presentate in questa Rivista[1]. La prima: una espressione formale, un enunciato ipotetico, hanno *significato empirico* se, e soltanto se, tutti i membri di una definita classe di osservatori hanno costruito, o sono in condizioni di farlo volendo, almeno un modello operativo-concreto. I caratteri definenti la suddetta classe di osservatori vanno ovviamente precisati in modo non equivoco; ma, va subito notato, tale precisazione è stata messa a punto soltanto a sviluppo inoltrato delle teorie per noi più interessanti; e ciò vedremo particolarmente nel seguito.

La seconda definizione infine che qui va ricordata vuole che il *contenuto empirico* di un sistema relazionale sia dato dall'insieme finito dei modelli così costruiti, e dichiarati significanti da tutti gli interessati.

2. - Le proposizioni costitutive del sistema interpretato - dunque della teoria fisica - sono in genere formulate in un linguaggio che non fa riferimento esplicito alle componenti, ovvero ai momenti, della nostra attività concreta; pur invocando, alle volte esplicitamente, altre implicitamente, enti esterni a noi - le « cose », gli « oggetti » - che fanno da supporto appunto a quelle componenti. Alcune proposizioni però contengono parole e forme verbali che direttamente si riferiscono alla nostra azione, e sono le cosiddette *definizioni operative*; esse introducono i termini caratteristici del discorso dei fisici precisando appunto le moda-

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[1] D. Faggiani, « *Scientia* », Luglio-Agosto 1969, vol. CIV, N. DCLXXXVII-DCLXXXVIII

lità con cui si deve operare, su definiti concreti corpi ed in precisate condizioni ambientali, al fine di costruire un membro di una classe definita - *in intensione*, si dice - proprio da quelle serie ordinate di operazioni concrete: la classe delle lunghezze, delle masse, eccetera. Sono infine queste definizioni operative che permettono di porre le corrispondenze tra i termini puramente relazionali e formali (con le loro eventuali combinazioni) e quelli osservazionali, e cioè col piano dell'esperienza; svolgendo dunque la fondamentale, insostituibile, funzione nel processo di interpretazione assegnante significato fisico.

I sistemi di corpi concreti, definiti - in una prima fase del processo - soltanto sul piano percettuale-pragmatico, e sui quali vengono effettuate le serie operative che fanno da *indice* per la corrispondente classe di quantità, quali quelle prima precisate, possono dirsi *portatori di quantità* (fisiche) appunto; così: un regolo porta lunghezza, un corpo porta massa e peso, un evento porta durata, eccetera. Con ciò quei corpi (ed i loro complessi) acquistano definizione in termini fondamentali del discorso dei fisici (definizione fisica): il « corpo » del comune linguaggio diventa dunque un portatore di quantità fisicamente definite (è ora un oggetto scientifico).

Un tale corpo, in una successiva fase della teoria, viene concepito come un *aggregato macroscopico* di particelle omogenee denominate molecole ed atomi; e ciò al fine di precisare le condizioni di riproducibilità e controllo di quelle esperienze alle quali si fa riferimento col termine « reazioni chimiche ». Detti costituenti del macro-aggregato vengono a loro volta concepiti come *sub-aggregati* di particelle molto più piccole, dette « elementari », quali: elettrone, protone, neutrone....., mesone, eccetera. A tutte queste « particelle » vengono assegnate, entro certi limiti, quantità analoghe a quelle caratterizzanti operativamente i concreti corpi del precedente capoverso; ma a rigore esse vanno considerate *pseudo-portatori* (di quantità fisiche), in quanto le corrispondenti operazioni-indice sono effettuate soltanto su collettività macroscopiche ad esse connesse, quali le macchie o i pennelli di luce emessa per eccitazione elettronica o fotonica, le « tracce » nella camera di Wilson ovvero nelle emulsioni fotografiche, eccetera.

Alcune teorie fisiche usano termini riferentisi soltanto a portatori, come prima intesi; altre chiamano in causa anche termini invocanti pseudo-portatori. I modelli delle prime sono dunque *direttamente* operativo-concreti; quelli delle seconde lo sono *indirettamente* (tramite le corrispondenti collettività); ecco così spiegato il ricorso ai due suddetti avverbi nel primo capoverso del precedente paragrafo. Tutti i termini che compaiono, come caratteristici, nelle prime teorie sono presenti anche nelle seconde; ma non tutti quelli caratterizzanti le seconde sono necessari per la significanza delle prime. Queste dunque svolgono una funzione propedeutica per tutto il discorso teorico, e possono pertanto venir dette *fondamentali*.

3. - La nostra analisi considera dapprima le teorie fondamentali, nel senso detto, e quindi in particolare la meccanica e l'elettromagnetismo con i loro sviluppi relativistici. Questi capitoli concorrono a formare un coerente sistema relazionale, che contiene le proposizioni primitive che hanno significato per tutto il discorso specifico sviluppato dai fisici, e lo caratterizzano; proposizioni infine che permettono di precisare, in tutte le articolazioni, il ruolo dei concetti fondamentali, quali: spazio e tempo (intesi fisicamente), massa, energia, eccetera.

I risultati sperimentali quantitativi di più osservatori, operanti nelle linee della *meccanica newtoniana*, sono tra loro paragonati, e quindi coordinati, tramite il gruppo di trasformazioni, ben noto, detto comunemente « di Galileo ». Questo funge dunque qui da organo fondamentale di coordinazione (mutua comparazione).

Nelle *esperienze elettromagnetiche* la comparazione dei risultati di più osservatori è invece fatta soddisfacentemente rispetto all'altro gruppo detto « di Lorentz », anche esso ben noto.

Si osservi ora che il gruppo di Galileo rappresenta il criterio rispetto al quale le equazioni della dinamica newtoniana risultano invarianti, e che il gruppo di Lorentz esprime il criterio d'invarianza delle equazioni di Maxwell. Ebbene la precisazione delle espressioni di trasformazione (in particolare delle coordinate) rispetto alle quali le precedenti forme sono invarianti, è questione di pura deduzione; e ciò perchè nelle equazioni della dinamica è implicito il gruppo di Galileo, e così nelle relazioni dell'elettromagnetismo è contenuto il gruppo di Lorentz.

Adottando la terminologia dei precedenti paragrafi si può dire in breve che i modelli meccanici da una parte, e quelli elettromagnetici dall'altra, sono coordinabili mutuamente in base al gruppo di invarianza *caratterizzante il corrispondente sistema relazionale*. Orbene, ritenere soddisfacente (come si è fatto per molto tempo) questa situazione appare oggi atteggiamento piuttosto acritico, perchè equivalente ad invocare significato senza precisare gli interessati all'interpretazione, addetti cioè alla costruzione dei modelli operativo-concreti.

I criteri di coordinazione, per la meccanica e per l'elettromagnetismo, sono dunque tra loro diversi; ecco perchè sono apparse delle contraddizioni quando i fisici hanno voluto costruire dei modelli dinamico-elettromagnetici (interpretanti cioè entrambi i sistemi relazionali suddetti) quali, ad esempio, quelli corrispondenti alla esperienza di Kauffmann e Bucherer, ovvero ad altri esperimenti analoghi (compreso quello ben famoso di Michelson e Morley).

La questione, come è ben noto, ha interessato per decenni la critica scientifica, da E. Mach ad A. Einstein[1]. Ebbene si può riassumere, col nostro linguaggio, detta analisi critica dicendo che con essa si è posto in luce la necessità di definire - in intensione - la classe di osservatori pienamente responsabili, per i quali cioè quella costruzione e comparazione (di modelli) potesse avere pieno significato; con la conclusione che il criterio di invarianza, su cui possono articolarsi soddisfacentemente quei paragoni, è quello *caratterizzante* - non tanto le forme di questo o quel sistema relazionale - quanto *la suddetta classe di osservatori*, a giusto titolo interessati.

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[1] Il nesso tra principio d'inerzia e sistemi di riferimento spazio-temporale fu posto chiaramente per primo da L. Lange nel 1885. Si veda M. JAMMER, *Concepts of space*, p. 138, Harvard Un. Press 1954.

La generalizzazione del principio di relatività, dal senso galileiano a quello einsteiniano, è appunto equivalente alla definizione di detta classe, che è notoriamente quella degli osservatori « inerziali », e cioè in moto relativo rettilineo uniforme.

La precisazione del nuovo criterio d'invarianza non è però questione puramente formale, sibbene tipica della scienza empirica; essa invero presuppone che i diversi osservatori precisino *operativamente* il loro moto relativo. Ora ciò può farsi *soltanto* se essi si scambiano dei segnali (luce o radio) dandone una *comune interpretazione*. Ebbene è proprio qui il nocciolo della questione contenente la soluzione; analizzatene infatti le conseguenze si conclude che il criterio di invarianza, caratterizzante la classe degli osservatori inerziali, è dato dal gruppo di Lorentz[1].

In conclusione: tanto le equazioni dell'elettromagnetismo, quanto quelle della meccanica, *debbono* venir subordinate - per coerenza metodologica - al gruppo di Lorentz. Questa conclusione ha rappresentato, come è ben noto, un momento rivoluzionario per la teoria fisica fondamentale; da esso invero è nata la Relatività speciale.

La prima definizione ricordata nel paragrafo 1) può essere completata dicendo che un modello operativo-concreto è *normalizzato* se esso è coordinabile in base al criterio d'invarianza *caratterizzante la classe degli osservatori interessati*. Precisiamo che ciò comporta che le diverse concretizzazioni del modello, messe a punto dai diversi membri della classe, siano caratterizzate da valori numerici, per le quantità significative, risultanti nel rapporto precisato dal gruppo di trasformazione definente quel criterio. Si può allora affermare che i modelli meccanici classici non erano normalizzati, e lo sono diventati nella luce in cui li ha posti la relatività speciale.

L'esame delle teorie fondamentali va chiuso con un cenno alla parte del discorso dei fisici che è articolato sul principio di evoluzione. Ecco: i modelli concreti della termodinamica sono normalizzati dalla posizione di invarianza, sempre rispetto al gruppo di Lorentz, della quantità caratterizzante la evoluzione, l'entropia. Posizione questa coerente, si noti, con la definizione operativa che introduce la quantità qui significante nei riguardi dell'interpretazione, e cioè la temperatura.

4. - Consideriamo ora brevemente le altre teorie fisiche. Per quelle che fanno riferimento a pseudo-portatori, nel senso visto nel secondo

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[1] Chi voglia conoscere i particolari dell'argomentazione veda la nota dello scrivente già citata.

paragrafo, basterà notare che il loro contenuto empirico è subordinato a quello delle teorie, dianzi considerate, che introducono termini fisici primitivi comuni; ed aggiungere che gli eventuali termini nuovi vanno interpretati - indirettamente nel senso precisato a suo luogo - da modelli operativo-concreti anch'essi normalizzati per una soddisfacente sistematizzazione. A questo riguardo è da notare che le teorie quantistiche, articolate sulla relazione fondamentale di Planck, rientrano nella suddetta regola perchè l'azione è invariante appunto rispetto al gruppo di Lorentz.

È stata molto discussa sul piano logico l'opportunità di ricorrere a termini invocanti pseudo-portatori, ed è stata messa in luce la possibilità di esporre completamente le teorie, che li invocano, con termini soltanto osservazionali e cioè, nel nostro linguaggio, con una struttura di soli modelli operativo-concreti diretti[1]; ma il discorso diventa così molto più complesso, essendo articolato in un ben maggior numero di proposizioni. A ciò è da aggiungere che quei termini (atomo, elettrone, mesone, eccetera) hanno svolto una funzione psicologica assai interessante, di polarizzazione dell'attenzione ed inventiva dei ricercatori, senza la quale sarebbe stato — come suggerisce la storia della scienza — ben più lento il processo di formazione di nuovi capitoli.

Per completare questa analisi infine è necessario considerare la *relatività generale*. Essa è costituita da un sistema relazionale che può essere presentato in astratto, e cioè in modo del tutto indipendente dall'esperienza, considerando un ordine distributivo di grande generalità e fornente il quadro in cui dovrebbero essere inserite le componenti descrittive dell'esperienza. Ma per porre le corrispondenze interpretative, e quindi significanti, tra tali termini relazionali tanto generali[2] e quelli descrittivi, si deve ricorrere ad una sorta di riduzione alla meccanica classica ed alla relatività speciale, sul che è bene dare qualche precisazione.

Un modo espressivo ed abbastanza semplice di esporre la questione è quello di riferirsi alla « traiettoria » di un corpo, che sarà poi un pianeta, ed in particolare Mercurio. Ebbene l'equazione di tale traiettoria può scriversi, secondo la relatività generale, così[3]:

$$\left(\frac{du_o}{d\varphi_o}\right)^2 + u_o^2 = k - \frac{I}{h^2}\, u_o + I u_o^3, \qquad (1)$$

con k, h ed I costanti per ora del tutto indeterminate, ed u_o, φ_o « coordinate » non ancora interpretate, si noti, in base ad operazioni fisiche.

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[1] Si veda: C. G. HEMPEL, *La formazione dei concetti e delle teorie nella scienza empirica*, Milano 1961, p. 151 e seg.

[2] « Senza significato fisico »: sono parole dello stesso Einstein. Si veda a pag. 122, l'opera: *50 anni di Relatività*, Ed. Universitaria, Firenze, 1955.

[3] G. Y. RAINICH, *Mathematics for relativity*, N.Y. London 1950, p. 158.

L'equazione classica dell'orbita di un pianeta (in particolare ellittica) si scrive nelle solite coordinate piane polari (fisicamente interpretate):

$$\left(\frac{du}{d\varphi}\right)^2 + u^2 = \frac{K}{H^2} + \frac{2M}{H^2}\,u, \tag{2}$$

dove M è la cosiddetta massa ridotta del Sole, H l'integrale delle aree e K quello dell'energia; quantità che trovano anch'esse precisazione diretta ed esauriente in operazioni fisiche ben definite.

La (2) offre la possibilità di assegnare significato alla (1) con alcune posizioni, che interessa qui dichiarare esplicitamente (sul che generalmente si sorvola). Eccole:

A) Si identificano le « coordinate » generalizzate u_o e φ_o con quelle operative u e φ.

B) Dalla condizione che la relatività generale comprenda quella speciale come caso limite, e dal fatto che la velocità del pianeta è sempre molto piccola rispetto a quella di propagazione c della luce, si ottiene come soddisfacente approssimazione la relazione:

$$h^2 = -\left(\frac{H}{c}\right)^2.$$

C) Con ciò la (1) assume forma simile alla (2) con un termine in più, quello in u^3. Paragonando allora i termini corrispondenti si identificano le costanti; e risultano così precisate operativamente k ed I.

In conclusione la (1) è fisicamente *semantizzata* nella:

$$\left(\frac{du}{d\varphi}\right)^2 + u^2 = \frac{K}{H^2} + \frac{2M}{H^2}\,u + \frac{2M}{c^2}\,u^3;$$

e qui va notato che lo *spostamento del perielio* di Mercurio trova giustificazione soddisfacente nel termine in u^3.

Orbene se diciamo « mediazione » la funzione così svolta dalla meccanica classica e dalla relatività speciale, i modelli operativo-concreti della relatività generale debbono dirsi diretti ma *mediati*.

L'analisi sviluppata porta dunque a suddividere le teorie fisiche normalizzate in tre tipi, dipendentemente dallo *status* dei modelli operativo-concreti costituenti la struttura interpretante le forme del sistema relazionale:

— teorie con modelli diretti (le fondamentali);

— teorie con modelli almeno in parte indiretti (sono in genere statistiche);

— teorie con modelli diretti mediati (relatività generale);
e si noti infine che con ciò non si vuole dettare una gerarchia, ma soltanto mettere in luce una distinzione.

5. - L'analisi qui sviluppata ha il pregio di rispecchiare piuttosto fedelmente lo sviluppo storico delle teorie fondamentali; essa inoltre, nel rispetto della tesi che vede nelle proposizioni scientifiche criteri di ordinamento dell'esperienza, porta a sostituire alla semplicità, ovvero all'economia - quali caratteri discriminatori tra teorie concorrenti - quella che può dirsi, date le definizioni prima accettate, *normalità*: riducibilità ad una definita norma, significativa *sia per la classe degli osservatori interessati, sia per le forme ordinatrici.*

I fisici in genere - anche se non accolgono tutti la suddetta tesi - ritengono che con la relatività speciale i due primi grandi capitoli della fisica teorica, meccanica ed elettromagnetismo, abbiano raggiunto una sistematizzazione soddisfacente. I caratteri di coerenza metodologica qui messi in luce fanno inoltre pensare che le nuove forme, che nel futuro costituiranno i cardini della coordinazione fisica, difficilmente potranno prescindere dalla suddetta sistematizzazione che, tuttavia, non può essere vista come la definitiva, ultima, parola del ricercatore.

Ammesso che il contenuto empirico di una teoria sia dato dall'insieme dei modelli operativo-concreti, si presenta la questione del maggiore o minore contenuto di teorie rivali quali, ad esempio, la dinamica newtoniana e quella einsteiniana. Generalmente si dice che la seconda ha maggior contenuto perchè dà risposta a domande, formulate in termini osservazionali, alle quali la prima è sorda; ed in base a ciò, e soltanto a ciò, alcuni considerano la seconda preferibile, facendo così una questione di sola quantità. Altri, forse oggi i più, tengono anche presente che la seconda risolve alcune contraddizioni presentate dalla prima, facendo così una questione piuttosto di qualità. Orbene sembra a chi scrive che il riferimento al criterio di normalità dia maggior peso proprio a quest'ultimo ordine di considerazioni, indubbiamente meglio caratterizzante il metodo scientifico, segnandone, come già notato, quella che può dirsi coerenza; la quale non può qui ridursi al solo aspetto formale. Si noti d'altra parte che il nostro criterio è stato operante nello sviluppo della scienza fisica in questi ultimi settanta anni, anche se di ciò non si è avuto consapevolezza nei termini introdotti in questa nota.

Negli ultimi decenni sono stati sviluppati molti nuovi tentativi per giustificare l'induzione sul piano della logica, alcuni dei quali hanno portato a teorie molto elaborate. Queste però si sono dimostrate anch'esse non idonee a comprendere le generalizzazioni, e tanto meno le relazioni funzionali, quali quelle su cui si articola una scienza empirica sistematizzata. Da molti Autori si è quindi rinunziato ad una tale giustificazione ripiegando su un programma meno ambizioso; precisare un criterio di scelta tra ipotesi o teorie rivali; ed alcuni hanno individuato nella semplicità tale criterio, che tuttavia è rimasto alquanto vago. S. F. Barker[1] piuttosto recentemente ne ha dato una precisa definizione, che qui interessa ricordare, suggerita dal concetto di misura logica di Kemeny; una teoria è tanto più semplice quanto minore è la sua misura logica, questa essendo data dal numero di modelli *logicamente* possibili del sistema corrispondente. Il criterio di Barker vuole che, tra due teorie di pari contenuto empirico (nel nostro senso) sia scelta quella di minore misura logica (più semplice). Ma si domanda: è stato questo criterio mai operante nello sviluppo della scienza? La risposta sembra a chi scrive del tutto negativa; invero, quando si sono incontrate due (o più) teorie rivali, esse non hanno mai presentato eguale contenuto empirico; con che il criterio in discussione non ha trovato alcuna possibilità di applicazione; e ciò in conseguenza della stessa sua formulazione.

Merita infine un breve esame il caso di teorie rivali presentanti diverso contenuto empirico. Sia n il numero di modelli operativo-concreti effettivamente costruiti ed N quello dei corrispondenti modelli logicamente possibili; ebbene diciamo *grado di saturazione* il rapporto $\frac{n}{N}$. Si presenta la questione: quale teoria dovremo scegliere; quella con maggiore n ovvero quella con più elevato grado di saturazione la quale, si noti, potrebbe risultare caratterizzata da n minore? Nel caso della prima scelta, ben più significativa per lo sviluppo effettivo della scienza, il criterio di semplicità non verrebbe nemmeno chiamato in causa.

Una analoga osservazione potrebbe farsi anche al riguardo del nostro criterio di normalità; domandare cioè: tra due teorie, una con minore contenuto empirico ma costituito da n modelli normalizzati, l'altra da n' modelli non normalizzati, con $n' > n$, quale scegliere? Chi scrive nota che il caso non si è presentato finora; tuttavia dà ugualmente una risposta: la prima sul piano sistematico (interessante la scienza), anche se in pratica (tecnica) possa essere preferita la seconda perchè più utile.

In conclusione chi scrive ritiene che soltanto la notata doppia significanza del criterio fondamentale di invarianza (gruppo di Lorentz) assegni, al discorso scientifico ed in particolare fisico, uno *status* del tutto soddisfacente: affianca invero alla pura coerenza formale, del sistema ipotetico-deduttivo, una inter-connettività, tra il sistema stesso e la sua interpretazione sperimentale, più profonda di quella data dal mero paragone finale tra le relazioni particolari, che chiudono quel sistema, e le corrispondenti descrizioni di situazioni concrete opportunamente scelte. Uno stesso criterio invero è vincolante, sia per le proposizioni primitive del sistema, sia per le definizioni operative fondamentali che acquistano così pieno significato; si pensi, ad esempio, a quelle di lunghezza e di durata, di massa e di energia, di carica elettrica e di massa ancora, come sono viste dalla relatività speciale. In altre parole: uno stesso criterio vincola gli sviluppi sintattici del discorso logico-matematico e quelli, di carattere semantico, del processo operativo-concreto. Dunque la coerenza metodologica, prima invocata, è operante anche a livello del significato.

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[1] S. F. BARKER, *Induction and hypothesis*, Cornell Un. Press N.Y., 1957, 67.

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PHYSICAL THEORIES AND THE NORMALITY CRITERION

The methodological analysis, presented here, closely follows the historical development of physical theories, and enters into the details of the status *of the (concrete-operational) models which constitute an interpretative structure in terms of physical operations. Such a (physical) model is called* normalized, *if the single, different, concretizations of it (made by several operators) can be coordinated with one another through the invariance criterion characterizing both the class of interested observers-operators and the relational system interpreted in that way. Therefore, let us divide theories into three kinds: a) with direct concrete - operational models, b) with partially* indirect *models, c) with* mediate *direct models; yet having no intention of putting a hierarchy. In conclusion, the normality criterion appears to be the most satisfactory for chosing among competing theories.*

1. — Rather established by now is the definition by which a scientific theory, in particular the physical one, is regarded as a relational system (derived by deductive means from a suitable set of hypotheses) *interpreted* by way of the terms directly or indirectly employed in describing the particular components, or moments, of our experience; such terms, therefore, perform a specific function in the *control* of the operations we do in the midst of both concrete things and subsequent events.

The *formal system*, which consists of some relations that are always expressed, in the so-called exact sciences, or expressible in mathematical form, has a peculiar internal coherence, nevertheless it is very far from fulfilling the logic requirements for completeness; this feature, however, would make the system closed, hence susceptible of undergoing no true development embracing some new components of our experience.

The above-mentioned *control* is aimed at enabling the observers, who operate complying with some specific environmental and operational conditions, to rely upon a satisfactory *reproducibility* of both the intrinsic characteristics and succession of the experimental components. It is just the attainment of such purposes which the aforesaid adjective « satisfactory » is linked with; here, furthermore, the term « reproducibility » is considered to be definable by ostensive means only; the author, therefore, shall be obliged to depend upon reader's collaboration.

The above-mentioned *interpretation* of the formal expressions of a relational system consists in putting their signs in correspondence with some relevant terms, employed in the descriptions of our experience (and explicitly agreed upon by all concerned), which the *status* of quantity, in the mathematical sense, is assigned to - as stated by some particular conventions called « metric ». The above-cited relevance is required to attain the *repeatability* of the operations done by the interested observers to achieve their purposes. We note that the same considerations, which were made about the term « reproducibility », hold as well for the term « repeatability ». In logic, the interpretations fulfilling a given form are called *models*; the last ones make the form itself significant, within a certain range of both human actions and reflections (upon the actions themselves). The interpretations, which are of interest here, assign some meanings in the range of the operations done amidst concrete things; and, therefore, give rise to the *models* (of the formal system) that may be called *concrete-operational.*

The models, set up by physicists and their collaborators, constitute a *functional structure*, characterized by an ordered arrangement aimed at a definite set of purposes; this order obviously matches a corresponding one in the formal system. The latter, therefore, comprehends some expressions that, when interpreted as briefly said above, become the ordering criteria of our experience, in a specified range of operations; and this, even if such criteria are not formulated in the terms explicitly related to the components of our concrete activity (see below, please).

In short, a scientific theory (in particular, the physical one), is constituted by a normative system of relations interpreted by ways of a functional structure of concrete-operational models.

From what is said above, two definitions, already presented in a preceding issue of this Review[1], appear quite acceptable now. In the former, it was stated that: a formal espression, or a hypothetical proposition, has an *empirical-meaning* if all the members of a determinate class of observers have already constructed, or could construct if they would, at least one concrete-operational model. It is obvious that the distinctive features of the aforesaid class of observers must be specified unambiguously; but it should be pointed out, at once, that the requisites of such specification were determined only at a rather advanced stage of development of the theories which concern us closely; this will be shown in detail later on.

The other definition, to remember here, states that the *empirical content* of a relational system consists of the finite set of the models constructed as shown above, and acknowledged as meaningful by all concerned.

2. — The propositions constituting an interpreted system - therefore, a physical theory - are generally formulated in terms not explicitly related to the components, or moments, of our concrete activity; even though they, either explicitly or implicitly, appeal to the external entities - as « things », « objects » - that serve as a support to those components. Some propositions, however, include some words and verbal forms directly pertaining to our actions, that is the so-called *operational definitions*; these definitions introduce the terms pertaining to the language of physics, and specify the procedures to be followed when one operates, on given concrete bodies and in specified environmental conditions, aiming to construct a member of the class which is defined - *in intension* - by the same ordered series of concrete operations: the class of lengths, masses, and so on. Furthermore, these operational definitions enable us to correlate the merely relational and formal terms (and eventual combinations of them) with the observational ones, i.e. with the experimental field; hence they perform a fundamental, unreplaceable, function in the interpretative process of assigning physical meanings.

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[1] D. Faggiani, « *Scientia* », July-August 1969, vol. CIV, N. DCLXXXVII-DCLXXXVIII.

(Physical) *Quantity bearers* can be called the systems of concrete bodies which - at an early stage of the process - were defined from the pragmatic-perceptual point of view only, and which are made undergo the series of operations that serve as an *index* to the corresponding class of quantities, as those specified above; so we can say: a rule bears length, a body bears mass and weight, an event bears duration, and so on. Consequently, these bodies (and systems of them) get a definition formulated in the fundamental terms of the language of physics (i.e.: a physical definition): the body of the common language, therefore, becomes a bearer of physically defined quantities (it is a scientific object, now).

At a later stage of the theory, such a body was conceived as a *macroscopic aggregate* of homogeneous particles, called molecules and atoms; and this in order to specify the necessary conditions for the reproducibility and control of those experiments, which the term « chemical reactions » refers to. The above constituents of the macro-aggregate are in their turn contrived as *sub-aggregates* of particles called « elementary », which are much smaller than the preceding ones, as: electron, proton, neutron, meson, and so on. Within certain limits, some quantities, which are analogous to those ones operatively characterizing the concrete bodies of the preceding paragraph, are also assigned to all the « particles » mentioned above: but, strictly speaking, the last ones must be considered as *pseudo-bearers* (of physical quantities); since the corresponding index-operations are carried out on some macroscopic collectivities only, linked with the particles themselves, as spots or pencils of light emitted by electron or photon excitation, « tracks » in the Wilson cloud chamber or on photographic emulsions, and so on.

The terms employed in some physical theories refer to bearers only, as those defined above; other theories involve some terms pertaining to pseudo-bearers as well. The models of the former theories, therefore, are *directly* concrete-operational, whereas the models of the latter only *indirectly* are (through the corresponding collectivities); here it is explained why we used the two above cited (in italic) adverbs in the first paragraph of the preceding section. All the terms appearing as characteristic in the former theories are present in the latter too; but not all the peculiar terms of the latter are necessary for the significancy of the former theories. The last ones, therefore, perform a propaedeutic function to the whole theoretical talk, and can consequently be called *fundamental*.

3. — The fundamental theories, in the above sense, will be taken into consideration first of all; hence, in particular, mechanics and electromagnetism, together with their relativistic developments. These branches concur to form a coherent relational system containing all the primitive propositions, which are meaningful to the whole specific talk developed by physicists, and that characterize it; such propositions, finally, enable us to specify, in full details, the functions of the fundamental concepts, as: space and time (as defined in physics), mass, energy, and so on.

The quantitative experimental results of several observers, who operate in conformity with *newtonian mechanics*, are compared, and then coordinated with one another, by the well-known group of transformations, generally called

« Galileo's » group. In this case, therefore, it is such a group that serves as the fundamental organon of coordination (mutual comparison).

Otherwise, the comparison of the results of several observers is satisfactorily made in *electromagnetic experiments* with another well-known group, the so-called « Lorentz's » group.

It should be noted now that Galileo's group represents the criterion over which the equations of Newtonian dynamics result to be invariant, whereas Lorentz's group expresses the invariance criterion of Maxwell's equations. Well, the specifying of the transformation formulas (particularly, of the coordinates), under which the aforesaid forms are invariant, is a matter of pure deduction; and this is because Galileo's group is implicit in the dynamical equations, as well as Lorent's group is implied in the electromagnetic equations.

By adopting the terminology used in the preceding sections, one can briefly say that the mechanical models on one hand, the electromagnetic ones on the other, can be coordinated with each other, on the grounds of the invariance group *characterizing the corresponding relational system.* Well, to deem this situation satisfactory (as it was done for a long time) appears a somewhat uncritical attitude nowadays, because this is equivalent to invoke a meaning without specifying the persons interested in the interpretation, i.e. those who are engaged in the construction of concrete-operational models.

The coordination criteria, in mechanics and electromagnetism, are unlike each other; consequently, some discrepancies became apparent when physicists determined to construct some dynamic-electromagnetic models (interpreting both the above-mentioned relational systems) as the models, for instance, related to Kaufmann-Bucherer's experiment or other analogous ones (the very famous Michelson-Morley's experiment included).

As everybody knows, this question had been of interest to scientific critics for decennia[1], from E. Mach to A. Einstein. Well, we can say, in our language, that it was just such criticism that displayed the necessity of defining - in intension - the class of the fully responsible observers, i.e. those to whom such constructions and comparisons (of models) are wholly significant. It follows that the invariance criterion, on the grounds of which those comparisons can satisfactorily be made, is the criterion *characterizing the aforesaid class of rightly interested observers* - rather than the forms of whatever relational system.

Moreover, to generalize the principle of relativity, from the Galilean to Einsteinian meaning, is the same as to define the above class, known as the class of the « inertial » observers, i.e. those who are in rectilinear and uniform relative motion.

However, the specifying of the new invariance criterion is not a merely formal matter, but a question pertaining to empirical science; such a specification presupposes, indeed, that already each observer has *operatively* given full details about his relative motion. Now, this can be done *on condition that* all observers exchange signals (by light or radio), and give a *common interpretation* of them. Well, here is the core of the problem, where lies its solution; after analyzing its consequences, indeed, one can infer that the invariance criterion characterizing the class of inertial observers is given by Lorentz's group[1].

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[1] The connexion between the principle of inertia and the spatio-temporal reference frames was stated first by L. Lange in 1885. Please, refer to: M. JAMMER, *Concepts of space*, p. 138, Harvard Un. Press 1954.

In conclusion: both the electromagnetic equations and the mechanical ones *must* be subordinated to Lorentz's group - for methodological coherence. It is well-known that this statement represented a revolutionary moment to the fundamental physical theory; it led to special Relativity, indeed.

Of the definitions quoted in section 1), the first can be completed by saying that a concrete-operational model is *normalized*, if it can be coordinated on the grounds of the invariance criterion *characterizing the class of interested observers*. We notice that, to meet this requirement, the different concretizations of the model, set up by different members of that class, must be characterized by the numerical values, of the significant quantities, which are in the ratio specified by the transformation group that defines the aforesaid criterion. It can be asserted, therefore, that the models of classical mechanics were not normalized, but became so, in the light of special relativity.

This examination of the fundamental theories shall be closed with a short account of that part of the physical talk dealing with the evolution principle. Here it is: the concrete models of thermodynamics are normalized by the invariance position of the quantity which is peculiar to the evolution, that is entropy, still under Lorentz's group.

It should be noted that this position is coherent with the operational definition which introduces the quantity significant for the interpretation in this case, i.e. temperature.

4. — Other physical theories will briefly be taken into consideration, now. About the theories referring to *pseudo-bearers*, in the sense defined in the second section of this paper, it is sufficient to remark that their empirical content is subordinated to that one of the previously considered theories, which introduce common and primitives physical terms; and, furthermore, there is to add that some eventual new terms must be interpreted - indirectly, in the sense we already defined - by way of some concrete-operational models, normalized in order to get a satisfactory systematization. In this connection, there is to notice that the quantum theories, developed on the grounds of the fundamental Planck's relation, fall within the above rule, on account of the invariance of the actions under Lorentz's group.

The opportunity of employing some terms referring to pseudo-bearers was widely discussed from the logical point of view. Is was pointed out the possibility of making a complete description of any theory, invoking some pseudo-bearers, by employing observational terms only, that is, in our language, a structure of direct concrete-operational models[2] only; but, being articulated into a great many propositions, the talk would become very much complicated in that way. In addition, it should be noted that such terms (atom, electron, meson and so on) performed a very interesting psychological function, without which much slower would have been the process of formation of some new branches of physics - as the history of science suggests.

Finally, in order to complete this analysis, it is necessary to take the

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[1] Those who would like to get a deeper knowledge of this argument may refer to the above-cited author's paper.

[2] Please refer to: C. G. HEMPEL, *La formazione dei concetti e delle teorie della scienza empirica*, Milano, 1961, page 151 and foll.

general relativity into consideration. This theory is constituted by a relational system that can be presented in the abstract, i.e. in a way which is quite independent of experience, for it deals with a very general distributive order giving a frame, which the descriptive components of our experience should be inserted within. However, to put the interpretative, therefore significant, correspondences between such so general relational terms[1] and the descriptive ones, one must make a kind of reduction to classical mechanics and special relativity, about which we think it would be well to give some details.

An expressive and rather simple way of explaining the question is that one making reference to the « trajectory » of a body, that is a planet, in particular Mercury, in this case. Well, the equation of its trajectory can be expressed as follows[2], in conformity with general relativity:

$$\left(\frac{du_o}{d\varphi_o}\right)^2 + u^2_o = k - \frac{I}{h^2} u_o + Iu^3_o, \tag{1}$$

where k, h and I are some constants wholly undetermined till now, whereas u_o and φ_o are some "coordinates" that, we notice, are not yet interpreted on the grounds of physical operations.

The classical equation of the orbit of a planet (in particular, an elliptic one) can be written, in plane polar coordinates (physically interpreted), as follows:

$$\left(\frac{du}{d\varphi}\right)^2 + u^2 = \frac{K}{H^2} + \frac{2M}{H^2} u, \tag{2}$$

where M = the so-called reduced mass of the sun, H = the integral of areas and K = the integral of energy; these quantities too have a direct and exhaustive specification in some well-defined physical operations.

Equation (2) enables us to assign a meaning to relation (1) by ways of some positions, of which it is of interest the explicit specification here (and this is a question generally disregarded). They are:

A) The generalized « coordinates » u_o and φ_o are identified with the operational ones u and φ.

B) On the condition that general relativity includes the special one as a limit case, and since the velocity of the planet is always much lower than the light velocity c, one obtains the following relation as a satisfactory approximation:

$$h^2 = -\left(\frac{H}{c}\right)^2.$$

C) On the grounds of that, relations (1) and (2) take a like form, but for the term in u^3 of equation (1). As soon as the constants are evaluated by comparison between the corresponding terms, k and I become operatively specified.

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[1] « Physically meaningless »: these are Einstein's words. Please, see page 122 of the book: *50 anni di Relatività*, Ed. Universitaria, Firenze, 1955.

[2] G. Y. Rainich, *Mathematics for relativity*, N. Y. London, 1950, page 158.

With that, equation (1) is *semanticized* for physicists' purposes in the following relation:

$$\left(\frac{du}{d\varphi}\right)^2 + u^2 = \frac{K}{H^2} + \frac{2M}{H^2}u + \frac{2M}{c^2}u^3;$$

and it is to be noted that *the shifting of the perihelion* of Mercury is justified by the term un u^3 in a satisfactory way.

Well, let us call « mediation » the function performed by classical mechanics and special relativity, as we said above; then the concrete operational models of general relativity may not simply be called direct models, but must be called *mediate* direct models.

From this analysis, it follows that the normalized physical theories can be divided into three kinds, depending on the *status* of the concrete-operational models that constitute the interpretative structure of the forms of the relational system:
— theories with direct models (fundamental theories);
— theories with some (at least one) indirect models (in general statistical theories)
— theories with mediate direct models (general relativity).

It should be noted, finally, that we do not intend to put a hierarchy with that, but only to throw light upon a distinction.

5. — The analysis carried out hereinbefore makes itself appreciated, because of its closeness in mirroring the historical development of the fundamental theories. In addition, complying with the thesis that considers the scientific propositions to be some ordering-criteria of our experience, this analysis lead to make the simplicity, or economy - as features for choosing among competing theories - be replaced by the *normality* criterion, which can be called so, on the grounds of the definitions accepted in the preceding: that is the reducibility to a defined norm, which becomes *significant for both the class of interested observers and the ordering-forms.*

Almost all physicists - though any of them do not consent to the above thesis - generally deem that, with special relativity, the two main, early chapters of theoretical physics, that is mechanics and electromagnetism, have achieved a satisfactory systematization. The characters of methodological coherence displayed here, in addition, lead to believe that the new forms, which in the future will constitute the cardinal axes of the physical coordination, will hardly be allowed to disregard the above systematization; such systematization, however, is not likely to be regarded as the ultimate word in science.

Let the empirical content of a theory be given by the whole set of concrete-operational models, then the question arises about the major or minor content of competing theories as, for instance, Newton's or Einstein's dynamics. The latter is generally said to have a major content, because it answers some questions, formulated in observational terms, about which the former is deaf; and it is on the grounds of this peculiarity, and only of this one, that some people consider Einstein's theory to be preferable, so reducing the question to a merely quantitative matter. Other physicists, maybe most of them to-day, keep as well in their mind that Einstein's theory resolves some contradictions involved in Newton's dynamics, so reducing the question to a rather qualitative matter. Well, it seems to the author that, by making reference to the normality criterion, the highest importance is attached to the last considerations, that undoubtedly better characterize the scientific method by designating what may be called its coherence, as noticed above; in this case, however, such a coherence must not restrict itself to the mere formal aspect. Besides, let us remark that our criterion has been working the last seventy years in the development of science, even if no one has been aware of that in the terms introduced in this paper.

In the last decennia, many new attempts were made to justify the inductive method from the logical point of view, and some of them led to very elaborate theories. But these ones also proved to be unsuitable for including the generalizations, and least of all the functional relations, like those ones on which a sistematized empirical science is articulated. Many authors, therefore, gave up such a justification and resorted to a less ambitious program: to set forth a criterion for choosing among competing theories or hypotheses; some of them recognized the simplicity criterion as suitable; this one, however, is still somewhat hazy. S. F. Barker[1] rather recently gave it a precise definition suggested by Kemeny's concept of logical measure, which is of interest to remember here; the simpler the theory, the less its logical measure, where the last one is given by the number of *logically* possible models of the corresponding system. But one may ask: whensoever worked this criterion in the development of science? It seems to the author that the answer is wholly negative; really, whenever two (or more) competing theories faced each other, they never presented the same empirical content; therefore, the criterion under discussion has found no possible application; and this, in consequence of its own formulation.

Finally, the case of competing theories with different empirical content deserves to be briefly considered. Let n be the number of the already constructed concrete-operational models, and N that one of the logically possible corresponding models; well, let us call $\frac{n}{N}$ *saturation ratio*. The following question arises now: which theories shall we choose? the one with the largest n or the one with the highest saturation ratio, which, we notice, may turn out to be characterized by a smaller n? In case we should prefer the first choice, which is much more significant for the true development of science, the simplicity criterion would not even be concerned in the matter.

An analogous remark could be made as well on what concerns our normality criterion; that is, one could ask: which of these two theories, one with a minor empirical content but consisting of n normalized models, the other constituted by n' non-normalized models, where $n' > n$, is to be chosen? The author notes that this case never occurred till now; nevertheless he will give an answer all the same: the former theory must be chosen in the systematic area (interesting the science), even if the latter may be preferred in practice (in the technique) because of its greater usefulness.

In conclusion, the author deems that the above-mentioned double significancy of the fundamental invariance criterion (Lorentz's group) assigns a quite satisfactory *status* to the scientific, in particular physical, talk: as a matter of fact, beside the merely formal coherence of the hypothetico-deductive system, the said significancy places an interconnectivity between the system itself and its experimental interpretation, which is deeper than that one put by the mere final comparison between the particular relations closing the system and the corresponding descriptions of suitably-chosen concrete situations. The same criterion is binding on both the fundamental propositions of the system and the basal operational definitions which thus become fully significant: for instance, think of those ones concerning length and time, mass and energy, electrical charge and mass again, such as these are envisaged by special relativity. In other words: the same criterion is binding on both the syntactical developments of the talk, in the logico-mathematical language, and the semantic ones of the concrete-operational process. Therefore, the methological coherence, invoked before, works also at the meaning level.

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[1] S. F. Barker, *Induction and hypothesis*, Cornell Un. Press N.Y., 1957, 67.

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CHINA'S POPULATION POLICY

China's population policy is an enigma. Over the years it has oscillated between the tendency to glorify population increase and what may be called « contraceptology », and has been influenced by political imperatives, nuclear ambitions and pragmatic considerations rather than demographic variables. Dogma, too, has played a role, but in case of failure it has seldom proved irreversible. Again, although they are not totally separate, four distinct phases are perceptible in the development of this « start-stop-start » population policy. Almost until the autumn of 1956 the policy was formulated, by and large, on the basis of motives favouring fertility. This period was, however, followed by a short-lived but intense birth control campaign. Then in the summer of 1958 the policy reversed and birth control became taboo. Another reversal, this time toward the close of 1962, and the birth control campaign was resurrected. In the current phase, the campaign's focal point is the dissuasion from early marriage, so that late marriage, along with sterilization, abortion and the use of conventional contraceptives, has become the hallmark of socialist virtue. At the same time, Mao's « man over weapon » theme continues to be reverently followed as the main scripture on nation building.

China's population policy is not well known. Before attempting to describe it and chart its course of development, a few introductory remarks are in order. In the first place, it must be emphasized that the present policy is governed mainly by the regime's obsessive desire to influence the world's political climate. In other words, the conventional demographic variable in the Malthusian sense, food, has played an insignificant role in its development. Secondly, even the Peking planners knew that their pro-fertility campaign before 1956 and from July 1958 to the close of 1962 would not have much effect on the already high levels of fertility in China; but such a move was seen as essential for keeping the population in a state of preparedness, in a psychological ferment in which a militant ideology could brew. The authorities could not create such an atmosphere against the pessimistic background of a fertility control programme. Thirdly, the planners in Peking knew that the birth control campaign carried out from autumn 1956 to summer 1958 had no effect even in those urban areas where it was pursued with missionary zeal. They were, however, in no mood to swing their pro-fertility policy in the opposite direction until virtually forced to do so by political considerations. Evidence will be presented below to clarify this proposition. Fourthly, it was not the anxiety about food supplies that triggered the 1956 birth control campaign, nor was it optimism on this front that brought about the switch to pro-fertility after July 1958. Similarly, the birth control campaign which has now been in effect since the close of 1962 cannot be explained in terms of the crop failures of the disastrous 1959-1961 period when a pro-fertility policy was operative.

Over the years, Chinese population policies have oscillated between the tendency to glorify population increase and what may be called « contraceptology ». Of course, the about-faces were not always easy to make because policy was not elastic. Ideological orthodoxy has always been opposed to birth control. But when pressed hard enough by reality, the Chinese are prepared to modify their policies. The problem of feeding the people always stares rulers everywhere in the face, and they must take it seriously at some stage. Yet in the case of the Chinese, other factors seem to have had more influence on their thinking, among them the strong nationalist desire to populate sparsely inhabited areas or dilute the racial stock of local frontier populations, and the need for defence tied to foreign relations. The aim of this article is to trace the influence of these other factors in the development of the official approach to the problem of population.

Like most aspects of domestic policy, the population policy of China during the last sixteen years has been a primarily political phenomenon. That is to say, basically political considerations have steered the course of the official approach to the population problem and have also permeated the economic arguments with which these policies have been rationalised. The evidence on « ideological ironies » presented below would tend to suggest that China's population policy has in fact resulted from pragmatic considerations. Dogma has played a role but has seldom been maintained in the face of clear indications that a given dogmatic solution was fruitless.

THE PROBLEMS. — When the Communists took over mainland China in October 1949, they inherited a vast country with an immense population. To cope with the aftermath of civil war and the problems of underdevelopment, they could only rely on their own fervour and, hopefully, the « 500 million » with no more than their traditional tools. This large human potential was regarded as the most treasured asset in carrying out a four-fold programme:

(a) To consolidate the uncontrolled territories — Sinkiang, Inner Mongolia and other distant areas dominated by warlords — into one « Great China ».
(b) To salvage the « lost territories » — Tibet, Korea, Viet-Nam, Laos, Thailand, and Outer Mongolia — which Peking claimed once owed allegiance to the « Land of Cathay » [1] and also to reclaim Manchuria over which the Russians still had control.
(c) To put the shaky national economy on a solid footing.
(d) As a leading member of the Socialist camp, to spread the Communist ideology throughout Southeast Asia and the acknowledgement of China's status as both a « superpower » and a model for the underdeveloped nations.

None of these objectives could be achieved easily or quickly. China could not reach independent power status unless she were militarily strong — a condition vastly complicated by the nuclear age.

It is against this background, and the subsequent concatenation of events that required repeated shifts of emphasis regarding the conventional « man over weapon » theme, that the rationale of Communist China's population policy must be studied.

When capital and other material resources are lacking, it is perhaps natural that an overpopulated and underdeveloped country should want to make the best of its manpower resources both for internal reconstruction and defence. This is Mao's own teaching. As early as 1936, he had proclaimed in his 'On Protracted War':

> The decisive factor is men, not material. The comparison of strength means more than a comparison of military strength and economic strength. It is also a comparison of manpower resources - Military strength and economic strength have to be manned.

Marx in his dispute with Malthus had maintained that in a Socialist state a large population was a cardinal virtue. All the rigour of Marx's logic was applied in China, where a large and growing population was favoured by the need to man the Red Army, build public and defence works and maintain supply and communications arteries to the front, and populate Chinese territories contiguous to the U.S.S.R. and India. Dean Acheson's concern with overpopulation in his 1948 White Paper [2], in which he deplored China's overpopulation, was dismissed with sheer contempt [3] by the new regime in 1949. Birth control was pronounced « anti-humanitarian » and condemned as « a method of killing the Chinese without shedding blood » [4]. The huge masses, in consequence, posed no problem for China's enthusiastic leaders. They regarded their manpower resources as a potent weapon in the reconstruction and defence of their country, which was undeveloped and lacking in the modern means of both production and warfare.

THE FIRST PHASE. — The Communists launched their population policy with the slogan, « Population, more population, still more population », and set about uniting the divided territorial units into a single « organic whole » and bringing immediately adjacent areas under their influence. A year later, after the establishment of the People's Republic, they remained true to their word about salvaging the lost territories by marching their armies up to Tibetan peaks. In 1950-51, the human wave of the Chinese armies forced United Nations troops back from the Yalu River to the 38th parallel in Korea. China's fear of a U.S.-backed Kuomintang invasion, and her self-conferred responsibility to strengthen North Vietnam, bolster the insurgent Pathet Lao forces, aid the guerrilla groups in Malaya and feed the « patriotic revolutionary war » in Indo-China, all added to the demand on her manpower resources. Similarly, the intensification of the « Liberate Taiwan » campaign in 1954, when petitions were signed in blood, speeches made and resolutions endorsed for « the immediate liberation of Taiwan », and the shorter and less intensive 1956 campaign which pledged « volunteers for Egypt » during the Suez crisis, also tended to underpin the preference for a large population to psychologically sustain the tempo of these militant and revolutionary ventures. In this respect, the Chinese were more concerned with their immediate gains than their long term objectives.

Inside China herself, a large population was needed not only to man labour-intensive public works but also to people the sparsely populated western border areas. By 1954, Sinkiang had been settled with PLA contingents of lakhs [5], not only to subjugate the indigenous population but also to diminish its numerical supremacy. About the same time, Inner Mongolian, where Mongols predominated, was joined with Suiyan Province, and towards the close of 1955, also with the northern part of Jehol. A large number of Chinese from the thickly populated centre and south were drafted into this region, and subsequent events tilted the balance of population in favour of the Chinese in a very significant manner. The local populations, whose loyalty to Peking was suspect, were submerged by the Chinese, thereby enabling Peking to consolidate its power in the frontier areas contiguous to the U.S.S.R. and India. This promoted a general psychological propensity toward a large population, and in such an atmosphere of unbounded optimism a pro-fertility policy was a logical outcome. Chinese leaders could not simultaneously maintain that China's vast extents of land needed to be peopled and ploughed, and also preach birth control to stem population growth.

All this added to the Marxist-orientated propaganda emphasis on large population. Jo Shui [6], in the « Sixth Comment on the White Paper » in the August 7, 1954 issue of People's Daily, said that a large population was « a very good thing », and averred that « all pessimistic statements were entirely groundless ». The 1954 Census, which revealed that China had a population of 583 million, inspired the statistician Pai Chien-hua [7] to declare in the People's Daily of November 1, 1954 that China's large population was « a great resource for domestic construction and national defence ». Among those responsible for planning, the conviction gained ground that « what really counts in nation building is men rather than material, morale rather than machinery, and revolutionary spirit rather than the tools and techniques of science and technology ». They took this quite literally, believing that if they could mobilize and manipulate the energy and enthusiasm of the masses they could overcome all the difficulties involved in transforming an old, agrarian China into a modern, industrial power. They could, in short, make China the centre of the world, embracing all the countries of Asia first, and later drawing other nations as well into its sphere of influence.

Pride in numbers and the traditional Chinese preference for large families with many sons coincided neatly with the Communist thesis that overpopulation was a capitalist, not a socialist, ailment. It became known later, however, that the optimistic language did not hide the very real Chinese concern about over population. Some apprehension [8] about the alarming growth existed in high places, but this remained stifled, probably due both to China's international military obligations and to dogmatic compulsions. About this time, during a visit to Peking in August 1954, Clement Attlee had a conversation with Chinese leaders in which they summed up their government's position on the population question. Attlee reported this conversation in The New Tork Times of September 13, 1954 as follows:

> When it was suggested that the important thing was the relationship between production and the number of consumers, and that what was desirable was a higher standard of living, they (the Communists) could give no effective reply except to say that China regarded a large population as desirable, as tending to increase the production of commodities... I think that the real reason for this is that China, being admittedly a backward country, hopes to make up in quantity what it lacks in quality, *in order to achieve a position of power in the world.* This is a disturbing thought. (Emphasis added.).

The relation between China's nuclear ambitions and its leaders' attitude towards the population problem makes an interesting study. When the leaders felt worried about the enemy's nuclear might, they tended to decry it. In terms of nuclear capacity, China herself was then in her infancy. With only 10 physicists in 1949, the new regime was primarily concerned with expanding its scientific effort at home and seeking outside cooperation, mainly Russian. Meanwhile, until the day when China could produce her own nuclear weapons, Peking had no option but to emphasis the merits of a strategy which relied mainly on manpower. A State pro-fertility policy was brought to the forefront, and this stage of development of China's attitude toward her population was characterized by the disparagement of nuclear weapons. The following statements illustrate the range of opinion during the period from Winter 1949 to Autumn 1956 [9].

> The atomic and hydrogen bombs are not formidable; only the growing strength of the people is formidable... If the imperialists dare to unleash a Third World War, they are preparing their own cremation.

> (Kuo Mo-jo, Chairman of the Chinese Academy of Sciences, at a mass « peace » rally in Peking, May 14, 1950).

On March 31, 1954 [10], the New China News Agency reported Madame Li Te-chuan, President of the National Red Cross Society of China, as saying: « No amount of sophistry can change the destructive nature of atomic weapons, which devastate a wide area and contaminate the atmosphere with deadly radiation ». On April 4, 1954, the same news agency quoted a Chinese nuclear physicist to the effect that atomic weapons, used « tactically » or otherwise, were weapons of savage mass destruction. K. M. Pannikar [11], the Indian Ambassador to Peking, reported in 1955 the nature of the Chinese consensus regarding the consequences of an atomic attack. He was told in a conversation with the Deputy Chief-of-Staff of the Chinese People's Liberation Army, General Nieh Jung-chen:

> The Americans can bomb us, they can destroy our industries, but they cannot defeat us on land... We have calculated all that. They may even drop atom bombs on us. What then? They may kill a few million people. Without sacrifice a nation's independence cannot be maintained... After all, China lives on the farms. What can the atom bombs do there? Yes, our economic development will be put back. We may have to wait for it.

And on August 22, 1956, the then Defence Minister, P'eng Tehhuai, told a mission of former military officers [12]:

> America possesses atomic weapons and is threatening us with them. But we are not afraid of atomic warfare. Why? Because China has 600 million people. Even if 200 million people were killed by atomic weapons, 400 million would still survive. Even if 400 million people were killed, 200 million would still survive. Even if 200 million survive, China will still constitute one of the world's big countries.

Consciously or unconsciously, P'eng here was projecting into the nuclear age the image earlier developed by Mao Tse-Tung in July 1936, during a conversation with Edgar Snow, in connection with the war against Japan [13]:

> China is a big country. If Japan succeeds in occupying even a large section of China, getting two hundred million people, we would still be far from defeated. We would still be left with a great force with which to fight against Japan's warlordism.

Even as late as 1961, a dispatch of Tillman Durdin's for The New York Times Magazine (the issue of January 15th) attributed to Mao and other prominent Chinese leaders a statement to the effect that:

> Nuclear war holds no terror for Communist China because, even if 300,000,000 Chinese were killed, there would still be enough left to exist as a nation and even take over a world devastated by nuclear blasts.

All the quotations above express a psychological tendency of the weak to seek solace in numbers, resulting quite naturally in a pro-fertility policy.

THE SECOND PHASE. — By the autumn of 1956, Communist China, although far from being a nuclear might in the international sphere, was well on her way to accomplishing her four-fold objectives. Tibet had been annexed, Sinkiang and Inner Mongolia had been made autonomous regions, and warlordism had been extinguished. Peking had also extended its influence to Korea, Vietnam and Laos, territories contiguous to the mainland and of great strategic importance. And above all, with the promise of Russian aid, Peking was quite optimistic about building her own nuclear capability in the near future. This optimism led to a sober re-assessment of the population policy. The authorities were now able to view more rationally the demographic realities that would compound their difficulties in the near future. There was, of course, some embarrassment at the subsequent volte-face. But in other instances the fear of inconsistency or deviation from Marxist doctrine on the subject had not proved to be a serious deterrent. The pro-fertility period was, then, closed and the new program of birth control was initiated when Premier Chou En-lai put the stamp of authority and respectibility on it. Speaking to the National Party Congress in September, 1956, he said [14]:

> To protect women and children and bring up and educate our younger generation in a way conducive to the health and prosperity of the nation, we agree that due measures of birth control are desirable.

This sounded a little un-Marxist. But once the leadership had made up its mind, it was ready to amend theory to suit the needs of the hour. In the present context China, although invoking different motives, adopted a frankly Malthusian attitude as soon as her planners began to see the citizens as a burden.

After Chou En-lai's rationalization of the « new programme », the concern with fertility management, which because of dogmatic compulsions had been kept below the surface for some time (since 1954, to be precise), erupted in a burst of vitriolic controversy that astonished a largely sceptical outside world. The avalanche of birth control propaganda that swept the country disclosed an immeasurable breach in the seemingly granite-like Communist regime's doctrinaire Marxist approach to the population problem. The pro-fertility façade the Chinese leaders had so ardently advocated a few years earlier crumbled as new developments at home and abroad created new problems.

A professional statistician, Ta Chen [15], revealed that China intended to reduce her birth rate in ten years by fifty percent, from 37 per 1000 to about 18 or 19. In 1957, according to official reports, contraceptives were distributed to 25 million families. The match boxes sold by cooperative stores bore words advertising the official line, « Mothers, limit your babies to preserve your health » [16] with a directness which even Western family planning propagandists would find shocking.

Hints of this shift in the population and nuclear policies were in evidence even before Autumn 1956 when the volte-face was about to be started. But theoretical considerations probably deterred the Peking planners from announcing the change. On January 18, 1955, for example, the USSR Council of Ministers announced that limited peaceful nuclear sharing would be undertaken by some of the countries in the Socialist camp [17]. This was followed by large-scale Russian H-bomb tests the subsequent autumn. Soviet self-confidence visibly increased, and possibly it was this that during the following year lent support to that element of the Chinese PCP inclined to believe that the Soviet Union now possessed enough deterrent force to cover even Chinese territory. At this stage the official line on the question of population growth had already deviated slightly away from Marxist theory. This is evident from a number of articles by Shao Li-tzu and Dr. Chou Ngo-fen in « China Youth » and other periodicals during 1955, all supporting population control without overt opposition.

In March 1957 Madame Li Teh-chuan, China's Health Minister, finally raised the curtain on the first full-scale birth control campaign in a Communist state, to the great surprise of a sceptical world. In the pessimistic tone characteristic of Malthus, Madame Li affirmed:

> If our population growth is not governed by planned childbirth, our country will be hindered in ridding itself of poverty and becoming prosperous and powerful... [18].

Many Chinese writers throughout 1957 supported the idea of birth control. They even had the authority of Engels to draw on. Writing in Ching Chi Yen Chiu (February 17, 1958), Wang Tso and Taj Yuan quoted Engels to the effect that [19]:

> If there is a time when a Communist society feels obliged to control the growth of its population, as in the earlier case of controlling material production, only that society can bring this about without difficulty. (Engels' letter to Kautsky, published in the 'Study and Translation Series', Issue No. 3, 1958). Therefore Marxists do not object to birth control.

Communist doctrine is very flexible indeed in this area!

It was also at this formative stage [20] in the development of a population policy in China that Russia agreed to assist her, under the terms of the Agreement on New Technology for National Defence signed on October 15, 1957, in the manufacture of the atom bomb. In fact, China received a great deal of cooperation from Russia in nuclear and other fields from 1955 onward, especially after the signing of the agreement. This apparently prompted the Chinese to embark on a nation-wide campaign of family limitation. About the time when the campaign reached its peak, Chen Yi, who in February 1958 succeeded Chou En-lai as Foreign Minister, told a German correspondent in Peking in May of that year that China would have atomic weapons in the near future (New York Times, October 1964). This was followed by a wide-spread and persistent rumor in July 1958, believed to have been circulated by Polish sources, that the Russians would help the Chinese build up an atomic and missile capability of their own. The possession of nuclear might, even if limited, would back up the claim to the great-power status which in the past China had been trying to enforce by the sheer weight of her population. It would increase China's ability to establish her hegemony in Asia. The Chinese recognized that being the only nuclear power in Asia would substantially increase their country's prestige among Asian élites and would strengthen the feelings of people like Prince Sihanouk of Cambodia and General Ne Win of Burma who felt that small countries in Asia must make their peace with Peking.

THE THIRD PHASE. — Only seven or eight months after this Promethean Chinese campaign against the birth rate had reached its peak in the spring of 1957, it ran into difficulties, and the first indication came of a change in Peking's policy. From September 1957 to June 1958 the 'rightests' and their ideas were continually subjected to criticism. The emphasis in policy statements again reverted to the Marxist optimism of an earlier period which saw people mainly as producers of wealth, not consumers, and implied that China therefore needed still more people. There was as yet, however, no visible relaxation in the enforcement of the birth control campaign and it had not been publicly repudiated. Nevertheless, by the summer of 1958, Peking's approach to the population problem had, to use a popular Chinese phrase, «made a 180 degree turn». No official reasons were given for this reversal. But an analysis of it as made by outside observers permits us to reach a few general conclusions.

According to one view, the chief reason for the reversal were the operational difficulties encountered in implementing a nation-wide fertility management programme. Possible causes of this could have been the difficulties in producing and distributing contraceptives and in disseminating contraceptive education, and the cultural inertia of rural folk who would not easily relinquish their age-old hostility towards limiting births. Yet if birth control had been linked up with the national economic programme, it would not have been abandoned merely because of such difficulties. The Chinese leaders have given sufficient proof in the past of their tenacity in pursuing other domestic programmes which they regarded as essential even though they were unpopular. The jettisoning of the birth control programme must, instead, have been the result of a fundamental re-orientation.

Some observers have suggested that the campaign was scrapped because the message of birth control had an overtone of pessimism which ran counter to the abounding optimism characteristic of the 'Great Leap Forward' period. The bumper harvest of 1957 inspired confidence that the country had enough to eat and that with even more hands to work the situation would improve still further. The scrapping of the campaign was in keeping with this confidence, and China's vast population came to be looked on as an asset rather than a liability in bringing about the tremendous leaps in agriculture and industry which the regime had planned. The People's Daily wrote from Peking on December 19, 1958:

> People in the past (have often worried) about 'over-population'... But the idea has been overturned... The question is not so much overpopulation as a manpower shortage.

Elsewhere [21], this same « belief that the value of man's daily labour will exceed the value of his consumption and that therefore China has no need to fear its growing population » has been labeled « economic nonsense ».

An analysis of the food situation vis-a-vis population policy for the period 1949-1958 rules out, however, the contention that mere optimism on the food front was sufficient to bring about a change in the population policy. In fact, there does not appear to be any fixed causal relationship between these two factors:

> Up to 1955 the food picture was not rosy but a pro-births policy was favoured nonetheless. Shao [22] had in fact hinted in 1954 that population pressure complicated the problem of food supply. This warning bell went unheeded for a year and a half, when the first birth control campaign became public. If the food problem was so very serious, long inaction was unjustified.
>
> During 1955 and 1956, i.e., immediately before the first birth control campaign, official statistics showed that grain production increased more rapidly than population, and the idea of population pressure was therefore rejected outright [23]. And yet the autumn of 1956 saw the laun-

> ching of a campaign to control China's exploding population. How could improvement in the food supply be reconciled with this campaign which, because of its pessimistic character, should have stemmed from food shortage? In fact, writers who tried to link the food problem with population growth were silenced during the anti-rightest campaign in mid-summer 1957 [24], and it was asserted that the campaigners were former landlords and wealthy peasants [25] but that now, since « right and wrong had been distinguished », everyone admitted that there were no shortages [26].
>
> During the « finest hour » of the campaign (autumn 1956 to summer 1958) the food situation had improved. In fact, the best figure for grain production was for 1957 (185 million tons). But at about this time it was not the pro-birth policy but the birth control campaign that was being intensified.

An equally important explanation for this reversal has to do with changes in the sphere of international relations, which strengthened the original Marxist predilection for uncontrolled population growth. Even ideology cannot be ruled out as a cause. On the home front, the « man over weapon » theme was again played up with a view to building up a vast militia of 220 million people, to serve as both a labour force and an auxiliary to the main fighting forces.

In the sphere of foreign relations, the end of 1958 brought the beginnings of disillusionment, specifically with Khruschchev's 1957 promise to provide the Chinese with an atom bomb. Khrushchev, no doubt alarmed by the militant Chinese policy of victory through war, tore up the agreement. The Sino-Soviet quarrel, which has today developed into a cold war, dates from this period and probably from this cause. It meant that Peking could no longer depend on the nuclear might of the Soviet Union in order to satisfy its own national objectives. The situation with regard to the offshore islands of Quemoy and Matsu also became critical; there were large-scale uprisings in Sinkiang and Tibet; and relations with India deteriorated. All these factors forced China once again to find solace in her most powerful weapon - population. Hence the reversion to a pro-births policy to ignite the psychological spark of militancy.

The break in Sino-Soviet relations imposed the need for a speedier assimilation of the native populations in the strategically important border areas of the West. Only a larger Han population could raise the ratio of Chinese to non-Chinese in such areas of national minorities. Consequently, some 9 million Chinese were introduced into Inner Mongolia alone, altering the Chinese—non-Chinese ratio from 2 to 1 to 8 to 1 in a total population of 11 or 12 million in 1960 [27]. In Sinkiang, the proportion of Chinese in the total population increased to 30% [28] from a bare 5% in 1949 and 6% in 1953 (census figures). In 1962, the Chinese in Sinkiang numbered 2,000,000 out of a total population of 7,000,000. In Tibet, 5,800,000 Chinese had been settled by the beginning of 1959 and 4,000,000 more were expected to be settled shortly [29]. These mass population movements required an optimistic atmosphere with an emphasis on population growth in order to sustain large-scale migration and consolidate Peking's hold over such vulnerable areas. Thus a large population, even if it created 'difficulties in living', once again came to be favoured as a factor of strategic importance. China's possession of a nuclear weapon at this stage might have obviated this need to recur to pro-birth policies. This stage was in fact characterized by a transitional strategy — that of relying upon manpower resources until such time as China could detonate a nuclear device of her own. In the meanwhile, a barrage of propaganda against nuclear weapons continued. The following statements, taken from the New China News Agency, English, Peking, and translated in the Survey of the China Mainland Press, Hong Kong, for the years 1961-1962, are significant:

> The Chinese people have all along stood for disarmament, prohibition of the tests, production, stockpiling and use of all nuclear weapons.
> (Liu Ning-yi, NCNA, March 31, 1961; translated in SCMP No. 2471).
>
> We resolutely support the firm, repeatedly reiterated stand of the Soviet Union on the question of disarmament and cessation of tests.
> (Kuo Mo-jo, NCNA, August 8, 1961; translated in SCMP No. 2559).
>
> The people throughout the world are opposed to nuclear war.
> (Ch'en Yi, NCNA, April 4, 1962; translated in SCMP No. 2717).
>
> We consistently advocate universal disarmament and the banning of nuclear weapons.
> (Yang Hai-po, NCNA-Moscow, April 16, 1962; translated in SCMP No. 2725).
>
> We are convinced that the outcome of struggle can only be the destruction of nuclear weapons by mankind, not the annihilation of mankind by nuclear weapons.
> (JMJP Editorial, NCNA, April 28, 1962; translated in SCMP No. 2731).
>
> The destiny of mankind is decided by the people, not by weapons. Atomic and thermo-nuclear weapons are merely paper tigers.... In the final analysis, mankind will destroy nuclear weapons, not vice versa. Only the people are the most powerful and invincible force.

> (Ta-kung Pao Editorial, NCNA July 12, 1962; translated in SCMP No. 2780).
>
> We consider the most urgent and effective measure to insure world peace and the security of mankind is to destroy completely all the nuclear weapons now in possession of the nuclear powers.
>
> (Pa Chin, NCNA, August 1, 1962; translated in SCMP No. 2795).

And yet, under the camouflage of this spate of statements disparaging nuclear weapons, Chinese scientists were actively but secretly engaged in producing their first atom bomb, which they finally tested in 1964.

THE FOURTH PHASE. — China's population policy was, however, destined to take another sharp turn — to come full circle, in fact — in the years that followed the Great Leap fiasco, or, more particularly, in the three subsequent years of « natural calamities » and agricultural failures. The course of events in the third phase, which were to be responsible for the change, also meant that China had to develop a nuclear might of her own — a task which, though rendered formidable by the cancellation of Russian aid in August 1960, was nonetheless within the realm of possibility. The nuclear weapons programme under Wang Kan-chiang made swift headway, and from all accounts the prospects of possessing an atom bomb, as they appeared towards the close of 1962, seemed quite bright. It was at this point that the pro-births policy which had been adopted again only a few years ago as a 'marriage of convenience' was once again dissolved. This reversal was implemented despite Peking's growing apprehension of a U.S.-backed Kuomintang invasion to reclaim the mainland, as well as territorial disputes with both the Soviet Union and India. The prospect of soon possessing a nuclear stockpile raised Peking's morale and prestige. The pendulum once again swung over to the side of population control. While some of the opposition represented Marxist dogmatism or still smouldering leanings towards a pro-births policy, the methods of controlling population were more sober and also less direct. To begin with, a crusade against early marriage was launched, and the press was crammed with articles, editorials and letters extolling the virtues of late marriage throughout 1962. The adverse effects of early marriage were stressed and the prudence of late marriage underlined. As time passed, this crusade was intensified to fever pitch. The Chinese press used emotionally arousing headlines like the following, which give the spirit of the times:

> « Early marriage hinders my work »; « What is the suitable age for marriage? »; « Early marriage is harmful, not ben-

> eficial »; « For late marriage »; « My view of marriage, love and children »; « Talk with young friends on the question of marriage »; « Be brave and get rid of the old custom of marrying early »; « We advise falling in love early but marrying late »; « A talk about the question of early marriage from the physiological angle »; « Do away with the custom of early marriage »; « On the encouragement of early marriage »; « Foresight and prudence are necessary in handling the question of marriage »; « Should I refuse to marry? », etc. etc.

« Some young people favour falling in love and getting married as early as possible on the grounds that 'You are only young once' », complained Huang Shu-tse in the China Youth News of March 30, 1963 in reply to a reader's letter. « Youth », Huang went on, « certainly does not mean around 20. If a man gets married at 27 or 28 and a woman at 25 or 26, both are still getting married in their youth and within the young people's age group ».

A Press Trust of India report indicates that the crusade against love and marriage is carried out using arguments that stagger the imagination of scientifically-orientated people. For example, erring young couples who fall in love are told that killing mosquitoes is a better pastime. The official attitude is that love is « a psychosomatic activity which consumes energy and wastes time », and that young couples in China should instead « devote their energies to campaigning for public health and to killing mosquitoes ». And another recent Associated Press dispatch from Tokyo says, « Women in China have no time for love and frills... The last thing a loyal follower of Mao Tse-tung would do is to show her curves. Others would look at her and production would be hampered ». This is the gist of reports coming to Tokyo from China, and they add, « Marriage is taboo until the mid-twenties for women... Billing and cooing is strictly bourgeois... Girl watching is, of course, completely out » (cf. Hindustan Times, Delhi, April 14, 1966).

The crusade against marriage was the strongpoint in the Chinese strategy to lower the birth rate, but other conventional measures, common to any birth control programme, co-existed alongside it. Tariffs on contraceptive instruments and medicines were lowered, and restrictions on abortion and sterilization were relaxed. In Canton, for example, factory clinics issue certificates entitling the holder to free abortions. The cult of contraception is being propagated: various means are being used to make the masses 'contraception-minded'. Among these means are condoms, diaphragms, intra-uterine contraceptive rings (made of gold, platinum, stainless steel or plastic), chemical spermicides, and the rhythm method [30]. One source warned against the mistaken belief that prolonged breast feeding had a contraceptive function [31].

Peking has not revealed any statistics on the success of its birth control programme. Private reports suggest that progress has been made in urban centres. In Shanghai, birth control propaganda is common. Lectures are given in factories and offices, and contraceptives can be purchased cheaply in shops and at cigarette kiosks. The government is still experimenting with a contraceptive pill and has not yet made it generally available (New York Times, April 27, 1966). There is, sometimes, even an element of coercion in these measures. Recent letters from mainland China and from provincial newspapers reveal that third pregnancies are artificially interrupted and that moreover the women concerned are sterilised. According to another news item, China's latest measures promise to provide a simple and drastic curb on unplanned babies. The fourth child and its unlucky successors are denied rations and their mother is deprived of maternity benefits. « Government food and clothing allowance usually granted at the birth of a child are being withheld for a fourth offspring » (New York Times, April 27, 1966). Late marriage, contraception, abortion, sterilisation, and the repressive measures explained above have now become the latest hallmarks of Socialist virtue in Communist China. The salient feature of this resurrected birth control programme is that this time Peking has avoided publicizing the details of it abroad, possibly to spare embarassment for the leadership. The basic motive for the campaign is summarized by Edwin P. Jones as follows [32]:

> One of the major tasks of the Party's indoctrination apparatus is to propagate the notion of a twochild family as a confortable ideal, a three-child family as the maximum, and a four-child family as an irresponsible excess justifying extreme pressure upon the parents to submit to sterilisation.

With these measures, China's population policy seems to have come full circle. The Lop Nor feat in November 1964 and the present campaign to control the country's population confirm the hypothesis that China's policy in this regard has been affected by international situations and the Communist regime's obsession with making its presence felt in the world. In the phase unfolding now, Peking seems to be seriously aiming at a large-scale fertility management programme. As Chou En-lai said in an interview with Edgar Snow, « We have sent people to Japan to study means and results of population control policy there. Our present target is to reduce population growth to below 2%; for the future we aim at an even lower rate » (New York Times, February 3, 1964).

Some observers have tried to link up this reversion to population control in the fourth phase with the deterioration of the food situation in the disastrous years of 1959, 1960 and 1961; it was in these years that the word « famine », which seemed to have taken a permanent holiday in China, again made its appearance in the Chinese press [33]. Yet despite these setbacks on the food front, the official population policy at the time clung desperately to the remnants of a pro-births outlook, and the new theory of « more hands » gained prominence over the old one which saw men only as « mouths to be fed ». Precisely at this point, in April 1960, Ma Yin-Ch'u, President of Peking University, was dismissed for his neo-Malthusian ideas on population and food supply.

The official population policy of the period was hedged about with paradoxes. While crops were failing everywhere, the « we-need-more-people » campaign was nevertheless being intensified. People on the mainland were dying of starvation and calls went out to hunt wild animals and eat plants to supplement the food supply. Yet the regime was exporting food grains in order to sustain its prestige in the international arena. Therefore the anxiety about the food shortage could not have been the reason for the reversal, which may have stemmed instead from a fundamental reorientation of the official view on international developments.

In 1963 and 1964 the food outlook was brighter, but it was at this time that the birth control campaign was heightened. Why did a bettered food situation lead to the re-adoption of a campaign to control fertility? If the independent variable in China's population policy *had* been food, the campaign, if it *had* existed, would if anything have been diminished in 1963 and 1964. Instead, the reversal in policy gained currency at a time when the years of hardship were already over.

The Chinese leaders claim, significantly, that:

> There is a distinction in substance between planned child bearing and the birth control or sterilisation advocated by the Stoics. We are not overpopulated; on the contrary, we need more people. Ours is a vast country, more labouring heroes are needed to construct it into a richer and stronger country. In our country, men are most precious. It is just because of this that it becomes more important to improve the physique of the next generation through planned child bearing [34].

This is a remarkable exercise in philosophical rationalization, and with it Peking is now trying to abandon its previous policy of encouraging births and strongly endorse planned parenthood. While the Party theoreticians seek by tortuous logic to dissociate the new policy from Malthusianism, the propaganda line for general consumption explicitly acknowledges the dangers of too-rapid population growth.

At the moment, in the current atmosphere of fertility management, Mao's « man over weapon » theme continues to be followed reverently as the main scripture on nation-building in China. The passage below reflects this:

> While atom bombs are very powerful, spiritual bombs are a thousand times more so. By spiritual bombs we mean the just nature of our war against agression, the indignation of hundreds of millions of people... With the support of the masses of the people, we are in the superior position. We have now mastered material bombs, but the enemy will never be able to obtain spiritual atom bombs [35].

In fact, the same « man over weapons » theme was forcefully emphasized in 1964, when the Chinese people were mobilized to emulate the « Four Firsts » programme devised by Defence Minister Lin Piao as a method for building an army. One of the « Four Firsts » enunciated in the programme is « Man counts more than weapons ».

In conclusion, China's population policy is aptly illustrated by the proverb, « The bamboo bends when a storm rages over the countryside, but rises up again when calm returns ».

Delhi.

A. Lal

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[1] Medieval name for China. Encyclopaedia Britannica, 11th ed., Vol. VI, p. 116.

[2] Dean Acheson's « White Paper » of 1948 had deplored China's over-population and recommended birth control for China.

[3] cf. E. Stuart Kirby, « *China Population Problem* », Far Eastern Economic Review, Hong Kong, Vol. XXIV, No. 17, April 24, 1958.

[4] Even the importation of doctor-prescribed contraceptives was banned by the Maritime Custom Office in Canton. « Survey of China Mainland Press » (hereinafter referred to as SCMP), Hong Kong, No. 545, April 5-7, 1953.

[5] Current Scene, Hong Kong, No. 15, 1963; vol. II, No. 24. The injection of PLA (People's Liberation Army) contingents was aimed not only at diluting the native populations numerically but also at subjugating them if they happened to be in contact with foreign powers across the frontiers of Sinkiang, Inner Mongolia or Tibet.

[6] Jo Shui, « *Six Hundred Million* », Jen-min Jih-pao (herein after JMJP), Peking, August 7, 1954; translated in SCMP, No. 890, September 17, 1954.

[7] Pai Chien-hua, « *600 Million People* », JMJP November 1, 1954; translated in SCMP, No. 962, November 10-11, 1954.

[8] On March 5, 1957, for example, an article entitled « *Exercise Appropriate Birth Control* » appeared in the JMJP (translated in the SCMP No. 1487, March 12, 1957). It indicated that even as early as August 1953, the State Council had directed the Ministry of Health « to help the masses exercise birth control » and had « approved meanwhile the Ministry's revisions in the method of contraception and induced artificial abortion ». Again, Deputy Shai Li-tzu had sought to reconcile the need for population control with the Communist thesis by declaring: « Lenin pointed out that the working class had no place for neo-Malthusianism, but he also said - This in no way prevents us from drastically discarding all laws seeking to punish abortion and the propagation of the medical theories of contraception ». JMJP, September 18, 1954, translated in SCMP, November 2, 1954. Even Pai Chien-hua, whom we have quoted in the text, while speaking of the census results with an extravagant eulogy, did not fail to admit that « in an underdeveloped country rapid increase in population may cause difficulties in living ». These statements indicate that at a time when a pro-fertility policy enjoyed currency publicly, the seeds of a birth-control policy were germinating among the Peking planners.

[9] New China News Agency (herein after NCNA), No. 374, May 15, 1956.

[10] Sporadic statements disparaging nuclear weapons were to be heard even before March 1954, when the crusade was greatly intensified. In 1946, for example, in an interview with Anna Louise Strong, Mao Tse-tung is reported to have said: « Its (atom bomb) bursting over Hiroshima destroyed it. For, the people of the whole world turned against it.... In the end the bomb will not annihilate the people. The people will annihilate the bomb ».

Anna Louise Strong: Dawn Out of China. 1948. The Stockholm Peace Appeal in summer 1950 with a signature campaign demanded that such weapons be banned and their first users be designated as war criminals. Again, Shih-shih Shou-ts'e, Vol. 1, No. 2, November 5, 1950 as translated in Current Background (herein after CB) Hong Kong, speaks of nuclear weapons as « cruel weapons that should be prohibited ». Barring these statements, discussion on the impact of nuclear weapons on modern military operations and strategic concepts had virtually been a taboo based primarily on emotion - the reason being absence of nuclear power with China.

[11] K. M. PANNIKAR, In Two Chinas. 1955.

[12] *Akio Doi*, Sashimukai no. Mo Taku-to (Tete-atete with Mao Tse-tung), Kagamiruashobo, Tokyo, 1957.

[13] Mao Tse-tung, On the Protracted War - 1936.

[14] NCNA, Peking, September 20, 1956; translated in CB no. 413, October 5, 1956.

[15] TA CHEN, « *New China's Populations Census of 1953 and its Relation to National Reconstruction and Demographic Research* ». A paper presented at International Statistical Institute, Stockholm, 1957. Also in Alfred Sauvy, Fertility & Survival - Population Theories from Malthus to Mao.

[16] ROLAND PRESSAT, *La Population de la Chine et son Economic, Population*, Paris, October-December, 1958.

[17] ANNE M. JONES, *The Soviet Union and the Atom: Peaceful Sharing*, 1954-1958, RAND Memorandum RM 2290, November 1958.

[18] LI TEH CH'UAN, « *Birth Control and Planned Families* », JMJP, March 17, 1957; translated in CB, No. 445, April 5, 1957.

[19] Engel's letter to Kautsky, published in '*Study and Translation Series*', Issue No. 3, 1958, translated in Extracts from China Magazines (ECMM), May 12, 1958.

[20] Formative in the sense that birth control concepts were still subject to the apprehensions that the campaign might fail.

[21] The Times, London, July 15, 1960.

[22] « Deputy Shao Li-tzu speaks on Birth Control at National People's Congress », JMJP, September 18, 1954; translated in ECMM, No. 920, November 1954.

[23] PAN CHING-YUAN, « *Two Years of Planned Purchase & Supply of Grain* », Hsin Chien-She (New Construction), No. 9, September 1955; translated in ECMM, No. 9, October 10, 1955.

[24] Writers such as Wu Shuo (ECMM, No. 85), Chen Keng-chi (SCMP, No. 1535), Wang Lin (ECMM, No. 74), Li Shu-teh (ECMM, No. 100) and Niu Chung-huang (ECMM, No. 106).

[25] NCNA, Chengchow, August 15, 1957; translated in SCMP, No. 1908, September 12, 1957.

[26] JMJP, August 28, 1957; translated in SCMP, No. 1608, September 12, 1957.

[27] cf. China News Analysis (CNA) Hong Kong, No. 431, August 3, 1962.

[28] Current Scene, Hong Kong, November 15, 1963, Vol. 2, No. 24.

[29] Lowel Thomas, Jr. The Silent War in Tibet (1959).

[30] Daily Worker, Peking, July 24 and September 4, 1962, cited by Michael Freeberne in Current Scene, Hong Kong, August 15, 1963.

[31] Southern Daily, Canton, October 23, 1962, cited by Michael Free-berne in Current Scene, Hong Kong, August 15, 1963.

[32] EDWAN P. JONES, *After the Reappraisals, Problems of Communism*, Washington, May-June, 1964, Vol. XIII, No. 3.

[33] « *Beat the Famine* », Ta-chung Jih-pao, Nov. 27, 1960. Another entry on 'famine' appeared in the December 2, 1960 issue of this publication.

[34] Canton Evening News, Canton, March 2, 1962, cited by Michael Freeberne in Current Scene, August 15, 1963.

[35] LIU YUN-CHEN, JMJP, November 18, 1964, in China Report, N. Delhi, February, 1965.

CRONACHE - REPORTS

Esperimenti sull'uomo

Negli ultimi tempi hanno destato scandalo in Italia le notizie di esperimenti medici compiuti non su animali da laboratorio, ma su esseri umani.

I fatti che oggi - giustamente, del resto, - vengono denunciati come riprovevoli e suscitano indignazione erano noti da lungo tempo, se non al vasto pubblico almeno ad un numero non certo esiguo di iniziati. Degli esperimenti compiuti, infatti, restano non tracce imprecise, ma documenti esaurienti e non sospetti nelle relazioni che spesso vengono pubblicate o sono oggetto di discussioni in Congressi scientifici. Valga il caso, rivelato poche settimane fa, di una clinica universitaria romana nella quale, per fini sperimentali, venne provocata una temporanea paralisi a venti donne: l'episodio, in realtà, era ampiamente documentato in una relazione apparsa undici anni or sono sul bollettino di una società scientifica. Non ci si potrà, comunque, dolere dello scandalo e dell'indignazione, se valgono a proporre un problema che è da presumere abbia tormentato più di una coscienza prima di trovare così larga eco.

Già quattro anni fa un medico inglese, M. H. Pappworth, aveva raccolto una vasta documentazione in una coraggiosa opera di denuncia che soltanto ora appare in italiano (M. H. Pappworth, « Cavie umane, la sperimentazione sull'uomo », Feltrinelli, Editore, Milano).

« Nel loro ardente desiderio di estendere le frontiere della medicina - scrive il Pappworth - molti medici sembrano essersi dimenticati, almeno temporaneamente, che i soggetti usati per i loro esperimenti sono pur sempre persone dotate di inalienabili diritti e, nella maggior parte dei casi, malati che sperano di essere guariti ».

Citeremo a caso dal libro: « Nel 1954 venne compiuto un esperimento di cateterismo cardiaco su sedici ragazzi di età compresa fra gli undici e i sedici anni e su un giovane di vent'anni. L'esperimento consisteva nel far passare un catetere, sotto controllo radiologico, attraverso la vena principale del braccio facendolo proseguire attraverso la vena succlavia del torace, la vena cava superiore, l'atrio destro del cuore ed introducendolo infine nell'arteria polmonare. Contemporaneamente venivano applicati sul viso del ragazzo un'aderente maschera facciale e uno stringinaso in modo da poter prelevare, per analizzarli, dei campioni di aria respirata. Dopo essere stati prelevati dei campioni di sangue dal cuore (attraverso il catetere) e dopo essere state ottenute delle registrazioni della pressione interna del cuore, i ragazzi dovevano pedalare (con il catetere cardiaco e la maschera facciale sempre in posizione) ».

« L'esperimento - aggiunge il Pappworth - portò a formulare la seguente conclusione: le carditi reumatiche attive interferiscono con l'efficienza funzionale del cuore. Questo fatto, tuttavia, era già noto e chiaramente provato. Scopo dell'esperimento era stato quindi, come accade spesso nella medicina sperimentale, la semplice conferma di una conoscenza già acquisita ».

Il libro del Pappworth, che nell'edizione italiana è preceduto da una polemica prefazione del Prof. Giulio A. Maccacaro, raccoglie una casistica amplissima: l'esempio che abbiamo citato non è che uno dei moltissimi riferiti, e non il più significativo.

A leggere queste pagine si impara come il corpo umano possa essere trafitto, inciso, esplorato come puro oggetto; come il malato (ma spesso si tratta di vecchi, di bambini, di carcerati) possa divenire puro sussidio tecnico dello sperimentatore.

Poichè, si badi, le sofferenze e i rischi - talvolta molto gravi - ai quali viene sottoposto non servono alla sua guarigione, ma ad altri fini che, nobili e importanti quanto si voglia, sono a lui totalmente estranei. Non è facile, oggi, ammettere che la necessità di acquisire nuove conoscenze, che il progresso della ricerca medica, possano giustificare non soltanto il dolore fisico inflitto ai soggetti dell'esperimento, ma, soprattutto, la riduzione di essi ad un mortificante ruolo strumentale. Non ci soffermeremo, qui, a considerare altri aspetti di questa amara realtà, come il fatto che taluni esperimenti vengano compiuti a beneficio di industrie farmaceutiche, ciò che porta ad una ripugnante mercificazione del paziente; o che sul medico ricercatore agiscano non solo gli stimoli del puro desiderio di sapere ma anche quelli - meno disinteressati - della personale ambizione.

Si può facilmente ammettere, infatti, che tutti i casi di tal genere rappresentino individuali deviazioni dalle norme etiche correnti e che quindi - per frequenti che siano - abbiano carattere di eccezionalità. Il nucleo fondamentale del problema sta nella subordinazione, istituzionalmente accettata, del malato allo sperimentatore. Questo mortificante stato di cose potrà mutare soltanto con il mutare delle istituzioni che lo tengono in vita e con l'evolversi della legislazione che lo legittima.

Non sarà molto tuttavia, se ancora resterà viva, perchè legata intimamente ad una civiltà e ad una cultura, l'idea che sta a fondamento della sperimentazione medica sull'uomo: l'idea, cioè, di una scienza dissociata da ogni valore umano ed assolta da ogni obbligo, da ogni responsabilità. Sopravvissuta ad altre età, questa concezione viene

spesso invocata per porre l'uomo di studio al riparo da tormentose perplessità: essa fa della scienza una sorta di divinità impietosa che esige riti cruenti; dello scienziato un sacerdote devoto ai suoi compiti separato dalla comune umanità.

Experiments on Man

In recent years there has been much outcry in Italy at the news of medical experiments performed not on animals but on human beings.

The facts which — rightly, indeed — are today denounced as reprehensible and arouse indignation had been known for a long time, if not by the general public at least by not a few initiates. In fact, of the experiments performed, there remain not imprecise traces but exhaustive documents, unsuspected in the reports that are often published or are the subject of discussions in scientific Congresses. Mention may be made of the case, revealed a few weeks ago, of a hospital of Rome University in which, for experimental purposes, a temporary paralysis was produced in twenty women: actually, the episode was fully documented in a report that appeared eleven years ago in the bulletin of a scientific society. However, the outcry and indignation cannot be regretted if they bring up a problem which, it is to be presumed, has tormented more than one conscience before attracting such wide attention.

Even four years ago an English doctor, M. H. Pappworth, had assembled a vast number of documents in a courageous work of denunciation that has only just appeared in Italian (M. H. Pappworth, «Cavie umane, la sperimentazione sull'uomo», Feltrinelli Editore, Milan).

«In their keen desire to extend the frontiers of medicine - writes Pappworth - many doctors seem to have forgotten, at least temporarily, that the subjects used for their experiments are still individuals having inalienable rights and, in most cases, are sick persons hoping to be cured».

We will quote at random from the book: «In 1954 a cardiac catheterism experiment was performed on sixteen children between the ages of eleven and sixteen and on a youth of twenty. The experiment consisted in passing a catheter, under radiological control, through the principal vein of the arm, then through the subclavial vein of the thorax, the upper caval vein, the right atrium of the heart and finally into the pulmonary artery. At the same time the child's face was covered by a close-fitting mask and a nose clamp, so as to be able to remove samples of air breathed, for analysis. After blood samples had been removed from the heart (through the catheter) and after recordings had been obtained of the internal heart pressure, the children had to bicycle (with the cardiac catheter and the face mask still in position)».

«The experiment — Pappworth adds — led to the following conclusion: active rheumatic carditis interferes with the working efficiency of the heart. However, this fact was already known and clearly proved. Hence, as is often the case in experimental medicine, the purpose of the experiment had merely been to confirm an already known fact».

Pappworth's book, which in the Italian edition contains a controversial preface by Prof. Giulio A. Maccacaro, comprises a vast number of case reports: the example we have cited is merely one of the large number reported, and not the most significant one.

Reading these pages, one learns how the human body can be pierced, cut and explored as a mere object; how the sick person (though often the subjects are old people, children or prisoners) can become a mere technical aid to the experimenter.

For, it may be observed, the sufferings and risks — in some cases very serious — to which he is subjected do not serve to cure him but are intended for other purposes which, noble and important as they may be, are totally extraneous to him. It is not easy, today, to assert that the necessity of acquiring new knowledge, or the progress of medical research, can justify not only the physical pain inflicted upon the subjects of the experiment but, above all, the fact of reducing them to the mortifying role of instruments. We will not dwell, here, upon other aspects of this bitter reality, such as the fact that certain experiments are performed for the benefit of pharmaceutical industries — which leads to a repugnant mercature of the patient — or the fact that the medical research worker is prompted not only by stimuli for a pure desire of knowledge but also by the less disinterested stimuli of personal ambition.

In fact, it may readily be admitted that all cases of this kind represent individual deviations from the current ethical rules and that, however frequent they may be, they are of an exceptional nature. The heart of the problem lies in the institutionally accepted subordination of the patient to the experimenter. This mortifying state of affairs can only be changed by a change in the institutions that sustain it and by the evolution of the legislation that makes it legitimate.

But this change will not amount to much as long as there still flourishes — because it is intimately bound up with a civilisation and a culture — the idea that lies at the root of medical experiments on man: namely, the idea of a science dissociated from every human value and absolved from all obligation and responsibility. A survival from other ages, this conception is often invoked to shelter the research worker from tormenting perplexities: it makes science into a sort of merciless divinity that exacts bloody rites and the scientist into a high priest, wholly dedicated to his tasks and cut off from common humanity.

I premi dell'Accademia dei Lincei

L'Accademia Nazionale dei Lincei ha tenuto a Roma, il 18 giugno, una solenne adunanza pubblica per la chiusura dell'anno accademico 1970-1971. Nel corso della seduta sono stati anche conferiti il premio nazionale del presidente della Repubblica, i due premi del Ministro della pubblica istruzione e numerosi altri premi e borse di studio.

Il presidente dell'Accademia, Prof. Beniamino Segre, ha illustrato in un'ampia relazione l'intensa attività svolta dall'Accademia nel corso dell'anno ed ha proclamato i vincitori dei premi.

Il premio nazionale del presidente della Repubblica è stato assegnato al Prof. Claudio Barigozzi, ordinario di genetica nell'Università di Milano e socio corrispondente dell'Accademia Nazionale dei Lincei, per le sue ricerche nei campi della citologia e della genetica, e in particolare per le indagini sui processi mitotici e meiotici e sui modi di formazione di razze e sottospecie del crostaceo *Artemia salina*, per poliploidia e partenogenesi; sulla funzione dell'eterocromatina nella determinazione dei caratteri quantitativi in *Drosophila*; sulla genetica e la fisiologia dello sviluppo dei tumori melanotici nello stesso organismo; sull'indagine citogenetica per mezzo della coltura di cellule di drosofila; oltre che per l'opera appassionata e illuminata dedicata alla creazione e allo sviluppo di una scuola di genetica nella facoltà di scienze dell'Università di Milano.

Il premio del Ministro della pubblica istruzione per la chimica è stato conferito al Prof. Guido Sartori, ordinario di chimica generale nell'Università di Roma e socio corrispondente dell'Accademia dei Lincei, il quale ha sviluppato la sua attività nel campo della chimica generale moderna portando, insieme ai suoi allievi, contributi di primo piano alle nuove concezioni che stanno trasformando struttura e contenuto di questa disciplina. In particolare, il Sartori si è dedicato allo studio dei composti di coordinazione e metallorganici ove, impiegando le più avanzate metodologie sia sperimentali che teoriche, ha risolto importanti problemi di struttura, stabilità e reattività.

In quest'ultimo periodo il Sartori e la sua scuola hanno affrontato, fra i primi, lo studio della emissione luminescente dei composti di coordinazione. I risultati ottenuti costituiscono una acquisizione recentissima della chimica moderna, in quanto hanno permesso di caratterizzare gli stati eccitati come specie chimiche a sè stanti, con questo aprendo nuove vie alla ricerca fondamentale chimica a seguito dei nuovi significati ad essa apportati.

Il premio del Ministro della pubblica istruzione per la critica dell'arte e della poesia è stato attribuito al Prof. Giovanni Macchia, ordinario di letteratura francese nell'Università di Roma. La motivazione definisce Giovanni Macchia «il più autorevole dei francesisti italiani, critico acuto ed informatissimo, saggista di non comune eleganza, interprete sempre originale di fatti e problemi di poesia e di cultura, con curiosità non soltanto letterarie, ma estese alle arti figurative, alla musica, soprattutto al teatro». «La sua vasta operosità - prosegue la motivazione - ha trovato per ora un degno coronamento nella 'Storia della letteratura francese' (dalle origini alla fine del '600), sintesi ricca di nuove prospettive e di giudizi sempre personali e spesso suggestivi».

Il premio linceo per l'archeologia è stato attribuito ex-aequo al Prof. Luigi Bernabò Brea, soprintendente alle antichità della Sicilia, e al Prof. Mario Napoli, soprintendente alle antichità di Salerno e Professore universitario nella stessa sede.

Gli otto premi del Ministero della Pubblica Istruzione, di L. 250.000 ciascuno, sono stati così assegnati: per le scienze filosofiche al Prof. Francesco Sirchia; per le scienze economiche, sociali e politiche, al Prof. Adriano Bonelli; per l'archeologia, storia e geografia antica al Prof. Carmelo Turano; per la storia medioevale e moderna, scienze ausiliarie e geografia antropica al Prof. P. Giovanni Baroni; per le scienze giuridiche al Prof. Giampaolo Bon; per la filologia e linguistica del mondo antico al Prof. Giovanni Tezzo; per la filologia e linguistica del mondo moderno al Prof. Giorgio Varanini; per la storia e critica dell'arte e della poesia alla Prof.ssa Mina Bacci.

Successivamente sono stati proclamati i nomi dei vincitori dei premi «Antonio Feltrinelli» che verranno consegnati nel corso di una cerimonia che avrà luogo nel prossimo autunno. Il premio internazionale di 20 milioni di lire per la matematica e applicazioni è stato assegnato al Prof. Jean Leray, mentre al Prof. Bruno Rossi è stato attribuito il premio internazionale, anch'esso di 20 milioni, destinato a un cultore di discipline fisiche e astrofisiche la cui opera abbia portato un contributo decisivo allo sviluppo della concezione moderna dell'universo e della sua struttura.

Nato a Nantes nel 1906, Jean Leray fu dal 1926 al 1929 allievo dell'Ecole Normale Supérieure; nel 1933 conseguì il dottorato di scienze all'Università di Parigi. È stato professore alle Università di Nancy (1938) e di Parigi (1943), dove insegnò rispettivamente matematica applicata e meccanica dei fluidi. Dal 1947 è professore di teoria delle equazioni differenziali e funzioni al Collège de France. È membro della Académie des Sciences de Paris e di molte altre accademie e istituzioni scientifiche, dottore honoris causa dell'Università di Chicago.

L'Accademia dei Lincei, nel motivare l'attribuzione del premio, rileva: «La sua produzione scientifica possiede caratteri di eccezionale profondità, ma ciò che più colpisce è che essa, pur nella sua multiformità, risponde a un disegno rigorosamente unitario.

« Oggi che tanto si parla delle ricerche interdisciplinari appare quasi un luogo comune il sottolineare la reciproca influenza che la matematica e la fisica hanno esercitato l'una sull'altra nei loro sviluppi degli ultimi due secoli.

« La fisica pone dei problemi, la matematica nello sforzo di risolverli va molto spesso assai al di là di questo scopo iniziale ed è portata a considerare questioni più generali e il cui carattere astratto sembra in un primo momento assai lontano da ogni possibilità di applicazioni. Ma alcuni dei risultati teorici che così si ottengono si rivelano a distanza di tempo capaci di contribuire alla risoluzione o addirittura all'impostazione di nuove questioni di carattere fisico. Questo aspetto dell'interazione fra matematica e fisica si presenta quasi sempre come opera collettiva di più persone realizzata in generale durante un lungo arco di tempo. Sono invece assai rari i casi di matematici che nel corso della loro vita abbiano realizzato da soli un complesso di ricerche e di risultati in cui sia possibile ravvisare tutti gli aspetti del processo di evoluzione più sopra descritto e che per la loro importanza segnino una tappa miliare nello sviluppo di qualche teoria matematica. Uno di questi matematici è senza dubbio Jean Leray.

« È infatti fin dalle sue prime famose ricerche nel campo dell'idrodinamica che Leray fu portato a riconoscere la necessità di dare una nuova sistemazione generale ai metodi per lo studio dei problemi non lineari. E nello stesso torno di tempo in cui, per rispondere alla stessa esigenza, Caccioppoli approfondiva la teoria dell'inversione delle corrispondenze funzionali, il Leray, in collaborazione con J. Schauder, pubblicò la sua celebre memoria « Topologie et équations fonctionnelles », che è stata certamente un punto di riferimento di fondamentale importanza per tutta una generazione di analisti. Il proposito di dare ai metodi di questa memoria una più ampia validità condussero più tardi il Leray a introdurre nuovi e importanti metodi e concetti nel campo della topologia algebrica, determinando fra il 1945 e il 1950 una vera svolta di questa teoria, la cui forte influenza in vari campi della matematica continuerà prevedibilmente per molti anni ancora.

« Da uno dei mezzi messi in opera per ottenere tali risultati il Leray trasse poi lo spunto per le sue prime ricerche sulla teoria delle equazioni iperboliche, il cui approfondimento occupa gran parte della sua attività dal 1951 fino ad oggi. In questo campo egli ha ottenuto una messe di risultati, alcuni dei quali sono suscettibili di notevoli applicazioni all'ottica geometrica e alla teoria dei quanta, mentre altri più recenti sono suggeriti da questioni di magnetoidrodinamica relativista.

« L'opera scientifica di Jean Leray nata dallo studio di questioni di fisica matematica, torna attraverso una lunga evoluzione ad altre questioni di fisica matematica, realizzando così una mirabile sintesi fra teoria e applicazioni.

« Per questo particolare carattere della sua opera, per la grande influenza che essa ha esercitato sui progressi della matematica, per l'importanza dei risultati da lui ottenuti, l'Accademia ha deliberato di conferire a Jean Leray il premio internazionale « Antonio Feltrinelli » per la matematica e applicazioni ».

Per quanto riguarda il secondo dei due premi, l'Accademia, nell'illustrare i motivi della sua scelta, dichiara: « Il premio internazionale «A. Feltrinelli» è stato attribuito a Bruno Rossi in riconoscimento del contributo fondamentale dato da lui e dalla sua scuola alla nuova visione del cosmo che si viene formando in questi ultimi anni, grazie alle scoperte straordinarie dell'astrofisica delle alte energie. Formatosi alla scuola di Bologna, dopo un breve periodo nelle università di Firenze e di Padova, Rossi dovette emigrare negli Stati Uniti poco prima dello scoppio della guerra, insieme con Enrico Fermi e vari altri valorosi fisici italiani. Ivi egli continuò le sue ricerche sui raggi cosmici concentrando la sua attività sui cosiddetti grandi sciami; si deve a lui la scoperta che gli sciami grandissimi sono prodotti da singole particelle nelle quali è concentrata una quantità così grande di energia che esse non possono essere trattenute da nessuna forza conosciuta all'interno di un sistema stellare; esse perciò si muovono negli spazi tra una galassia e l'altra fino al momento in cui sono catturate, come accade per esempio nella nostra atmosfera. Iniziatisi gli esperimenti spaziali, Rossi vide immediatamente le enormi possibilità offerte da queste nuove tecnologie e convinse gli organi dirigenti della NASA a includere nel programma americano lo studio del plasma interplanetario. L'esperienza eseguita da Rossi e dai suoi collaboratori sul satellite Explorer X, fu la prima in questo campo a mettere in evidenza le caratteristiche fondamentali del cosiddetto vento solare, che è uno dei processi più importanti per la fisica dello spazio interplanetario e presumibilmente dello spazio nella vicinanza immediata di ogni stella. Contemporaneamente a queste ricerche Rossi rivolgeva la sua attenzione alla possibilità di rivelare raggi X e gamma provenienti da sorgenti celesti diverse dal sole. L'esperimento ideato e realizzato da lui e dalla sua scuola nel giugno 1962 segna la data di nascita di una nuova astronomia, quella dei raggi X non solari. Le scoperte derivate da queste nuove ricerche, riallacciandosi a quelle ottenute dalla radioastronomia, sono così fondamentali che tutta la cosmologia e forse anche la cosiddetta fisica di base - cioè la fisica delle grandi leggi universali - ne risulta profondamente modificata. La possibilità di riallacciare in un unico grande processo cosmologico la produzione di particelle di altissima energia e di flussi di fotoni energetici dà così all'opera di Rossi una profonda unità ideale.

« L'Accademia dei Lincei onora in lui uno scienziato tra i maggiori viventi per la visione ampia e profonda dei problemi affrontati, un maestro che ha saputo suscitare una scuola vivace e brillante, aprendo così all'indagine scientifica nuovi orizzonti di incalcolabile vastità ».

I cinque premi « Antonio Feltrinelli » riservati a cittadini italiani, di lire 5.000.000 ciascuno, sono stati così assegnati: per la matematica, meccanica e applicazioni al Prof. Aldo Andreotti; per l'astronomia, geodesia, geofisica e applicazioni al Prof. Giuseppe Colombo; per la fisica, chimica e applicazioni al Prof. Adriano Gozzini; per la geologia, paleontologia, mineralogia e applicazioni al Prof. Bruno Accordi; per le scienze biologiche e applicazioni al Prof. Pasquale Pasquini.

Sono stati inoltre conferiti numerosi altri premi: premio medaglia d'oro « Stanislao Cannizzaro » all'Ing. Prof. Duilio Arigoni; premio « Battista Grassi », di L. 500.000, per la zoologia, parassitologia e talassografia biologica al Dott. Paolo Brignoli; premio « Luigi D'Amato », di L. 300.000, per la medicina interna al Dott. Ettore Savagnone; premio « Camillo Golgi », di L. 1.000.000, destinato a lavori di anatomia del sistema nervoso al Prof. Ennio Pannese; premio « Dott. Giuseppe Borgia », di L. 1.000.000, destinato a scoperte o invenzioni scientifiche degne di premio fra quelle verificatesi nel quinquennio precedente, al Prof. Giovanni Magni; premio internazionale « Eugenio Morelli », di L. 2.000.000, destinato a studi di pneumologia comprendendo in tale disciplina, oltre la parte di scienza pura, la clinica medica e chirurgica, al Prof. Emilio Agostoni; premio internazionale « Guido Lenghi », di L. 2.000.000, destinato a studi di virologia biologica o clinica, al Prof. Robert J. Huebner; premio internazionale « Giovanni Di Guglielmo », di L. 1.000.000, destinato a studi riguardanti le malattie eritremiche ed eritroleucemiche, al Dott. Giuliano Spremolla; premio « Roberto Almagià della Società di Studi Geografici », di L. 1.000.000, destinato ad un'opera di geografia o di storia della geografia o della cartografia, al Prof. Orlando Ribeiro.

È stato anche proclamato il nome del vincitore del premio « Carlo Erba alla memoria di Gianni Pauletta », di L. 1.000.000, per il tema « Nuove tecniche diagnostiche e terapeutiche in oncologia », assegnato alla Prof.ssa Luisa Massimo.

L'Accademia ha anche attribuito nel corso dell'anno due borse di studio « Giorgio Levi Della Vida », di L. 1.800.000 ciascuna, per il perfezionamento nelle discipline semitiche e arabo-islamiche ai Dott. Giovanna Calasso e Sandro Filippo Bondì.

Sono state inoltre conferite dalla Fondazione « Guido Donegani » otto borse di perfezionamento post-universitario negli studi di chimica, di L. 1.200.000 ciascuna.

Successivamente l'accademico Prof. Luigi Califano ha parlato sul tema: « Aspetti attuali della ricerca sulla cancerogenesi ».

Il Prof. Califano ha esordito ricordando che il problema della cancerogenesi costituisce, nel momento attuale, uno dei campi di maggiore interesse in biologia ed in medicina e studiosi di varia preparazione nelle nazioni culturalmente più evolute si dedicano ad esso.

Dopo la scoperta effettuata nel 1911 da Rous, nell'Istituto Rockefeller di New York di un virus filtrabile quale agente causale di un tumore del pollo, numerosi altri tumori di animali sono stati trovati, dai quali è stato separato un virus filtrabile. In questi ultimi anni progressi enormi sono stati compiuti ed al presente si distinguono tumori da virus ad acido ribonucleico (RNA) e tumori di virus ad acido desossiribosio (DNA). Le infezioni delle cellule coltivate in vitro con virus a DNA possono avverarsi in due maniere cioè come infezione produttiva con produzione di nuovo virus e morte della cellula ed infezione abortiva che determina non la morte della cellula, ma la trasformazione neoplastica di essa. Nelle cellule trasformate viene trasmessa dalla cellula madre alla cellula figlia tutto o una parte del genoma virale, si determina, cioè, quella che si dice infezione verticale.

I virus a RNA che appaiono come particolarmente interessanti anche per i tumori dell'uomo possono andare incontro ai due tipi di infezione, ma possono anche albergare il virus completo come endosimbiante, cioè convivente con la cellula ospite.

Sulla base di tale fatto è emerso il concetto della trasmissione di genoma virale attraverso le cellule germinali. Ciò non significa che il cancro sia ereditabile in senso tradizionale, ma significa che, potenzialmente, esiste nell'organismo, probabilmente anche in quello umano, la causa del cancro, cioè il virus oncogeno. Esso è, generalmente, allo stato inattivo e viene attivato per cause varie: chimiche, fisiche o biologiche. È in uso indicare con la espressione di « switched off » la condizione di inattività del virus e con quella di « switched on » quella di attività.

Come che sia - ha concluso il Prof. Califano - la eziologia virale dei tumori appare, al momento presente, la più probabile. Il fervore di studi in tal senso è in fase di alto aumento ed è prevedibile che nei prossimi anni la eziologia dei tumori - compresa quella dei tumori umani - sarà del tutto chiarita.

Prizes Awarded by the Lincei Academy

On June 18th, the Accademia Nazionale dei Lincei held, in Rome, a formal public meeting for the closing of the academic year 1970-1971. During the course of the session, the national prize of the President of the Republic was awarded, as well as the two prizes of the Minister of Education and numerous other prizes and scholarships.

The President of the Academy, Prof. Beniamino Segre, delivered a full

report on the intense activity of the Academy during the year and announced the prize-winners.

The national prize of the President of the Republic was awarded to Prof. Claudio Barigozzi, lecturer in genetics at Milan University and corresponding member of the Lincei Academy, for his studies in the fields of cytology and genetics, and in particular for his research on the mitotic and meiotic processes and on the ways of formation of breeds and subspecies of the crustacean *Artemia salina*, through polyploidy and parthenogenesis; on the function of heterochromatin in determining the quantitative characteristics in *Drosophila;* on the genetics and physiology of the development of melanotic tumours in the same organism; and on cytogenetic investigation by means of Drosophila cell cultures; as well as for his devoted and enlightened work for the creation and development of a school of genetics in the science faculty of Milan University.

The prize of the Minister of Education for chemistry was awarded to Prof. Guido Sartori, lecturer in general chemistry at Rome University and corresponding member of the Lincei Academy, whose activity has been carried out in the field of modern general chemistry and who, together with his pupils, have made contributions of prime importance to the new conceptions that are transforming the structure and content of this discipline. In particular, Sartori has devoted his attention to the study of the co-ordination and metallorganic compounds, in which field, by employing the most advanced experimental and theoretical methods, he has solved important problems of structure, stability and reactivity.

Recently, Sartori and his school have approached the study of the luminescent emission of the co-ordination compounds. The results obtained constitute a very recent discovery in modern chemistry, since they have enabled the excited states to be characterised as chemical species in themselves, thus opening up new paths in fundamental chemical research as a result of the new meanings brought to it.

The prize of the Minister of Education for art criticism and poetry was awarded to Prof. Giovanni Macchia, lecturer in French literature at Rome University. In the accompanying citation, Giovanni Macchia is described as «the most authoritative of the Italian French scholars, a keen and highly-knowledgeable critic, an essayist of uncommon elegance and an ever original interpreter of facts and problems in poetry and culture, whose curiosity extends not only to literary subjects but also to the figurative arts, music and especially the theatre». «His vast work of study — continues the citation — has now been worthily crowned by the completion of his 'History of French literature' (from its origins up to the end of the 17th century), a synthesis rich in new approaches and opinions that are always personal and often impressive».

The Lincei prize for archaeology was awarded *ex aequo* to Prof. Luigi Bernabò Brea, superintendent of antiquities for Sicily, and Prof. Mario Napoli, superintendent of antiquities for Salerno and professor at Salerno University.

The eight prizes of the Minister of Education, of a value of Lire 250,000 each, were awarded as follows: for the philosophical sciences, to Prof. Francesco Sirchia; for the economic, social and political sciences, to Prof. Adriano Bonelli; for archaeology, history and ancient geography, to Prof. Carmelo Turano; for medieval and modern history, auxiliary sciences and anthropic geography, to Prof. P. Giovanni Baroni; for the juridical sciences, to Prof. Giampaolo

Bon; for the philology and linguistics of the ancient world, to Prof. Giovanni Tezzo; for the philology and linguistics of the modern world, to Prof. Giorgio Varanini; and for the history and criticism of art and poetry, to Prof. Mina Bacci.

There followed an announcement of the names of the winners of the «Antonio Feltrinelli» prizes, which will be awarded during the course of a ceremony to take place next autumn. The international prize of 20 million lire for mathematics and its applications was awarded to Prof. Jean Leray, while Prof. Bruno Rossi was awarded the international prize, also of 20 million lire, intended for a researcher in the field of the physical and astrophysical disciplines whose work has made a decisive contribution to the development of the modern conception of the universe and its structure.

Born at Nantes in 1906, Jean Leray attended the Ecole Normale Supérieure from 1926 to 1929; in 1933 he obtained a Doctorate of Science at Paris University. He was lecturer at the universities of Nancy (1938) and Paris (1943), where he taught applied mathematics and the mechanics of fluids, respectively. Since 1947 he has been a lecturer in the theory of functions and differential equations at the Collège de France. He is a member of the Académie des Sciences de Paris and of many other scientific academies and institutions and holds an honorary Doctorate of Chicago University.

In the citation accompanying the award of the prize, the Lincei Academy stated: «His scientific production is of an exceptionally profound nature, but what is most striking is that, in spite of its multiform character, it all conforms to a strictly unitary design.

«Today, when there is so much talk of inter-disciplinary studies, it seems almost commonplace to emphasise the mutual influence that mathematics and physics have exercised upon each other in their developments during the last two centuries.

«Physics poses problems; mathematics, in an effort to solve them, very often goes far beyond this initial purpose and is led to consider more general questions, the abstract character of which at first seems very far removed from any possibility of applications. But, at a distance of time, some of the theoretical results thus obtained are seen to be capable of contributing to the solution — or even to the formulation — of new questions of a physical nature. This aspect of the interaction between mathematics and physics nearly always appears as the collective work of several persons, generally carried out over a long period of time. In fact, there are very few cases of mathematicians who, in the course of their lives, have carried out, on their own, a whole complex of studies and have achieved results in which it is possible to perceive all the aspects of the process of evolution described above and which, through their importance, constitute a milestone in the development of some mathematical theory. One of these mathematicians is undoubtedly Jean Leray.

«In fact, from the time of his first famous studies in the field of hydrodynamics, Leray was led to recognise the necessity of undertaking a new general systemisation of the methods for studying non-linear problems. And in the same period in which, to fulfil the same need, Caccioppoli was making a more thorough study of the theory of the inversion of functional correspondences, Leray, in collaboration with J. Schauder, published his famous report «Topologie et équations fonctionnelles», which has certainly been a reference point of fundamental importance for a whole generation of analysts. With the object of extending the validity of the methods described in this report, Leray was later led to introduce new and important methods and concepts in the field of algebraic topology, thus causing, between 1945 and 1950, a radical change in this theory, the strong influence of which, in various fields of mathematics, may be expected to continue for many years to come.

«It was one of the methods employed for obtaining these results that later prompted Leray to carry out his early studies on the theory of hyperbolic equations, the further pursuit of which has engaged most of his activity since 1951. In this field he has obtained a vast quantity of results, some of which are capable of extensive applications to geometrical optics and the quantum theory, while other more recent results are suggested by questions of relativistic magneto-hydrodynamics.

«Jean Leray's scientific work, which originated from the study of questions of mathematical physics, returns, through a long evolution, to other questions of mathematical physics, thus achieving a wonderful synthesis between theory and applications.

«On account of this particular character of his work, the great influence that it has exercised on the progress of mathematics and the importance of the results that he has obtained, the Academy has decided to award Jean Leray the «Antonio Feltrinelli» international prize for mathematics and its applications».

As regards the award of the second of the two prizes, in giving the reasons for its choice, the Academy stated: «The 'A. Feltrinelli' international prize has been awarded to Bruno Rossi in recognition of the fundamental contribution made by him and his school to the new view of the cosmos that has been taking shape during these last few years, thanks to the extraordinary discoveries of high-energy astrophysics. After his schooling in Bologna and then a brief period at the universities of Florence and Padua, Rossi had to emigrate to the United States just before the outbreak of the war, together with Enrico Fermi and various other bold Italian physicists. There he continued his studies on cosmic rays, concentrating his activity upon the so-called great swarms; to him is due the discovery that the greatest swarms are produced by individual particles in which such a large quantity of energy is concentrated that they cannot be restrained by any force known inside a stellar system, so that they move about in the spaces between one galaxy and another until they are captured, as is the case, for example, with our own atmosphere. When experiments in space began, Rossi immediately saw the enormous possibilities offered by these new technologies and he persuaded the NASA executive authorities to include the study of interplanetary plasma in the American programme. The experiment that Rossi and his collaborators carried out on the Explorer X satellite was the first in this field to reveal the fundamental characteristics of the so-called solar wind, which is one of the processes that is most important for the physics of interplanetary space and, presumably, of the space in the immediate vicinity of every star. Simultaneously with these studies, Rossi turned his attention towards the possibility of revealing X-rays and gamma rays originating from celestial sources other than the sun. The experiment that he and his school planned and carried out in June 1962 marks the birth of a new astronomy: that of non-solar X-rays. The discoveries resulting from these new studies, integrating and combining with those obtained from radio-astronomy, are so fundamental that the whole of cosmology and perhaps also the so-called basic physics — that is, the physics of the great universal laws — are found to be radically modified by them. The possibility of combining and integrating, in a single great cosmological process, the production of very high energy particles and that of flows of energy photons thus gives Rossi's work an ideal and deep-seated unity.

«In him, the Lincei Academy honours a scientist that is one of the greatest alive today, on account of his broad and profound view of the problems that he has approached, and a master responsible for the creation of a lively and brilliant school, thus opening up to scientific study new horizons of incalculable immensity».

The five «Antonio Feltrinelli» prizes reserved for Italian citizens, amounting to 5,000,000 lire each, were awarded as follows: for mathematics, mechanics and applications, to Prof. Aldo Andreotti; for astronomy, geodesy, geophysics and applications, to Prof. Giuseppe Colombo; for physics, chemistry and applications, to Prof. Adriano Gozzini; for geology, paleontology, mineralogy and applications, to Prof. Bruno Accordi; for biological sciences and applications, to Prof. Pasquale Pasquini.

Numerous other prizes were also awarded: the «Stanislao Cannizzaro» gold medal prize to Prof. Duilio Arigoni; the «Battista Grassi» prize, of 500,000 lire, for zoology, parassitology and biological thalassography to Dr Paolo Brignoli; the «Luigi D'Amato» prize, of 300,000 lire, for internal medicine, to Dr Ettore Savagnone; the «Camillo Golgi» prize, of 1,000,000 lire, for anatomical studies on the nervous system, to Prof. Ennio Pannese; the «Dott. Giuseppe Borgia» prize, of 1,000,000 lire, for scientific discoveries or inventions made during the previous five years, to Prof. Giovanni Magni; the «Eugenio Morelli» international prize, of 2,000,000 lire, for studies in pneumology (comprising not only the purely scientific side but also the medical and surgical clinical side), to Prof. Emilio Agostini; the «Guido Lenghi» international prize, of 2,000,000 lire, for studies in biological or clinical virology, to Prof. Robert J. Huebner; the «Giovanni Di Guglielmo» international prize, of 1,000,000 lire, for studies on the erythremic and erythroleukaemic diseases, to Dr Giuliano Spremolla; the «Roberto Almagià» prize of the Society of Geographic Studies, of 1,000,000 lire, for a work relating to geography or the history of geography or cartography, to Prof. Orlando Ribeiro.

An announcement was also made of the winner of the «Carlo Erba» prize «to the memory of Gianni Pauletta», of 1,000,000 lire, for «New diagnostic and therapeutic techniques in oncology», which was awarded to Prof. Luisa Massimo.

The Academy also awarded, during the course of the year, two «Giorgio Levi Della Vida» scholarships, of 1,800,000 lire each, for advanced studies in Semitic and Arabo-Islamic subjects, to Dr Giovanna Calasso and Dr Sandro Filippo Bondì.

The «Guido Donegani» Foundation also awarded eight grants for advanced post-university studies in Chemistry, of 1,200,000 lire each.

The Academy member, Prof. Luigi Califano, then spoke on the subject: «Present-day aspects of research on cancerogenesis».

Prof. Califano began by mentioning that the problem of cancerogenesis, at the present time, constitutes one of the fields of major interest in biology and in medicine and that it was being studied by experts in all the various branches in the more culturally advanced countries.

Since the discovery made by Rous in 1911, at the Rockefeller Institute in New York, of a filterable virus as the causal agent of a chicken tumour, numerous other animal tumours have been found, from which a filterable virus has been separated. In recent years, enormous progress has been made and two types of virus tumour are now distinguished: the ribonucleic (RNA) type and the deoxyribonucleic (DNA) type. Infections of cells cultivated *in vitro* with the DNA virus may appear in two ways: as a productive infection with the production of new virus and death of the cell, or as abortive infection which results not in the death of the cell but in its neoplastic transformation. In the transformed cells there is transmitted, from the mother cell to the daughter cell, all or part of the viral genome; that is, so-called vertical infection is caused.

The RNA viruses that appear particularly interesting also for human tumours may encounter the two types of infection, but they may also harbour the complete virus as endosymbiont: i.e. living together with the host cell.

From this fact has emerged the concept of viral genome transmission through the germinal cells. This does not mean that cancer can be inherited in the traditional sense, but it means that, potentially, there exists in the organism — and probably also in the human organism — the cause of the cancer: the oncogenous virus. The latter is generally in the inactive state and it is activated through various causes: chemical, physical or biological. The expression «switched off» is commonly used to indicate the condition of inactivity in the virus, and «switched on» to indicate that of activity.

However that may be — concluded Prof. Califano — at the present moment the viral etiology of tumours seems the most probable one. The already fervid activity in studies on this subject is being intensified still further and it may be expected that during the next few years the etiology of tumours — including that of human tumours — will be entirely explained.

La burocrazia e la ricerca

Il progressivo organizzarsi della ricerca scientifica in grandi strutture, se ha consentito il raggiungimento di risultati altrimenti impensabili, presenta anche aspetti negativi tutt'altro che trascurabili: primo fra tutti l'ingigantirsi degli organismi amministrativi che regolano l'attività scientifica e il moltiplicarsi delle pratiche burocratiche ad essa connesse. Una inchiesta condotta recentemente nell'Unione Sovietica ha rivelato che questo settore, che ha pura funzione sussidiaria, assorbe metà del costo globale della ricerca, sia per le retribuzioni del personale amministrativo, sia per il dispendio di tempo che impone agli studiosi. Sembra estremamente probabile che le conclusioni non sarebbero diverse in qualunque altro paese.

Più della metà degli scienziati sovietici interpellati ha dichiarato di dedicare alla ricerca soltanto una parte del proprio tempo e della propria attività: l'esame dei dati raccolti ha rivelato che il lavoro scientifico vero e proprio (esecuzione di lavori teoretici e sperimentali, raccolta e valutazione del materiale di ricerca, elaborazione di relazioni ed articoli) occupa soltanto il 61,5 per cento del tempo disponibile. In alcuni istituti i dirigenti sono costretti a dedicare almeno metà della giornata lavorativa ad attività extrascientifiche; ciò accade anche nella città della scienza di Novosibirsk. Per valutare con qualche approssimazione l'incidenza economica della dispersione, si dovrà tener presente che nell'Unione Sovietica gli studiosi impiegati in attività di ricerca sono 800.000 e che il contributo di ciascuno d'essi all'aumento della produttività nazionale viene calcolato in 50.000 rubli.

Nella relazione che illustra i risultati dell'inchiesta si fa rilevare che non è certo di nocumento il fatto che il ricercatore impegnato in un esperimento collabori al montaggio delle apparecchiature, e in tal modo contribuisca a creare gli strumenti necessari alla verifica delle proprie idee. Devono invece essere considerati come pura dispersione, e quindi come spesa improduttiva, tutti i lavori marginali di carattere tecnico e tutti gli atti di natura burocratica che il più delle volte vengono imposti allo studioso. L'esecuzione di lavori meccanici assorbe due ore di lavoro al giorno, vale a dire novanta giornate lavorative in un anno, da parte di scienziati altamente qualificati. Altra notevole perdita di tempo deriva dalla ricerca di informazioni. Si è calcolato che per evitarla si dovrebbe aumentare di centomila unità il numero dei collaboratori scientifici. Insufficiente è anche il personale tecnico:

il rapporto fra ricercatori e assistenti di laboratorio dovrebbe essere di uno a tre, mentre oggi è di uno a uno. Si è inoltre calcolato che le cure burocratiche comportino il dispendio di quasi un quarto della giornata lavorativa, mentre lievemente superiore (il 26,9 per cento) è il tempo che viene trascorso nell'attesa di visti o autorizzazioni per superare difficoltà di rifornimento.

L'inchiesta ha anche rivelato che lo spreco di tempo aumenta in ragione diretta della qualifica dello scienziato: raggiunge infatti il 16 per cento per i collaboratori con funzioni direttive e addirittura il 70 per cento per i più alti dirigenti. Avviene quindi, paradossalmente, che proprio gli studiosi più qualificati hanno meno possibilità di dedicarsi alla ricerca, perchè sopraffatti da compiti organizzativi, tecnici, burocratici.

Si è già detto che le conclusioni dell'inchiesta condotta nell'Unione Sovietica potrebbero, nella sostanza, essere riferite a qualunque altro paese, poichè dovunque l'ingrandirsi delle organizzazioni scientifiche ha determinato anche la crescita degli apparati burocratici ad esse connessi. Si dovrà, anzi, fare un'ulteriore osservazione: mentre nell'Unione Sovietica la ricerca scientifica è altamente centralizzata, altrove essa è affidata ad un gran numero di organismi pubblici e privati diversi, che talvolta agiscono in concorrenza fra loro. La dispersione di capacità e di energie e lo sperpero di tempo si vanno così moltiplicando indefinitamente, fino a costituire un ostacolo talvolta gravissimo al lavoro scientifico.

Bureaucracy and Research

Even if the progressive organisation of scientific research into large-scale structures has enabled otherwise unthinkable results to be achieved, it also presents negative aspects that are anything but negligible: first and foremost, the growth - to gigantic proportions - of the administrative organisations that regulate scientific activity and the multiplication of the bureaucratic procedures connected with it. An investigation recently carried out in the Soviet Union has shown that this sector, which has a merely subsidiary function, absorbs one half of the overall cost of research, not only for the salaries of the administrative staff but also on account of the expenditure of time that it imposes upon the experts. It seems extremely likely that the same state of affairs exists in every other country.

More than half of the Soviet scientists questioned stated that only part of their time and activity was devoted to research: examination of the data collected has shown that the actual scientific work (the execution of theoretical and experimental work, the collection and assessment of research ma-

terial, the planning and writing of reports and articles) occupies only 61.5% of the time available. In certain institutes the executives are compelled to devote at least half their working day to extra-scientific activities; this also occurs in the «science city» of Novosibirsk. In order to assess, with a fair degree of approximation, the economic effect of this dissipation, it must be borne in mind that, in the Soviet Union, there are 800,000 experts engaged in research activity and that the contribution that each of them makes to the increase in national productivity is calculated at 50,000 roubles.

In the report giving the results of the investigation, it is pointed out that there is certainly no harm in the fact that the research worker engaged in an experiment should collaborate in setting up the apparatus, and in this way help to make the instruments necessary for verifying his own ideas. On the other hand, it is impossible to consider as anything but wasteful — and hence as unproductive expenditure — all the marginal work of a technical nature and all the bureaucratic tasks that are so often imposed upon the expert. The execution of mechanical jobs takes up two hours of a working day — that is to say, ninety working days in a year — for highly skilled scientists. Another considerable waste of time results from the search for information. It has been calculated that, in order to avoid this waste, the number of scientific collaborators would have to be increased by one hundred thousand. There are also insufficient numbers of technical personnel: the ratio between research workers and laboratory assistants should be one to three, whereas at present it is one to one. It has also been calculated that bureaucratic formalities take up nearly a quarter of a working day, and a slightly greater percentage of the time (26.9%) is spent in waiting for permits or authorisations for overcoming difficulties with regard to supplies.

The investigation has also shown that the waste of time increases in direct proportion to the degree of skill of the scientist: in fact, it amounts to 16% for collaborators having administrative functions and to as much as 70% for the highest executives. Therefore this means that, paradoxically, it is just the most highly skilled scientists that have the least change of working on research, since they are overwhelmed by organisational, technical and bureaucratic tasks.

It has already been said that the conclusions resulting from the investigation carried out in the Soviet Union could, broadly speaking, be applied to any other country, since everywhere the expansion of scientific organisations has also resulted in the growth of the bureaucratic systems connected with them. Indeed, a further observation should be made: whereas in the Soviet Union scientific research is highly centralised, elsewhere it is entrusted to a large number of different public and private organisms, which sometimes compete with one another. Thus, waste of time and the dissipation of skill and energy will go on multiplying indefinitely, until they constitute an obstacle that may at times be extremely detrimental to scientific work.

Un convegno di studi su Teilhard de Chardin

Organizzato dall'Istituto Stensen di Firenze ha avuto luogo un Convegno su « Gli aspetti filosofici dell'opera di Teilhard de Chardin ».

Il Convegno era impostato su due punti: fenomenologia e dialettica. La relazione iniziale del padre N. M. Wildiers è stata fondamentale. Lo studioso belga, a cui si devono le prefazioni di diversi volumi dell'opera omnia di Teilhard[1], ha discusso il valore e la portata della prima delle due ipotesi che T.d.C. mette all'inizio de « Il fenomeno umano ». La prima è che il primato è stato attribuito allo psichico e al pensiero nella stoffa dell'universo, la seconda è quella del valore biologico attribuito al fatto sociale che ci attornia. P. Wildiers ha trattato della prima di queste due ipotesi secondo i due aspetti che essa presenta. Il primato della psiche e del pensiero umano come ipotesi di lavoro è realmente il punto iniziale delle ricerche teilhardiane che si riassumono in un tentativo di dare una spiegazione unitaria del mondo, partendo dall'uomo. Sono noti i tentativi di altri scienziati moderni per una spiegazione ugualmente unitaria, ma in base al primato della materia; ad essi egli ha opposto la sua strada e non sembra che essa sia di valore inferiore.

L'altro aspetto secondo cui padre Wildiers ha esaminato la prima delle due ipotesi di lavoro premesse a « Il fenomeno umano » è questo: supponendo compiute le imprese di T.d.C. di costruire cioè una teoria scientifica spieghi in modo monistico tutta la realtà, ci si può domandare se questa è scienza, filosofia o visione mistica. Nella sua accezione vera la parola « scienza » designa non solo una descrizione di fatti provati, di cui si enunciano le leggi, è scienza anche una teoria che coordina i fatti, li spiega in un quadro più vasto, anche, se è possibile, interdisciplinare.

Il Convegno poi è proseguito con importanti relazioni di studiosi italiani e stranieri.

Il Prof. Ferdinando Ormea, della Facoltà di medicina dell'Università Cattolica e autore della più importante opera italiana su Teilhard (« Teilhard de Chardin: guida al pensiero scientifico e religioso » voll. 2, Firenze, 1968) ha sviluppato un parallelo tra Teilhard ed Hegel, rilevando come in ambedue c'è una aspirazione a conciliare il cielo e la terra con un tentativo ardito, caratterizzato dal fatto che il metodo diventa contenuto. Le differenze però sono anche notevoli; se Teilhard avesse studiato Hegel forse avrebbe arricchito la sua sintesi di note importanti, come quella dell'elemento dialettico negativo necessario per una adeguata spiegazione del problema del male.

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[1] P. N. M. Wildiers, professore di teologia all'Università di Lovanio, all'Università di San Francisco e alla Scuola di Teologia di Berkeley (California) è autore del volume « Introduzione a Teilhard de Chardin », Milano, 1966.

Il P. Giovanni Magnani, dell'Università Gregoriana di Roma, parlando di « Fenomenologia e totalità » ha illustrato il principio di totalità, che si verifica quando una molteplicità di elementi collegandosi, pur mantenendo una loro sufficiente individualità, danno origine ad un « tutto » che è qualche cosa di nuovo, irriducibile alla mera somma degli elementi precedenti. Questo fenomeno, presente in vario modo in tutte le scienze ed approvato largamente dagli psicologi della « Gestalt », è stato studiato da Teilhard su scala interdisciplinare. Nella sua sintesi fenomenologica T.d.C. descrive l'ortogenesi della totalità definitiva, cioè l'evoluzione del mondo verso una forma superiore e ultima, chiamata punto Omega attraverso la nascita e la successiva unione delle singole totalità. La legge di complessità-coscienza da lui proposta è questa via per cui si formano, emergono e infine creativamente si uniscono le varie totalità.

Il Prof. Giorgio Straniero della Facoltà di Filosofia dell'Università Cattolica e autore dello studio « L'ontologia fenomenologica di Teilhard de Chardin », Milano, 1970, ha presentato una comunicazione su « L'ontologia di Teilhard » sostenendo la possibilità di arrivare ad una metafisica attraverso il metodo fenomenologico. Altri esperti ed autori di opere su T. d. C. hanno presentato comunicazioni e animato i dibattiti, fra i quali il Dott. Wilhelm Kunz di Friburgo (Germania) ha presentato una ricerca sul tema « Dalla dialettica alla complementarietà ».

Il Professore Giuliano Toraldo di Francia dell'Università di Firenze, il P. Alessandro Dall'Olio dell'Istituto Stensen hanno parlato sullo strutturalismo di Teilhard; le professoresse Annette Daverio e Elena Cortellese Platania, P. Anselmo Staico O.P., il Dott. Marcello De Stefano sono intervenuti con originali trattazioni sul tema.

Nel Convegno si è constatato che il tipo di fenomenologia che T.d.C. propone è di tipo positivistico. Si distingue da altri tipi di fenomenologia per il fatto che non è una ricerca delle cause, ma una riflessione sulle riflessioni delle singole scienze (una iperfisica), che ne coglie il nesso, ne ricongiunge i punti di contatto, sino ad individuare un disegno, uno sviluppo organico di tutta la realtà, e soprattutto una direzione e un senso di tale sviluppo. Questa sua riflessione sulle scienze egli la propone prima con il metodo tipico delle ipotesi di lavoro della scienza, cioè in termini strutturalistici, come un modello di una struttura invisibile della realtà. Poi ne dimostra il valore esplicativo e l'assenza di contraddizioni con l'esperienza. Con ciò si potrà rimproverare a T.d.C. questa o quella ipotesi utopistica in certe illazioni, ma non una mancanza di coerenza in tutta la sua spiegazione.

Infine, la dialettica di T. d. C. non è una dialettica di vera impronta filosofica, ma è un metodo necessariamente usato nella critica della realtà.

Il Convegno si è chiuso con un impegno organizzativo: quello della pubblicazione periodica degli studi fatti in Italia in questo campo e della preparazione del prossimo Convegno di Studi Teilhardiani all'Istituto Stensen di Firenze nel prossimo settembre, sul tema della evoluzione generalizzata.

Firenze, Centro di Studi e Ricerche Teilhard de Chardin.

A. Dall'Olio S. J.

Study Meeting on the Work of Teilhard de Chardin

A Meeting has been held on « The philosophical aspects of the work of Teilhard de Chardin », organised by the Stensen Institute in Florence. The Meeting covered two main points: phenomenology and dialectics.

The opening paper, delivered by Father N. M. Wildiers, was of fundamental importance. The Belgian scholar, who is responsible for the prefaces of several volumes of Teilhard's Complete Works[1], discussed the value and scope of the first of the two hypotheses that Teilhard states at the beginning of « The Human Phenomenon ». The first is that the primacy has been attributed to the psychical and to thought in the substance of the universe; the second is that of the biological value attributed to the social fact that surrounds us. Fr Wildiers discussed the first of these two hypotheses according to the two aspects that it presents. The supremacy of the psyche and of human thought as a working hypothesis is really the starting point of Teilhard's studies, which are summed up in an attempt to give a unitary explanation of the world, starting from man. As is well known, other modern scientists have attempted an equally unitary explanation, but on the basis of the supremacy of matter; he opposed them with his own line of thought, which does not seem less valid than theirs.

The other aspect from which Fr Wildiers has examined the first of the above-mentioned two hypotheses is this: assuming the achievement of the tasks that Teilhard had undertaken - to construct a scientific theory that explains the whole of reality in monist terms - it may be asked whether this is science, philosophy or mystical vision. In its real accepted meaning, the word « science » designates not only a description of proved facts, of which the laws are stated; it also designates a theory that co-ordinates the facts and explains them in a contexts that is broader and, if possible, interdisciplinary.

The Meeting continued with important papers read by Italian and foreign scholars.

Prof. Ferdinando Ormea, of the Medical Faculty of the Catholic University and author of the most important Italian work on Teilhard (« Teilhard de

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[1] P. N. M. Wildiers, Professor of Theology at Louvain University, at San Francisco University and at the Berkeley School of Theology (California), is the author of the volume « Introduzione a Teilhard de Chardin », Milan, 1966.

Chardin: guida al pensiero scientifico e religioso », 2 vols, Florence, 1968) developed a parallel between Teilhard and Hegel, pointing out how, in both of them, there is the aspiration to reconcile heaven and earth in a bold attempt, characterised by the fact that the method becomes content. However, there are considerable differences: if Teilhard had studied Hegel, perhaps he would have enriched his synthesis with important ideas, such as that of the negative dialectical element necessary for an adequate explanation of the problem of evil.

Fr Giovanni Magnani, of the Gregorian University in Rome, speaking on « Phenomenology and Totality », discussed the principle of totality, which arises when a multiplicity of elements, associating with one another yet maintaining sufficient individuality, give rise to a « whole » that is something new and cannot be reduced to the mere sum total of the previous elements. This phenomenon, present in various ways in all the sciences and widely approved by the Gestalt psychologists, was studied by Teilhard on an interdisciplinary scale. In his phenomenological synthesis, Teilhard describes the orthogenesis of definitive totality: that is, the evolution of the world towards a higher and final form, called point Omega, through the birth and subsequent union of the individual totalities. The law of consciousness-complexity that he proposed is this way by which the various totalities form, emerge and finally unite creatively.

Prof. Giorgio Straniero, of the Faculty of Philosophy of the Catholic University and author of the study « L'ontologia fenomenologica di Teilhard de Chardin » (Milan, 1970), read a paper on « The Ontology of Teilhard », in which he maintained the possibility of arriving at a metaphysics through the phenomenological method. Other experts and authors of works on Teilhard also read papers and took part in the debates, including Dr Wilhelm Kunz, of Fribourg (Germany), who presented a study on the subject « From dialectics to complementariety ».

Prof. Giuliano Toraldo di Francia, of Florence University, and Fr Alessandro Dall'Olio, of the Stensen Institute, spoke on Teilhard's structuralism; Profs Annette Daverio and Elena Cortellese Platania, Fr Anselmo Staico O.P. and Dr Marcello De Stefano presented original papers on the subject.

At the Meeting it was shown that the type of phenomenology the Teilhard proposes is of the positivistic type. It differs from other types of phenomenology in the fact that it is not a search for causes but a reflection on the reflections of the individual sciences (a hyperphysics) that gathers them all together and reunites their points of contact, to the point of identifying a design, an organic development of the whole of reality and, above all, a direction and a sense of this development. This reflection of his on the sciences is first proposed by him with the method typical of the working hypotheses of science - that is, in structuralistic terms - as a model of an invisible structure of reality. He then demonstrates the explanatory value and the absence of contradictions with experience. In this it will be possible to criticise Teilhard for this or that utopian hypothesis in certain deductions, but not for a lack of consistency in his whole explanation.

In conclusion, Teilhard's dialectics are not a dialectics of a real philosophical stamp but a method necessarily used in the criticism of reality.

The Meeting closed with a decision on an organisational undertaking: that of periodically publishing the studies carried out in Italy in this field and of preparing another Meeting on Teilhardian Studies at the Stensen Institute in Florence, to be held next September, on the theme of generalised evolution.

RECENSIONI - BOOK REVIEWS

C. D. DARLINGTON, *The Evolution of Man and Society*, George Allen and Unwin, London, 1969, 754 pp., figs., £3.50.

The volume entitled « Recent Advances in Cytology », which Darlington published as far back as 1932, revealed him as an « infant prodigy », and no biologist with fairly wide interests - even if not a histologist - could afford to ignore this work, the value of which is shown by the fact that in 1965 it appeared in a new edition that was almost a reprint. Similarly, « The Evolution of Genetic Systems » of 1939 was reprinted in 1958 and confirmed the author's fame as a morphological cytologist with wide interests in Genetics and Evolutionism.

The present volume is, chronologically, a work of late maturity, though it shows no signs of flagging inspiration. On the other hand, as is stated in the preface, the author was prompted to write it forty years ago, when he had « the opportunity of travelling among the peoples of what we now call the Ancient East. I was struck by the differences between their societies and those I already knew. They raised questions in my mind which, it seemed, ought to be answerable in terms of historical or in terms of biological principles, or perhaps of both ».

In this volume there is little mention of chromosomes, though they are briefly referred to in the first part, essentially biological, which comprises barely 65 pages out of a total of 700 pages of text. The work may be termed a universal history, seen by an author with a solid biological training, whose thought, supported by a prodigious intelligence and nurtured by decades of extensive reading in the anthropological field, and still more so in the historical field, nevertheless draws ever closer to a humanistic view of history. And I maintain that the choice is right, because, for the man that has scarcely emerged from the turmoil of evolution, for the man of the Neolithic Age that becomes a farmer and a member of « provident societies », cultural evolution takes on a far greater value than genetic evolution. But Darlington, without straining the point, whenever he follows the expansion of peoples and the new contacts between different civilisations - from the spread of the Persian, Macedonian and Roman Empires to the recent invasions of Europeans in Africa, America and Oceania - speaks not only of cultural contributions and selection but also of genetic hybridisation. He thus draws attention to a factor that, while not preponderant, is certainly undervalued by the majority of the best historians and wrongly assessed by second-rate authors that dabble in biological problems and unfortunately have a great hold over less critical readers, who are far more numerous and are ready to accept suggestions of nationalism and racialism.

The volume covers a vast field of information, ranging from the effects of a consanguinity carried to extremes, resulting in the decline and extinction of the famous XVIII Pharaonic dynasty, to the history of the celibacy of Christian priests; from the struggles for succession in the Ottoman dynasties -

with the law that authorised fratricide by the newly enthroned monarch and almost sterilised the dynasty at every generation and necessitated the recruitment of a new élite from the subjugated populations - to the oligarchic system of government in the Republic of the Veneto; from a host of curious information on the casts in India and on the effects of genetic isolation, to the history of her colonisation; from the invasion of the Hamites, who introduced corn and the ox into Africa about 5000 years B.C., to the expansion of the Bantus, the arrival of the Europeans and apartheid in South Africa; from the statistics on the importation of negro slaves into America, to those on the political executions in France, in the Reign of Terror, after the Commune of Paris and at the Liberation in 1944; and I could go on for whole pages, listing important subjects and erudite curiosities.

Whom will this book interest? I should say, a great number of readers whose culture is prevalently naturalistic or humanistic; and I hope to see it translated and launched by some Italian publisher, because it is just in Italy, more than in the English - speaking countries, that there is such a sharp and harmful separation between the two cultures - a separation that this volume successfully attempts to overcome.

Firenze, Università, Istituto di Anatomia Comparata.

E. Padoa

P. Omodeo (ed.), *Opere di Jean-Baptiste Lamarck* (Series: *Classici della Scienza*, No. 13), Unione Tipografico-Editrice Torinese, Torino, 1969, 480 pp., L. 7.500.

It often happens that the work of the most important people in science is represented in the form of a stereotype which is transmitted from treatise to treatise, and even in books dealing with the history of a particular discipline, without any critical evaluation, without the original works having been read by the person who quotes them. To this attitude, which is general in the field of the historiography of science, there are few exceptions. One of these is Omodeo who, it is clear from all his works on the history of biology, has critically reread the more significant works of the period, or those of the authors on whom he directs his attention, and has often carried out original research in the archives.

This edition of some of Lamarck's works is a true revelation. Instead of presenting the usual and commonplace outline of the inventor of a system of evolution fully discredited by modern science - Lamarckism, based on the fallacious principle of the inheritance of acquired characters - Omodeo reconstructs a complex and lively scientific personality who, despite certain inconsistencies, turns out to be the most complete forerunner of Comte's positivism. Not only do we owe to Lamarck the first formulation of a coherent and complete evolutionary theory, but, contrary to the usual stereotype, he also deserves credit for having attempted a general concept of biology that was essentially materialistic, or at least positivistic. Moreover Omodeo considers that the internal forces accepted by Lamarck as evolutionary factors should be interpreted in a deterministic, rather than a vitalistic, sense 'as has been claimed by more than one crude critic'.

In the introduction the author sketches an outline of Lamarck's biography and of his thought. As to the former there are still many gaps; on the latter there is a scarcity of critical data, partly remedied now by some research carried out by Omodeo himself in the archives of the Muséum and at the Bibliothèque Nationale in Paris, and by various interpretations deriving from the wide historico-philosophical survey which Omodeo provides as a background to the work of the French naturalist.

The works chosen for this publication and translated according to the criteria stated in a bibliographical note are: the Prolusion for the course of the year X (about fifty pages); the Introduction to the Natural History of Invertebrates (about 250 pages); and the Analytical System of man's positive knowledge (about 100 pages). There is nothing of the *Philosophie Zoologique,* which is the best known work and also the most easily available in comparatively recent editions. The 'Prolusion' contains the essence of the evolutionary theory. The 'Introduction' is a sort of general zoology, or rather general biology, extremely interesting because it frames the vital phenomena within a general, essentially mechanistic, vision. The 'Analytical System' is like a short treatise of epistemology, justifying the position of forerunner of positivism which the author ascribes to Lamarck.

We are grateful to Omodeo for having given us the opportunity of reading the most significant works of this biologist (works which are very rare and only obtainable with difficulty) in an excellent edition, for which credit must be given also to the publisher. But above all we must point our that, rather than a simple critical edition, it is a work which will be an authority in international fields, inasmuch as it presents the figure of Lamarck in a new and more complete light, which emphasises his originality and the breadth of his thought.

Roma, Università, Istituto di Genetica.

G. Montalenti

H. Harris, *The Principles of Human Biochemical Genetics,* North-Holland Publishing Company, Amsterdam, 1970, x+328 pp., figs. and tables, Dfl. 22.50.

In 1908 Sir Archibald Garrod published a book entitled *Inborn errors of metabolism,* the value of which was not fully appreciated in his time. In this volume, the bases of the research were founded on the action of the genes, studied in biochemical terms. Biochemical genetics, which largely coincides with the study of the primary effect of the genes, began to develop rapidly when, in 1940, Beadle and Tatum introduced, as an object of genetic research, the mould *Neurospora.* Studies were very soon extended, in increasing numbers, to various micro-organisms, in which, in fact, it is easier to recognise the action of the genes as controllers of particular biochemical reactions, and the famous relation: one gene - one enzyme was established, later modified into: one gene - one protein.

It was then realised that man is an excellent subject for the study of certain problems of biochemical genetics, and a return was made to the line of research opened up many years previously by Garrod. The climax of these studies is represented by the analysis of the mutants of hemoglobin, in which it has been possible to show that it is sufficient to replace a single amino-acid in a peptide chain in order to confer on the hemoglobin abnormal characteristics that are reflected in a well-defined pathological syndrome: the sickle-cell or hemoglobin S syndrome. Hence the name « molecular diseases » given to those morbid forms that may be traced to a variation in a protein molecule.

The author of the present volume has made very important contributions to the knowledge of the action of the genes in determining the characteristics of the molecule whose formation they control. In particular, Harris has studied the so-called biochemical polymorphisms: that is, the presence, in human populations, of alleles of one and the same gene, which give rise to variants in the enzymes or other proteins. Today, this field of research is actively expanding on account of its importance from the point of view both of general genetics and of medicine. Much needed, therefore, is a summary account of the main facts and problems, presented very clearly by a highly competent and specialised expert. The present volume will therefore be enthusiastically received by all geneticians.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

Air Pollution (Proceedings of the First European Congress on the Influence of Air Pollution on Plants and Animals, Wageningen, April 22 to 27, 1968), Centre for Agricultural Publishing and Documentation, Wageningen, 1969, 416 pp., figs. and tables. No price stated.

The problem of air pollution is one of the most important among those connected with the threats that hang over mankind in its race towards the destruction of the biological environments that is a result of demographic development and the evolution of technology.

This first European Congress on the subject was held at Wageningen in 1968 through the initiative of the Netherlands Government and the Committee for the Preservation of Nature and Natural Resources of the Council of Europe. In all, 35 papers were read, grouped according to the following sections: 1. Inventory of damage due to industrial and urban air pollution and research done in relation to this pollution; 2. Experimental research; 3. The effects of air pollution on plants; 4. The effects of air pollution on vascular plants; 5. The effects of air pollution on non-vascular plants; 6. The effects of air pollution on animals; 7. Measuring air pollution in vascular plants; 8. Measuring air pollution in non-vascular plants; 9. Resistance of plants to air pollution; 10. Shelter belts for air purification.

As may be seen from this list, the subjects discussed covered most of the problems concerning the action of contaminants on vegetable and animal organisms, including those connected with the measurement of the damage.

Attention was drawn to the importance of closer international collaboration for the study of this problem and for the standardisation of the means and methods employed.

Roma, Università, Istituto di Genetica.

G. Montalenti

A. Alland Jr., *Adaptation in Cultural Evolution: an Approach to Medical Anthropology*, Columbia University Press, London, 1970, x+204 pp., £3.15.

The author's interest in human ecology, particularly its medical aspects, developed during his field experience in the Ivory Coast, West Africa. The observations he made led him to examine the role of hygiene and therapy in relation to environmental adaptation, and especially on the ecological adjustments in general and their place in the evolution of human behavioral systems. As he states in the preface, in this book is offered a tentative and somewhat speculative analysis of disease-oriented behaviour in relation to human ecology and behavioral evolution. In the introduction we find the following statement: « My theoretical bias is tied to the biological theory of evolution. It is my strong conviction that the evolution of human behavior, including what has come to be called cultural history, can best be understood in terms of such a theory. My major debt, therefore, is to Darwin and modern theoretical biology, although the work of social evolutionists has also had an important influence on my thinking ».

On this very sound ground, the Author examines the following subjects, each one constituting a chapter: Evolutionary theory; the Ecology of human disease; Adaptation to disease; Native medical practice; Native medical systems; Medical systems under acculturation. The matter is treated mostly from a speculative point of view, with the subsidy of some first hand or second hand experience. Factual data, however, are not abundant and statistics are entirely lacking.

This is thus a good book for general orientation of physicians and ethnologists working on primitive populations.

Roma, Università, Istituto di Genetica.

G. Montalenti

Facets of Genetics (Readings from Scientific American), W. H. Freeman and Company Ltd., Berkshire, 1970, xii+354 pp., figs., £2.30.

Thirty five articles published in the journal *Scientific American* from 1948 to 1970, dealing with different aspects of genetics have been selected and reprinted. They are written by the main authors of some of the most important discoveries made in this lapse of time in the fields of genetics, developmental biology and molecular biology. They are arranged into five sections, namely: The elements of inheritance, The nature of the gene, From gene to organism, Genetics and evolution, Genetics and Man. Cross references and index make easy the consultation of this book, which constitutes a sort of textbook of advanced genetics, beautifully illustrated.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

R. ROSSINI, *Trattato di psichiatria*, Cappelli Editore, Bologna, 1969, 822 pp., figs. and tables, L. 12.500.

The Handbook of Psychiatry (Italian title « Trattato di Psichiatria ») by R. Rossini is divided into a first part dealing with the troubles of consciousness, the instinctual-affective, intellective and volitional sphere. The second part analyzes the exogenous, involutional and endogenous psychoses and psychoneuroses. The last part is devoted to the therapy of mental illnesses, with particular regard to shock therapies, psychopharmacology and psychotherapy.

Dealing with the problem of mental illness mention is made of the psychopathological, dynamic, phenomenological, anthropoanalytical approaches to the problem itself. The concepts of conscious and unconscious according to Freud, Jung and Janet, above all the freudian theory, are widely discussed. Then the psychology of the evolutionary age is presented and the contributions of Klein, Osterrieth, Erikson, etc., taken into a necessarily brief review. Also the latest neurophysiological contributions to the knowledge of some psychopathological mechanisms of basis are mentioned: in this regard the chapter on the physiopathology of sleep and dream is of particular interest.

Going through this bulky volume one notices how many psychological and psychopathological concepts have been simplified by the Author for didactic purposes and his subtleness in discussing complex and controversial problems. A certain attention is paid to psychotherapy which is presented as an integrating therapy for neuropsychoses. One can but admire the Author's reserve in presenting psychosurgery and the caution necessary before adopting it. Psychopharmacology is widely and rightly treated in view of the general approach, of organicistic type, by which many mental illnesses are presented.

In conclusion Rossini's Handbook of Psychiatry satisfactorily meets the needs of many students and experts who were looking for a synthetical, simplified and above all up to date approach to the problems of psychiatry.

Milano, Università, Laboratorio di Fisiologia.

M. MANCIA

G. Benedetti, *Neuropsicologia* (Le attività psichiche in una sintesi di psicologia sperimentale, neurofisiologia e psichiatria) (Series: *Biblioteca di Psichiatria e di Psicologia Clinica*, No. 22), Feltrinelli Editore, Milano, 1969, 750 pp., figs., L. 10.000.

The volume « Neuropsychology » by Benedetti reports an enormous quantity of bibliographical data referred to the experimental works in neurophysiology, behaviourism and neuropsychology of the last 20-30 years. The volume is divided into four main parts.

The first part, dealing with the functional models, reports most part of the experiences carried out mainly on animals in order to study the processes of memory, instinct, learning, states of consciousness, attention, sleep, dreams, perception, will, motivations, pain and pleasure, emotion, anxiety and aggressivity. In all the chapters one can see the constant effort of the Author to overcome the division between the neurophysiological disciplines, taken as object of research, of study, of measurement, of objectification, and the psychological disciplines with their subjective, non measurable, non objectificable elements. Therefore the Author often follows an analogical approach, which not always can be accepted, and advances often imprecise hypothesis on the basis of experimental data which, because of their objectivity, can rarely be interpretatively reduced. In the chapter dealing with sleep there are some bibliographical voids and many experiments are discussed without the relative reference. It is worth noticing the elaboration of the data relative to the centrifugal control of sensations and the Author's attempt to interpret some psychological data.

The second part concerns the psychophysiology of the different cerebral structures: hypothalamus, lymbic system, thalamus, cortex, frontal, parietal and occipital lobes. In this case the Author's work is valid and extremy helpful to the psychologists interested to more strictly neurophysiological problems.

In the third part the interpretation which can be given by a psychotherapist, such as the Author, of the above said basic neurophysiological concepts is presented.

In the fourth part one founds the interesting attempt to unite in single chapters neuropsychology and sociology, through the studies and the interpretations of the data of Delgado and of other behaviourists; neuro-psychology and psychosomatics, where neurophysiology can provide many useful data to interpret the functional mechanisms which are of great importance for the psychotherapist and for the medical doctor; neuropsychology and psychoanalysis, where psychoanalysis is given a biological dimension and the attempt is made to reduce the dichotomy between subjective and objective worlds, a dichotomy which is not only a limit to the study but also the always present problem of scientifically objectifying the signs communicated from the subjective experience; neuropsychology and philosophy, where the Author speaks about the relationship of causality, of reality and perception, of philosophy and natural sciences, and leaves the rigid world of experimental sciences to start a wider discussion on the problems which comprehend the totality of man.

Milano, Università, Laboratorio di Fisiologia.

M. Mancia

C. A. Valentine, *Culture and Poverty* (Critique and Counter-Proposals), The University of Chicago Press, London, 1968, xiv+216 pp., £2.70.

About three quarters of the volume consist of a prevailingly polemic review of the sociological and anthropological literature dealing with concepts such as « culture of the lower classes » and « culture of poverty ». The first concept has its classical exposant in E. F. Frazier, the negro sociologist devoted to the study of the negro-american family. The second concept is the leading theme of Oscar Lewis' work consisting mainly in the analysis of the peasant and urban societies of Mexico and Puerto Rico. Valentine contests the use of these two concepts - and this is the leading thesis of the book - which in his discussion end by identifying themselves. It is absurd - he says - to speak in terms of culture about the poor and the lower classes (obviously all the discussion is to be referred to the complex western societies leaving aside the « poverty » of preindustrial non-western societies). In fact they are not « cultures » but « subcultures », that is « subsocieties » and not «societies», as poverty is not a sociologically isolated and autonomous phenomenon with respect to the entire society of which it is part. Poverty has to be identified with the socio-economical inequality. Thus, we are in front of a comparative notion referred to a relative peculiarity, that is referred to its counterpart « wealthy », « rich », « affluent ». The fact is that many dominant cultural values and attitudes of the « poor » are strictly related to those of different social classes from which they can not be separated within the frame of an anthropological analysis. Hitherto we can not dissent from the Author. Moreover he criticizes also the governmental policy which, being inspired to the principles put forward by those anthropologists he is contesting, is aimed at carrying out a « war against poverty » taking poverty as its unique target instead of inserting it into the context of a whole society needing revolution or reform. Such policy will most probably result in a failure or at the most in a program by which the « poor » subculture will be assimilated to the middle class subculture taken as a positive model.

The Author's objections to Lewis or to other anthropologists (Glazer, Moynihan, Miller, etc.) are, from a critical point of view, gratuitous. In fact Lewis himself is perfectly aware of the inadequacy of the term « culture of poverty » as he knows that the proper term is « subculture », but he uses the word « culture » conventionally, out of simplification.

The ambitious and pretentious Valentine's book which wants to open a « new » way for the interpretation of poverty as a sociological and anthropological phenomenon, has its weakest point in the disproportion between the *pars destruens*, constituted by the above said polemics, and the *pars construens* which is but a short appendix. Here he insists only on the opportunity of a deeper anthropological analysis of poor subsocieties and, from a practical point of view, on the need of an intervention policy which, aiming at overcoming poverty and supported by a strong pressure exerted by the mass of the population, may guarantee the same opportunities to everyone. As an example the Author quotes the movement for the civil rights. As a matter of fact the proposals he makes are quite modest, especially in view of the expectancy raised by a book which declares to be anticonformist and antitraditionalist. The contribution given to the explanation of the different interpretative models on the problem of poverty is more positive: a contribution which is essentially theoric but nonetheless useful for a practico-political and epistemological approach.

The first model of interpretation is the one most bitterly criticized by the Author. This model attributes to the lower classes a distinct subculture consisting of disorganized, pathological and uncomplete manifestations of the middle class subculture. The poor subculture would therefore perpetuate its own cultural models and its psychological inadequacies in virtue of the process of socialization: a self-generating and self-perpetuating culture. According to this interpretative model the fittest program would be to eliminate poverty by means of the cultural assimilation to the middle class model: a process which is carried out through revolution in underdeveloped countries and through social work, such as psychiatry and education, in the more complex western societies. The second interpretative model, which follows a marxistic orientation and is the one adopted by the american radical left, consists in recognizing the active causes of the pathology, of the distorsions and of the inadequacies of the poor subculture within the structure and the entire social system of which they are part, and in the denial of cultural resources operated by society towards the poor. The program and the remedy in this case consist in a radical modification of society through social revolution, redistribution of the resources and consequent elimination of poverty. The third interpretative model, with which the author personally agrees, implies the concept that the poor of the lower classes have some subcultural values of their own and other traits common to the way of life of the middle classes or of the entire social system; that together with pathological aspects they present sound, positive and adaptively creative characteristics; that an increase of wealth in favour of the poor must be a result of a modification of the entire social structure and of some subcultural models on the basis of social movements of cultural renewal carried out both by the poor and by the entire society. As one can see the Author only partly agrees with the second interpretative model in spite of its uncompleteness as it does not analyze all the possible instances of society; for instance it does not consider, in the Author's opinion, that in the U.S. society a violent taking up of power in favour of the poor classes seems to be impossible.

In conclusion Valentine's book contributes to make even more problematic the ethnological approach to the phenomenon of mass poverty in the third world societies and in specific social groups of western society. Moreover it does not emphasize the enormous differences existing between non-western poor cultures - with their character of « cultures » in opposition to other « cultures » - and the poor subcultures of the complex societies of the euro-american world, where the antagonism is lived between the social classes and not between the cultures.

Roma.

V. Lanternari

S. Segal, *Ecological Notes on Wall Vegetation*, Dr. W. Junk N.V. Publishers, The Hague, 1969, 326 pp., figs. and tables, guilders 60.

This volume is the fruit of lengthy studies begun in 1951 and which have until now only been published in a partial form. The author has taken samples of wall vegetation from over 1200 stations in Great Britain, Central Europe, Poland and Italy. For all these regions, except France and Italy, the literature on the subject was either scarce or totally nonexistent. After an opening chapter and a chapter on methodology, the book deals with the ecology of wall vegetation, its structure, and the criteria of classification and of dynamism. The longest chapter is that given over to a review of the various types of vegetation, which are put into various groups, as, for example, all those with *Parietaria judaica*. Then there are the vegetational groups with *Hedera helix, Rumex scutatus, Hieracium, Corydalis lutea, Asplenium trichomanes, Asplenium viride, Polypodium, Sagina procumbens, Soleirola soleirolii, Chrysanthemum parthemium*, etc. The text is accompanied by a list of the species cited, and for each the author gives the taxonomic group, the degree of ploidy, the biological form according to both Raunkiaer and Iversen, the habit, the sociability, the color of the flower, the dissemination mechanism, the type of vegetal formation to which it belongs, and the geographical distribution. Many pages are dedicated to a discussion of the problems of the structure and analysis of the vegetation.

This book is interesting for various reasons. It offers abundant data for students of flora, phytogeographers, geneticists, and evolutionists. But Segal's study will also be useful for people who are concerned with broader historical disciplines which go back to the period when human society began to have an effect on the history of the biosphere. The voluntary or involuntary effects of much human activity is in fact stored up in vegetation: harmful species carried to distant areas as seeds, species which were once cultivated and which now today are only harmful, plants transported with cargo and various goods (for example wool), and so on. This great record is not, of course, easy to read, but it is important to begin to organize it. On a purely botanical plane, the study of wall vegetation gives us a way of analyzing various mechanisms of dissemination and succession and the importance of various ecological factors; and it should also be remembered that walls are often a place of refuge for species which are now either rare or absent from the surrounding territory. The study of wall vegetation is not, therefore, as arid as might seem at first glance, although we would not want to see in the not distant future this type of vegetation become the richest in existence surrounded by a monotony of fields sprayed with herbicides and mountain slopes covered with uniform plantings of pines, when perhaps even the football field on the edge of the city will make plant collectors cry out with joy.

Siena, Università, Istituto di Botanica.

G. SARFATTI

R. BAJA, *La méthode biologique* (Textes choisis), Masson et C.ie, Paris, 1969, VIII+210 pp., figs., 26 F.

This volume was originally conceived with modest intentions: as the author states, the object was to provide high school students with scientific texts that could offer a solid basis for their observations on biological methods. But once he had started upon the work, the author (as he informs us) soon found himself right on the borderline between pure science and what is usually termed the philosophy of sciences. He then decided to arrange his chosen texts in a logical manner and to connect them together by means of short commentaries that would give continuity to his discourse, which on the contrary would necessarily be fragmentary if confined to an anthology of the well-know authors.

The resulting volume falls into four sections: the first concerns the general characteristics of the biological method; the second, the procedures of biological research; the third, the results of the experimental method; and the fourth, the limits of the field of action of biological methods. Each of these parts is divided, in turn, into several chapters and sections. For example, the first part comprises three chapters: The rules and aims of the method (The Rules; The Aim; The Limits; The Means); The forms of biological reasoning (The Method of Agreements; The Method of Differences); The characteristics common to all forms of biological reasoning (Intrinsic Character; The Ordering of Biological Reasoning; Unity of Work: the role of ideas; Conclusion: the reasoning of biologists is not that of mathematicians). The same arrangement is followed in the other parts.

In each of the main sections, every chapter essentially consists of a text taken from some work by a famous biologist (or philospher), with a commentary by the author. Twenty-seven authors are cited, beginning with Descartes and ending with Teilhard de Chardin. Only five of these authors are not French (or French-speaking): namely, Bayliss, Darwin, Harvey (always wrongly spelt Harvey), Mendel and Spallanzani. Those most frequently cited are Claude Bernard, Réaumur, Pasteur and Spallanzani. The volume concludes with a chronological table of the main stages in the evolution of biology, which is rather incomplete.

The work could prove useful to students as an introduction to the highly complex subject of the biological method. At the same time it must be realised that it gives a very incomplete, and therefore very superficial, survey of the historical development of the biological method. Many important classical authors - such as Redi, for example - are not mentioned. Likewise, there is practically no mention of the latest discoveries of molecular biology, which are of fundamental importance for a mechanistic interpretation of biological phenomena.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

R. G. HARRISON, *Organization and Development of the Embryo*, Yale University Press, London, 1969, XXIV+290 pp., figs. and plates, £6.75.

Ross Granville Harrison, (1870-1959) professor at the Yale University, New Haven, Conn., was one of the most eminent biologists of the generation of American scientists who came to Europe for their postgraduate work, then returned home and gave a wonderful impulse to scientific research in the USA.

Within a few decades the United States reached a leading position in several scientific fields. In Europe at that time the most advanced laboratories and leading scientists were located especially in Germany. Young Harrison was associated (1892-1894) with the Anatomisches Institut in Bonn, whose director was M. Nussbaum. There he performed an investigation on the development of fins in teleosteans.

A few years later Born invented the technique of tissue grafting, which in subsequent years and up to date has allowed to collect a tremendous yield of results in experimental embriology. Harrison soon adopted the method for the study of growth and regeneration of the tail, of the relationship of developing muscle and the nervous system in the embryo, etc. In 1907 appeared Harrison's classic paper: « The outgrowth of the nerve fiber as a mode of protoplasmic movement » in which he described for the first time how to grow animal tissues in culture outside the body. He is thus the originator of the tissue culture method, which was largely applied and further developed thereafter. Subsequently Harrison continued working in the field of experimental embriology; most of the 83 papers he published during his life are now considered as classics.

In the spring of 1949 R. G. Harrison delivered a series of Silliman Lectures at Yale University, which were a kind of summary and conclusion of his life long work. Unfortunately illness in his last years prevented him from preparing the lectures for publication. His research assistant Sally Wilens decided to reprint in this volume five Harrison papers which are the major source of the matter be expounded in his Silliman Lectures. They are accompanied by some excerpts from his lecture notes. A list of the papers published by Harrison (1893-1948) and a list of memorial tributes published after his death are appended to this beautiful and extremely useful book, which deserves a place of honor in every biological library.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

S. OHNO, *Chromosomes sexuels et gènes liés au sexe*, Gauthier-Villars, Paris, 1969, x+194 pp., figs., 48 F.

The present volume is the French translation (by C. Palevody) of the work originally entitled « Sex Chromosomes and sex-linked Genes » and published in 1967 by Springer-Verlag in the series « Monographs on Endocrinology ».

As is well known, the author had for a long time been engaged upon a study of many questions regarding the genome, particularly in the field of the Vertebrates. This volume constitutes a clear and brilliant condensation of his ideas on the importance of the part played by the sex chromosomes in the evolution of the vertebrates. After a brief introduction, the main part of the work is divided into three sections, each consisting of several chapters, devoted to: The Evolution of the sex chromosomes and genomes in the Vertebrates; The Evolution of the quantitative compensation mechanism of the sex-linked genes; and The so-called sex-determinig factors and their relation to sex-determination.

The subject-matter, parts of which are treated in a highly personal manner, fully covers its particular field according to the most up-to-date criteria and will therefore prove useful to all biologists in general. However, it will chiefly interest specialists in cytogenetics and, on account of its constant connections with the facts of evolution, also experts in the phylogenesis and taxonomy of the Vertebrates.

Pavia, Università, Istituto di Anatomia Comparata.

M. VIALLI

P. MEADOW and S. J. PIRT (eds.), *Microbial Growth* (Nineteenth Symposium of the Society for General Microbiology, held at University College, London, April 1969), Cambridge University Press, London, 1969, x+450 pp., figs., tables and plates, £5.

This nineteenth volume of the fine series published by the Society for General Microbiology is devoted to microbial growth. The volume was eagerly awaited, in view of the enormous development of studies in this field which, though mainly regarding general Microbiology, also concerns many other fields of Biology, chief of which, on account of its immediate effect with regard to humans, is perhaps that of Oncology.

The volume contains the papers presented at the Symposium held at University College, London, in April 1969. An introduction, by J. Maynard Smith, on limitations of the speed of growth, is followed by the indivdual contributions, suitably grouped in two sections: the growth of the populations, and the growth and differentiation of the cells. A paper of not only microbiological but also general interest is that by D. Lloyd on the development of organelles involved in the production of energy.

Another paper that goes beyond pure Microbiology in its significance is that by D. H. Northcote, on the growth and differentiation of plant cells *in vitro*, and also the parallel work by J. Paul on animal cells.

The present volume is one of the most important in the series, since the results that it reports mark essential steps in the development of the fundamental and intricate problem of cell growth.

Napoli, Università, Istituto di Patologia Generale.

L. CALIFANO

P. V. GLOB, *The Bog People* (Iron-Age Man Preserved), Faber and Faber, London, 1969, 200 pp., figs., £2.50.

One day, in May 1950 the author was urgently requested to inspect a dead man discovered when digging for peat in the Tollund Fen, in Bjaeldskov Dal in the center of Jutland. The body was extraordinarily well preserved, by a tanning process which occurred in the bog; it was laying on his right side in a natural attitude of sleep. At a closer inspection however a rope was disclosed which encircled the neck in a noose drawn tight into the throat: the man had been strangled, or hung.

Starting from the description of this exciting discovery, the author makes a survey of all other findings of dead men lying in peat beds, which are numerous in Denmark, Germany and some other countries. They belong to people who lived in the fens 2000 years ago, in the Iron age, at the time of expansion of the Roman Empire. Most of them bear the signs of murder: probably they are ritual victims.

On the basis of several excavations, of findings of garments, pottery, weapons, silverware, etc. and of careful checks with the texts of Tacitus, Caesar, Strabon and other writers of the classic times who described the « Germani » and of other historical reminiscences, the author succeds in giving a fascinating reconstruction of the way of life, the habits, the religious rituals of these fierce populations, whose bravery was admired by Caesar. In fact they were able to give much trouble to the invading Roman army.

The book is of high vulgarisation; it makes a very easy and interesting reading and is illustrated with 76 splendid photographs. I suppose it may be useful also to archaeologists and ethnologists, inasmuch as it is bears a bibliographical list.

Roma, Università, Istituto di Genetica.

G. Montalenti

R. Riedl, *Fauna und Flora der Adria* (Ein systematischer Meeresführer fur Biologen und Naturfreunde), 2nd rev. ed., Verlag Paul Parey, Hamburg, 1970, 702 pp., figs. and plates, DM 64.

Approximately 6000 species of plants and animals have been described out of 12000-15000 estimated to live in the Adriatic Sea. The book under review contains the description of about 1500 species. For each one of them a short description is given accompanied by one of more illustrations and by indication of the habitat, the frequency, the economic use. Besides the scientific name, the common names in German, Croat, Italian are also given. The chapters devoted to the description of larger groups (e.g. *Rhodophyta, Copepoda, Echinoidea*, etc.) are made up of seven sections, namely: a short diagnose of the group; methods of preservation and study; number of species that the group includes; habitat, methods of collection; biology (i.e. growth, sex-ratio, reproduction, etc.); development; literature.

The different sections are worked out by a total of 20 specialists, including Prof. Riedl, who has planned, elaborated and edited the whole book. This represents an excellent introduction to the knowledge not only of the Adriatic, but (with some obvious limitations) of the Mediterranean Sea. It should be on the desk of every student of marine biology.

Roma, Università, Istituto di Genetica.

G. Montalenti

ARISTOTE, *Histoire des animaux*, 3 vols., Société « Les Belles Lettres », Paris, 1964/1968/1969; VOL. I: *Livres I-IV*, LVI+182 pp.; VOL. II: *Livres V-VII*, 170 pp.; VOL. III: *Livres VIII-X*, 190 pp.; No price stated.

This is an excellent scholarly edition of the famous aristotelian treatise, the History of Animals. From the philological point of view, it seems perfect to the reviewier (who, however, is not particularly competent in this field). As to the translation and the notes, which on the whole are excellent, two remarks may be made. The first is merely technical: the notes to the translation are to be found in part at the bottom of the page, in part (usually the numbers subsequent to 5 or 6) at the end of each volume. It would have been preferable to have them all together at the end. As to the translation of the scientific names, it would have been better not to translate the world *malachion* by *céphalopodes* and *malacostracon* by *crustacés*, etc. These are modern terms having a precise zoological signification, which often does not coincide exactly with the meaning of the aristotelian text.

But these are only minor points. This edition of Artistotle's fundamental biological treatise is on the whole excellent, and is certainly welcomed both by philologists and historians of biology.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

G. M. CLARKE, *Statistics and Experimental Design* (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1969, XII+162 pp., figs. and tables, £2.10.

Quite a number of texts of statistics for biologists exist on the book market. The present one is the result of lectures given to biology students in an Honours degree course and of the experience made in teaching. It presents the common statistical methods in an easy way, without too much mathematical theory and with a great deal of practical applications and exercises. At the end of the book the student finds answers and comments to the exercises following the different chapters. This is a good text book for teaching the elements of statistical methods to those who are lacking mathematical preparation, as is usually the case for most students in biological sciences.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

N. E. MORTON (ed.), *Computer Applications in Genetics* (Proceedings of a Conference sponsored by the University of Hawaii and the Genetics Study Section, National Institutes of General Medical Sciences, and dedicated to Lawrence Hasbrouck Snyder), University of Hawaii Press, Honolulu, 1969, x+168 pp., figs. and tables, $10.00.

Electronic computers are a very useful tool in genetic research. Especially in the field of human genetics, in which experiments are not feasible and collection of data is laborious and often incomplete, computers have provided means for elaborating complex bodies of data in order to extract the maximum possible information. Problems of formal genetics, such as segregation and linkage can be analytically elaborated in a way that would be practically impossible by normal calculation means. Problems of population genetics, which often involve very elaborate computational procedures have profited very much of the possibilities offered by high-speed computers. The simulation of natural populations with parameters determined by the experimenter has also given interesting results.

The symposium was held at Honolulu, Hawaii, organized by N. E. Morton, head of the Population Genetics Laboratory of the University of Hawaii and responsible for the elaboration of methods for the application of computers in genetics, and was chaired by J. F. Crow, head of the Genetics Study Section of the National Institutes of Health. It consisted of 16 papers covering different problems. Each paper was followed by lively discussion, which is published in full.

Roma, Università, Istituto di Genetica.

G. Montalenti

J. A. Ternisien, *Les pollutions et leurs effets* (Milieux inhalés, ingérés et acoustiques), (Series: « *La Science Vivante* »), Presses Universitaires de France, Paris, 1968, viii+188 pp., tables, 14 F.

J. A. Ternisien, *La lutte contre les pollutions* (Milieux inhalés, ingérés et acoustiques), (Series: « *La Science Vivante* »), Presses Universitaires de France, Paris, 1968, 184 pp., 14 F.

These two volumes, in the Series « La Science Vivante », are closely related to each other, the second being more specifically dependent upon the first. The first concerns pollution and its effects; the second is devoted to the fight against pollution.

An appendix to each of the two volumes contains a useful list of the government organisations, with their relative publications, and a list of the main international organisations and their publications, that are concerned with the problems in question. The first volume also includes a list of the French study and research centres that are directly or indirectly concerned with the problems of pollution and damage to environments.

In the first volume, the first part is devoted to the Origins and characterisation of the contaminating media inhaled, the media ingested and the harmful acoustic media. The second part concerns the effects of harmful environmental media on man, the main affections produced by the inhalation or ingestion of harmful substances or by harmful acoustic media, the pathological states arising from occupational causes and the effects of contaminating and harmful media on animals, vegetable matter and materials.

The second volume is also divided into two parts: the first concerns the technical means suitable for providing protection against contaminating and harmful agents; the second is devoted to the legal means and regulations governing the fight against pollution and environmental damage.

The account is very clear and straightforward, so that even the layman will easily be able to understand and appreciate the serious problems presented by pollution and damage caused by harmful environmental media.

Each of the two volumes contains a short but accurate bibliography, enabling the reader to consult major works on the subject.

Napoli, Università, Istituto di Patologia Generale.

L. CALIFANO

E. SEGRÈ, *Enrico Fermi Physicist*, The University of Chicago Press, London, 1970, XII+276 pp., plates, £3.15.

The figure of Enrico Fermi is certainly one of the most significant in the history of the science of this century, not only on account of the important contribution that he made to the development of modern physics but also because he, like very few others, found himself directly involved, as chief actor, in what was - as is emphasised in the preface to the present volume - perhaps the greatest intellectual adventure of scientific discovery.

The author of the volume, Emilio Segrè, Nobel prize-winner for Physics, was one of the closest collaborators of the great Italian scientist.

In his own words, he shows himself to be now the friend, now the disciple and now the scientist that has had to face the same problems and overcome the same difficulties as those encountered by Fermi.

The picture that he succeeds in giving of this great protagonist of the science and civilisation of our century is exceptionally interesting and has the great quality of being extremely truthful and obsolutely devoid of rhetoric and any form of fulsome writing.

The figure of Enrico Fermi that emerges is truly exceptional: the reader does not know whether to be more astonished at the remarkable capacities as a scientist or at the simplicity and clarity with which Fermi treats the problems that lie at the basis of his life as a man.

Of particular interest are the letters, collected in the Appendix, that Fermi wrote in his youthful years to his friend Enrico Persico, who also became one of the greatest theoretical physicists of the decade preceding the war.

The correspondence began when Fermi and Persico were only sixteen and continued for over ten years. The two young men exchanged information on their researches, their work programmes and academic life. But the reader will be surprised never to find, in spite of the great friendship that bound the two together, any mention of intimate - or, I would say, human - problems. Perhaps these letters already outline one of the most stimulating problems of the present age: the « unidimensionality » of the scientist: that is to say, the drastic cleavage between scientist and man - a subject that was to constitute, in the immediate postwar years, a motive of deep reflection on the part of sociologists and philosophers.

In my opinion, the present volume should be singled out also for the clarity, objectivity and unemotional style with which it is written: it will certainly prove useful and instructive reading for all young would-be research workers and, in any case, for all those interested in the development of scientific thought in our age, which so abounds with achievements and also with contradictions.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

R. S. Cohen and R. J. Seeger (eds.), *Ernst Mach: Physicist and Philosopher* (Series: *Boston Studies in the Philosophy of Science*, Vol. VI), D. Reidel Publishing Company, Dordrecht, 1970, viii+296 pp., figs., Dfl. 38.

Had it been held in 1938, centennial year of Mach's birth, this symposium would have resulted, to put it in O. Blüh's words, in a « partisan teach-in ». The declining of the influence exerted by the neopositivistic school and its interpretation (here represented by the Appendixes by P. Frank and R. von Mises) of some particular Machian theses, seems now to be opening the way to a fresher and more fruitful approach to Mach's work and to the problems it raises.

The works about Mach's « Contributions to the analysis of sensations » (F. Ratliff), « Contribution to the development of gas dynamics » (W. F. Merzkirch) and « Curiosity about shockwaves » (R. J. Seeger) represent the due homage to his experimental curiosity and ingenuity.

O. Blüh has contributed a paper on « His life as a teacher », « Biographical data » and (with Merzkirch) a « Bibliography » comprising original editions in German, English translations, and studies on Mach in English and (since 1930) other languages.

The most interesting core of the volume is represented by the writings of P. G. Bergmann, E. N. Niebert, R. S. Cohen and G. J. Holton; two essays, by K. Menger and H. Gonner respectively, dealing with technical details with « Mathematical implications of Mach's ideas » and « Mach's principle and Einstein's theory of gravitation », appear to be of lesser critical import.

P. G. Bergmann (« Ernst Mach and contemporary physics ») underlines the weakness of the traditional phaenomenistic overemphasis on the concept of observation, which contemporary epistemology of quantum physics takes over from Mach.

E. N. Niebert (« The genesis of Mach's early views on atomism ») appears to suggest, though not very conclusively, that young Mach's relative adhesion to the traditional views on atom being bounded to mechanistic interpretations of chemistry and various related matters, had necessarily to change to a refusal along with his renunciation to the current interpretations of mechanics and of its place in the physical science.

As much interesting as unusual, and surely deserving further prosecution by Machian specialists, is the interpretation advanced by R. S. Cohen (« E. Mach: physics, perception and the philosophy of science »): emploing a sort of self-application of Machian principles and methodical canons on themselves, he demonstrates that - far from supporting each other in a « science of science », as Mach contended - they relay, in ultimate analysis, upon an intuition of « mystic » kind, which colours of itself the whole Machian conception of science.

G. J. Holton (« Mach, Einstein and the search for reality ») compares « the Machist attachment to empirical data or heuristic proposals as the sole source of theory » with the opposed « aesthetic-mathematical attachment to persuasive internal armony as the warrant of truth », reconstructing Einstein's gradual philosophical reorientation (not without waverings) from the former to the latter.

Roma, Università, Istituto di Filosofia.

B. Cermignani

P. Chambadal, *À la recherche de la réalité physique*, Librairie Scientifique et Technique A. Blanchard, Paris, 1969, XII+258 pp., 24 F.

The problem of physical reality has undergone an evolution through the centuries, corresponding to the development of science and particularly of physics. It is this very evolution that the author of this volume seeks to present to the reader. His point of view is centred upon the conviction that physical reality cannot be given a precise definition, like that which one believes can be given of an electron, an invariant, etc., because, if one attempts to do so, one always ends by giving reality a definition that conforms to our personal conceptions and is therefore devoid of any objective value. This is the conclusion reached by the author as a result of an examination of the opinions expressed on physical reality by various scholars and thinkers of our time. The volume, which is not intended as a treatise of epistemology, makes easy reading for any cultivated man, even if he is not a specialist in these problems.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

V. V. Fedynskii (ed.), *The Earth in the Universe* (Geographical Series), Israel Program for Scientific Translations, Jerusalem, 1968, VI+402 pp., figs. and tables, $14.50.

The purpose of the volume is to make known various aspects of the influence of cosmic factors on natural phenomena on the Earth. The subjects are summarized in the introduction; another introductory paper by E. T. Faddeev examines the philosophical side of « cosmization » of science, i.e. of the change from an earthbound to a more universal outlook. The contributions begin in the first part reviewing the present knowledge of the structure of matter and the gravitation; the papers of the second part that of galaxies and extragalactic Universe, as well as of the solar system. Two articles, one by the editor, link space science to terrestrial geologic problems. The third part presents some original ideas putting orogenesis, morphology, climate and hydrography history in relation with the irregularity of the Earth's rotation; some articles briefly exposing the results by Russian scientists of studies of the Earth's crust and of paleomagnetism. In the fourth and fifth parts the relations of physico-chemical and biological terrestrial phenomena with the activity of Sun and various extra-terrestrial factors are investigated. The brief sixth part is a claim for the acknowledgement of K. E. Tsiolkovskii's priority in space exploration. The volume is interesting, mainly because of the difference of point of view of Russian scientists in many subjects and the ample lists of Russian works, in general too little known in the West. The value of the contributions and their development are various: some papers are fairly elaborated, some are short reviews or statement of problems, some other are little more than lists of publications. Here and there references to the authority of Engels, Marx and Lenin in subjects in which their opinion cannot have any weight will probably strike the reader, however broadminded and unbiased.

Torino, Università.

A. DEBENEDETTI

P. B. MEDAWAR, *Induction and Intuition in Scientific Thought* (Jayne Lectures for 1968), The American Philosophical Society, Philadelphia, 1969, XII+62 pp., $2.00.

If we ask a scientist to describe the method by which he conducts his own research, the answers we get are almost bound to be unsatisfactory, and are not infrequently trite or even wrong. There must of course be some methodology by means of which scientists arrive at the results that we call scientific discoveries or laws of nature; but either they are completely unaware of it, or else they have never bothered about explaining it to non-scientists. This attitude is so universal that, accordimg to the author, the only exception is Claude Bernard with his « Introduction à l'Étude de la Médecine Expérimentale » (Paris 1865); and now, we would add, Medawar himself.

Besides the main problems of a logical, philosophical or psychological kind which underlie a comprehensive study of the nature of scientific method, there are certain particular questions which ought to attract the full attention of researchers: for instance, the question of verification or confirmation of experimental data, and questions relating to the concepts of emergency, riducibility and causality.

Following the line of Whewell, Peirce and Popper, Medawar favours a hypothetico-deductive scheme for the interpretation of the methodologies of research, with a clear distinction between the phase of conjecture and the phase of verification or proof. The generative act, of an intuitive and alogical nature, is the proposing of a hypothesis; and the deductive consequences of the hypothesis, following the confrontation with reality, provide a negative feedback according to which the hypothesis is adjusted.

The method of scientific research is seen as a cybernetic dialogue between imagination and reality, with a process of asymptotic approximation of the possible to the actual.

John Stuart Mill's « inductive method », and induction as a means of scientific reasoning, have no place in this scheme.

With these 1968 Jayne Lectures, Medawar has opened up a subject of the greatest importance to scientists and philosophers, and has done it with truly admirable style and intelligence. This is a little masterpiece, to be unreservedly recommended.

Roma, Biblioteca Casanatense.

A. Serrai

S. N. Levine, *Electronique quantique* (Series: *Ouvrages de Mathématiques à l'Usage des Physiciens*), Masson & C.ie, Editeurs, Paris, 1968, 296 pp., figs. and tables, 70 F.

This book originates from a course held by the author with the aim of giving to future engineers notions of Physics necessary for the understanding of modern electronic systems. In this connection, its contents are divided in various chapters not always closely linked one another: the principles of the quantum theory and of statistical mechanics, electronic theory of solids, the theory of transport phenomena, the interaction between electromagnetic radiation and matter.

The author has constant care to show the link between the theoretical notions developed and the practical problems which are met with in the construction and employment of masers and lasers.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

M. Lamotte and F. Bourlière (eds.), *Problèmes d'Ecologie: L'échantillonnage des peuplements animaux des milieux terrestres*, Masson & C.ie, Paris, 1969, 304 pp., figs. and tables, No price stated.

The quantitative analysis of the populations and communities of organisms is an essential groundwork for a knowledge of ecosystems. But for such a study the refinement and comparison of adequate sampling techniques, as well as the evaluation on a statistical plane of what a given technique means, is indispensable.

This publication, put together under the auspices of the French Committee of the International Biological Program, satisfies these requirements; it develops the results of two conferences held in 1963 and 1964 with some fifty Belgian and French ecologists in attendance.

The methods of collecting and capturing the specimens, and the very interpretation of the data, vary with the animal groups and in particular with their size. Therefore separate chapters have been included, directed by different specialists, to deal with the quantitative methods of sampling populations of invertebrates in herbaceous environments, more selective and specialized methods based on the use of traps, and the techniques for sampling bird and small and large mammal populations. Another group of methods concerns hypogeal fauna, with chapters dedicated to the soil microarthropodes and nematodes and the terricolous oligochaeta.

The work is accompanied by numerous diagrams and figures and a rich bibliography, and it is rigorously scientific. It is a highly useful tool for all researchers concerned with the problems of ecology and in particular those of animal synecology.

The authors expect another volume of the same type, this time dedicated to acquatic animals, to follow the present work.

Genova, Università, Istituto di Zoologia.

M. Sarà

J. D. Clark, *Kalambo Falls Prehistoric Site*, Vol. 1: *The Geology, Palaeoecology and Detailed Stratigraphy of the Excavations*, Cambridge University Press, London, 1969, xvi+254 pp., figs., tables and plates, £4.50.

This work, which deals with the prehistoric Kalambo Falls, concerns the upper basin of the largest African river after the Nile, which descends from the Tanganyika (and other) lake region, also known for its rapids and falls.

The Kalambo Falls originate in a tributary east of the Congo River, a tributary called Kalambo which descends from the highland escarpment of Lake Tanganyika. The reader who wants to identify the location of the Falls can find the word Kalambo in the large T. C. I. atlas, using as a simpler point of reference the word *Abercorn* at 1647 meters, which lies between Lakes Tanganyika and Nyassa (Malawi in this book's naming system) and which can be found on large-scale school atlases.

Abercorn, as we read in the introduction to the book, was the supply center for the explorational expedition into the Tanganyika-Nyassa Corridor, through which it is said the prehistoric road passed which put South Africa into communication with North East Africa; but today this center is linked to the Falls by a well-beaten road that can be covered in only three quarters of an hour.

In the book's first chapter, written with a rare clarity of exposition which invites the reacer to follow it, we find a general description and identification of the Falls on the edge of the escarpment (geographical coordinates 8°30' S.

lat., 31° 15' long.). This is presented with a wealth of charts and descriptive orientation.

In these illustrative descriptions the reader can find a summary of the arguments to be developed in later chapters: prehistory, physiogeography, geochronology, vulcanology, lithology, climatology, and seasonal conditions which reflect temperatures, thermical movements, precipitation, and air currents that carry water from the Ocean to the Congo basin, which has the greatest flow rate after the Amazon. This is the basin known through the works of the naturalists, particularly in its mineralogical aspects but also in its quality as a shield — that is, an ancient Pre-Cambrian or Archaic base, a fact which encouraged Wegener to link it with the Brasilian massif in his theory of continental drift. This part of the great shield, with a variety of rock formations but a predominance of arenareous and also quarzite and endogenous rocks like dolerites and porphyrites etc., is known as the Tanganyika plateau or expanse, lying between 1220 and 1525 metres and relating to the Katanga system, a name well known for copper deposits.

In successive chapters and the Appendix some subjects are dealt with by other collaborators.

Chapter II contains an article given over to geology and physiography, with a collection of regional maps and figures, descriptions and illustrations of the lithology, geomorphology, structure, hydrology, and sections with a detailed stratigraphy of the Falls; all of this includes the work of collaborators. A bibliography accompanies every chapter.

Chapter III is dedicated to vegetation during the Pleistocene or ancient Quaternary.

Chapters IV and V cover a hundred pages, including the contributions of collaborators, and are entirely given over to the excavation. They are illustrated by a large number of sections with ample detailed tables which generally refer to dates after 1900, incluiding some very recent ones.

Chapter VI, very short, is an archaeological correlation of the region in which these Falls exist.

The statistical data which refer to seasonal hydrological and thermal conditions are also interesting. The period of maximum annual flows is from January to March the half-yearly period of summer rains), while the dry period dominates from October to April. The range of temperature change at the Kalambo Falls is 20° - 21°.5; and on the plateau it rises to 28°. The hottest temperature, in October, oscillates between 32° and 35°, and the rain-bearing air currents come from the Indian Ocean.

These six chapters are followed by an Appendix which contains individual articles by various collaborators. The articles present details about geology, fruits and seeds (also illustrated with tables), fossil boods, items on botany and useful plants, analyses of heavy minerals, archaeological references concerning the Iron Age, prehistorical industries, etc.

There are also 32 beautiful photographs with titles, which illustrate the Falls and the course of the Congo in the Kalambo region and the excavations carried out from 1956 to 1961.

This work, which has an extremely detailed summary and an alphabetical index, presents a wide variety of details concerning prehistory and history, on among other topics, habitations, cultivation, road networks, dormant centers of vulcanic activity, lacustrine basins, living animals, etc. Concerning, for example, the Tse Tse fly which is so dangerous to cattle, it is pointed out that it is absent in the higher areas (the Tanganyika-Nyassa Corridor) while present lower down in the hot zones.

It is thought that in prehistoric and even ancient historical times animal life was far richer than at present but that the regions of the basin where man has not worked have seen preservation from prehistoric times to the present. One of these regions is the Falls to which the author has dedicated this work, which he has provided with ample illustrative descriptions, charts and photographs.

Pisa, Università, Facoltà di Geologia.

R. Masini

B. E. Tumanian, *History of Armenian Astronomy*, Volume II, 306 pages of Armenian text and several tables, Summary in Russian and English, The Erevan Publishing House « Mitk » (Intelligence), 1968.

Vol. I was reviewed in « Scientia » IX-X-1970, pp. 668-670.

Vol. II follows the history of Armenian astronomy from the beginning of the 19th to the first quarter of the 20th century. This was a painful period for Armenia. The invasion by Persians finished in 1827 with the liberation of Yerevan by Russian troops with the active participation of Armenian volunteers. In 1828 Eastern Armenia joined Russia. Western Armenia was from the 16th century under the yoke of the Turkish Empire. Thousands of Armenians were dispersed into the world at large and formed Armenian communities in many countries. These communities became centres of Armenian culture beyond native frontiers and published scientific books and journals.

In 1840 the first Armenian astronomical observatory on the island of St. Lazare in Venice was established.

In 1895-96 more than 300,000 Armenians in Western Armenia were wiped out by the ferocious hordes of Sultan Hamid, and thousands of Armenian families became refugees. World War I increased the number of victims.

After the great October Revolution in Russia a new period in the history of culture and science began in Eastern Armenia. During the period covered by Vol. II, some remarkable works connected with astronomy were written in Armenian in many countries.

In Chapter I, devoted to problems of the calendar, the author mentions the works of Kh. Surmelian (« Ledger-Keeping », Venice, 1818) and of G. Varjapetian (« The New or Gregorian Calendar and the Armenian Holy Church », Constantinople, 1926). They established some historical data connected with reforms of the Armenian calendar.

Chapter 2 considers the cosmogonical hypothesis of N. Tagavazian and a fundamental work by H. Barseghian « Comets », where the table of all comets from the beginning of our era to 1882 observed by the naked eyes (certain in full day light: in 43, 1407, 1532, 1577, 1618, 1744, 1843, 1882, etc.) are given.

Special attention is given by the author to the activity of the Venetian scientist Kh. Sinanian, who discovered the 6th satellite of Jupiter in 1894 (10 years before Perrine). Sinanian improved Aristarchus of Samos' method for determining the relative distances between the Sun and the Moon. He took into account the refraction of the Earth's atmosphere and the influence of the Moon's parallaxis.

Chapter 3 describes the Armenian manuals. Chapter 4 deals with popular books and booklets written on a high level. In this chapter the author mentions the works of C. Flammarion, F. Arago and others which had been translated into Armenian.

Chapter 6 is very important because the author investigates here the history of Armenian scientific terminology in astronomy. Chapter 7 presents a reference to important papers in astronomy published in different journals in Armenia and in numerous Armenian cultural colonies in other countries. The first such publication appeared in Madras (India) in 1794.

The last Chapter, is devoted to a detailed analysis of very valuable astronomical records in Armenian chronicles. Perhaps this Chapter and the main part of Chapter I (Calendar problems) should be transferred to Vol. I for better chronological harmony.

A detailed list of literature and of manuscripts on the history of Armenian astronomy completes this remarkable work by B. E. Tumanian. In the Addendum the author gives the extensive (pp. 246-306) table for converting the dates in major Armenian chronology into Julian calendar dates.

Russian and English summaries (7 pp. each) are added at the end. The appearance of the Russian version (perhaps slightly condensed) is hoped for in the near future.

I should like to stress the importance of this two-volume « History of Armenian Astronomy » to all those interested in the history of world science.

Moscow, University, Sternberg Astronomical Institute.

P. G. KULIKOVSKY

G. CECCHINI, *Il cielo. Luci ed ombre nell'Universo*, 2 vols., 2nd rev. ed., Unione Tipografico-Editrice Torinese, Torino, 1969, VOL. I: XVI+718 pp., figs. and plates; VOL. II: XVI+730 pp., figs., plates and maps, L. 24.000.

It is 17 years since the publication of the first edition of this work by Professor Cecchini, now emeritus director of the Pino Torinese Observatory. During that time astronomy has made remarkable progress, and the work, in 1952 already unique of its kind, naturally had to be brought up to date in accordance with the discoveries and new results of this period, derived both from observations and from recent experimental and theoretical research.

It was unique of its kind, at least in Italy, which had no work covering the major part of what we know of the vast, immense and wonderful structure that forms the starry sky. It is often thought that astronomy can be studied only by those endowed with particular aptitudes. Though this may be true of the specialised branches of astronomy for which a knowledge of higher mathematics and advanced physics is required, one must remember that there exists within the vast field of astronomy the part which may be called descriptive, accessible to anyone having a secondary-school education. Cecchini's book, in this new edition, gives a clear and accurate account of our present knowledge of the heavens, with many fine illustrations, is an excellent one for the purpose, and should be known and valued by Italians and by those in other countries who understand Italian. Now that man has left the Earth and is beginning the exploration of space, it becomes more than ever necessary to be aware of all that is known of our universe, to increase our admiration of its unity and its unending variety and extent.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

M. Hack and O. Struve, *Stellar Spectroscopy. Normal Stars,* Osservatorio Astronomico, Trieste, 1969, 204 pp., figs., No price stated.

In her preface, Hack describes the origin of the series of books on stellar spectroscopy, of which this book is the first, dealing with normal stars. The work was suggested by the late Otto Struve, the last of a famous family of astronomers, who was director of several American observatories and under whom Hack worked. The plan of the series has undergone some changes, partly through the death of Struve and partly through changes in the subject, and it is now intended that this first part should be followed by a further three, dealing respectively with peculiar stars, pulsating stars, T Tauri stars, white dwarfs and binary stars.

The treatment of the normal stars in this book comprises the following four chapters: stellar atmospheres and methods for the study of stellar spectra; spectral classification by photographic methods and by the continuous spectrum; stellar rotation and its likely origin; quantitative analysis of stellar spectra, the curve of growth and stellar composition. Each chapter is followed by an extensive bibliography.

The purpose of the book, according to Hack, is to provide for graduate students at universities a review of the main results obtained through the study of stellar spectra. The theories of stellar atmospheres are not described in detail, but the spectra and observational facts are discussed on which such theories are based, always with reference to the fundamental problem of astrophysics, that of stellar evolution.

The book was produced at the Children's Village, Trieste, and its appearance is very attractive.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

W. M. KAULA, *An Introduction to Planetary Physics* (The Terrestrial Planets), (*Space Science Text Series*), John Wiley & Sons, Inc., London, 1968, XVIII+490 pp., figs. and tables, £6.75.

This book deals with the part of planetary physics that is dominated by solid matter. It therefore considers in detail only the terrestrial planets (Mercury, Venus, Earth, Mars) together with the asteroids, meteorites etc.

The book can be divided into five parts.

The first part, comprising the first three chapters, deals with the interiors of the planets. After a discussion of the mechanical, thermal and electromagnetic properties of the Earth, the corresponding properties of the terrestrial planets are described.

In the second part (fourth and fifth chapters) the dynamics and evolution of the solar system are examined.

The third part (sixth and seventh chapters) concerns observations of planetary surfaces either by « passive » analysis of electromagnetic radiation coming from the planets or by « active » analysis using radar.

The fourth part (eighth chapter) deals with the study of meteorites, the only extra-terrestrial solid materials available for study in the prehistoric period of space exploration when the book was written (1968!).

In the final part, consisting of the last chapter, the observations and theories regarding the origin and evolution of the terrestrial planets are summarised.

The book is intended for university courses in planetary physics, but is can be read with profit both by the non-specialist scientist seeking to augment his knowledge of a very fashionable field of research, and by the specialist in the subject to see the topic of his research against a broader background of current problems.

A systematic bibliography at the end of each chapter will be of great use to anyone wishing to pursue further the subjects discussed.

Catania, Osservatorio Astrofisico.

G. GODOLI

J. M. ZIMAN, *Elements of Advanced Quantum Theory*, Cambridge University Press, London, 1969, XII+270 pp., figs. and tables, £2.75.

If, on the one hand, the evolution of several modern sciences towards increasingly abstract forms enables them to be presented far more elegantly and synthetically, on the other hand it also makes the experimental sources ever more remote.

The effort of « mathematisation » may even act as an inhibitor of the physical intuition and critical spirit that lies at the basis of efforts of generalisation and revision.

This situation applies very particularly to the quantum theories, which are increasingly difficult to master, even for professionals.

Considerable recognition is therefore due to works such as the present volume by J. M. Ziman, who, by keeping his discourse on a fairly accessible plane, seeks to enable the reader to understand the principal concepts and methods of the subject, which he illustrates by means of simple and well-explained examples.

He goes on to discuss bosons, fermions, perturbation methods, Green functions, the n-body problem, the relativistic formulation and the theory of symmetries and groups.

The work can also be recommended to those interested in the problems involving solid state in quantum chemistry.

Milano, Politecnico.

R. Piontelli

O. Heckmann, *Theorien der Kosmologie*, Springer-Verlag, Berlin, 1968, x+114 pp., figs., DM 24.

A philosopher (and others) faced with a book such as this will ask a question that has an almost Kantian flavour: how can there be a theory of cosmology? The epistemological hypothesis on which any cosmological theory is based is the possibility of extrapolation: the assertion that mathematics alone, without empirical assistance (this comes in, if at all, at a later stage to confirm or refute a particular theory), can tell us what happens in regions of space-time of which we have no experience. (Thus we see that the problem of extrapolation is closely connected with those of induction, causality and uniformity). But, being presupposed, the possibility of extrapolation is omitted from any cosmological theory, and the conditions for it to occur are not considered. Such a procedure is clearly incorrect and misleading. Until there is a valid theory of extrapolation (which does not yet exist), there will be no yardstick whereby to test the validity of a given theory.

I have written the above because I feel that Heckmann's book, despite its many and correct discussions of « fundamental » epistemological problems, deals only in passing with this most important problem of all. The book describes the three main theories which have been put forward in cosmological research during the last sixty years: the *dynamic theory*, based on classical Newtonian mechanics with its concepts of absolute time and space, mass, force etc.; the *metric theory*, based on the relativistic ideas of Einstein and others, which makes particular use of the metric field concept; and the *kinematic theory*, put forward by Milne and based on invariance under the Lorentz transformation, so called because it uses elements of kinematics. Thus we see that by « theory of cosmology » Heckmann means, not a theory of the origin and structure of the universe, but a theory of the mathematical methods used to study this origin and structure. The book is therefore, despite the defects previously mentioned, one which will interest also the philosopher and the historian of science.

Roma, Università, Istituto di Filosofia.

A. Verdino

LIBRI RICEVUTI - BOOKS RECEIVED

Le opere elencate nella rubrica "Libri ricevuti,, non vengono raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

The works listed in the section "Books received,, are not grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

ALBERIGI QUARANTA A. and FOCARDI S. - Sommario di fisica generale.
ALLPORT G. W. - Structure et développement de la personnalité.
ARNOLD A. - Les mathématiques à la portée de l'ordinateur.
BARCHEWITZ P. - Spectroscopie atomique et moléculaire.
BARGMANN W. and SCHARRER B. (eds.) - Aspects of Neuroendocrinology.
BASTIDE R. - Le prochain et le lointain.
Beauté du Monde Animal.
BETH E. W. - Aspects of Modern Logic.
BETH E. W. - Science a Road to Wisdom.
BOCKRIS J. O'M. and REDDY A. K. N. - Modern Electrochemistry.
BÖHLKE J. E. and CHAPLIN CH. C. G. - Fishes of the Bahamas and Adjacent Tropical Waters.
BOLIS L., KATCHALSKY A., KEYNES R. D., LOEWENSTEIN W. R. and PETHICA B. A. (eds.) - Permeability and Function of Biological Membranes.
BORST C. V. (ed.) - The Mind-Brain Identity Theory.
BOURNE G. H. (ed.) - The Chimpanzee.
BRIDGMAN P. W. - La critica operazionale della scienza.
BRODY B. A. (ed.) - Readings in the Philosophy of Science.
BURCHETT W. - La guerra di popolo dell'Indocina.

Burdese A. - Manuale di metallurgia.
Carboni P. - Dizionario tecnico-scientifico Tedesco-Italiano e Italiano-Tedesco.
Cassirer E. - Determinismo e indeterminismo nella fisica moderna.
Ceccato S. - Cibernetica per tutti.
Ciacaglia G. E. O. (ed.) - Periodic Orbits, Stability and Resonances.
Cicha I. - Stratigraphical Problems of the Miocene in Europe.
Crombie A. C. - Da S. Agostino a Galileo.
Crompton D. W. T. - An Ecological Approach to Acanthocephalan Physiology.
Cullmann G. - Recherche opérationnelle.
Daboni L. - Calcolo delle probabilità ed elementi di statistica.
D’Ans-Lax - Taschenbuch für Chemiker und Physiker.
De Finetti B. - Teoria delle probabilità.
De Jager C. (ed.) - Reports on Astronomy.
Dejussieu-Pontcarral P. (ed.) - Encyclopédie de l’électricité.
Deterline W. A. - Introduzione all’istruzione programmata.
Duro F. - Analisi chimica quantitativa.
Eidmann H. and Kühlhorn F. - Lehrbuch der Entomologie.
Espir M. L. E. and Rose F. C. - The Basic Neurology of Speech.
Essler W. K. - Induktive Logik.
Essler W. K. - Wissenschaftstheorie I.
Filippi F. (ed.) (E. Perucca founder) - Dizionario di Ingegneria.
Fino J.-F. - Forteresses de la France médiévale.
Florescu F. B. - Die Trajanssäule.
Florkin M. and Stotz E. H. (eds.) - Comprehensive Biochemistry.
Fouchier J., Billet F. and Epstein H. - Chemical Dictionary.
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Siefkes D. - Büchi's Monadiac Second Order Successor Arithmetic.
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The Insects of Australia.
Valzelli L. - Elementi di psicofarmacologia sperimentale e clinica.
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Weingartner P. and Zecha G. (eds.) - Induction, Physics, and Ethics.
Wilson L. G. (ed.) - Sir Charles Lyell's Scientific Journals on the Species Question.
Zapparoli G. C. - La perversione logica.

ALBERIGI QUARANTA, A. and FOCARDI, S., **Sommario di fisica generale**, Casa Editrice Ambrosiana, Milano, 1970, XII+212 pp., figs. and tables, L. 2.500.

Contents. - *I: Analisi vettoriale. - II: Fluttuazioni statistiche e teoria degli errori. - III: Meccanica. - IV: Elasticità. - V: Oscillazioni. - VI: Onde. - VII: Fluidi. - VIII: Termodinamica. - IX: Teoria cinetica dei gas perfetti. - X: Elettrostatica. - XI: Correnti elettriche stazionarie. - XII: Campo magnetico. - XIII: Forze elettromotrici indotte. - XIV: Circuiti in correnti alternate. - XV: Equazioni di Maxwell e onde elettromagnetiche. - XVI: Conduzione elettrica nel vuoto e nei metalli. - XVII: Dielettrici. - XVIII: Proprietà magnetiche della materia. - XIX: Le onde elettromagnetiche nei mezzi materiali. - XX: Ottica geometrica. - XXI: Interferenza. - XXII: Diffrazione. - XXIII: Spettroscopi. - XXIV: Irraggiamento.* - Appendice.

ALLPORT, G. W., **Structure et développement de la personnalité** (Series: *Actualités Pédagogiques et Psychologiques*), Delachaux et Niestlé, Neuchatel, 1970, 504 pp., figs., SFr. 45.

G. W. Allport, professeur à l'Université de Harvard (USA), présente dans cet ouvrage les résultats de 40 années de recherches sur la personnalité. D'accès facile, cet ouvrage est destiné à tous ceux qui tentent, même sans connaissances particulières en matière de psychologie, de résoudre l'énigme de la personnalité humaine. Ce livre, qui se présente comme un compte rendu global et pourtant éclectique des recherches scientifiques sur la personnalité, fournit également une théorie coordinatrice. G.W. Allport attire particulièrement l'attention sur le fait que: ***Exactement comme la biologie morphogénique est en retard sur la biologie analytique et moléculaire, la psychologie morphogénique est en***

retrait par rapport à la psychologie analytique et moléculaire. En outre, l'étude de la psychologie constitue une option sur la philosophie de la personne et invite à tenir compte des conséquences philosophiques qu'entraîne l'adoption d'une interprétation aux dépens d'une autre: *Ma propre conception est que, pris dans l'ensemble, les indices dont nous disposons ne dépeignent pas l'homme comme un robot réactif. Ils nous amènent plûtot à le concevoir comme un être muni d'un potentiel unique de développement. La plupart de ses virtualités ne sont jamais réalisées et, jusqu'à ce que nous les connaissions mieux, elles ne verront pas le jour.*

ARNOLD, A., **Les mathématiques à la portée de l'ordinateur** (Series: *Initiation aux nouveautés de la Science*, No. 22), Dunod, Paris, 1970, x+158 pp., figs. and tables, 29 F.

CONTENTS. - *I: Les mathématiques et la logique. - II: Les mathématiques et les systèmes formels. - III: Description d'un langage de formalisation (1). - IV: Description d'un langage de formalisation (2). - V: Questions ouvertes. - VI: Le travail de la machine.*

BARCHEWITZ, P., **Spectroscopie atomique et moléculaire,** VOL. I: **Mécanique de l'atome et de la molécule, spectroscopie atomique,** Masson & C.ie, Paris, 1970, 212 pp., figs. and tables, 48 F.

CONTENTS. - I. Introduction à la mécanique quantique de l'atome et de la molécule. *I: Rappel des notions fondamentales de la mécanique ondulatoire et de mécanique quantique. - II: Méthodes d'approximation. - III: Interaction rayonnement-matière.* - II. Atomistique. *I: Spectroscopie de l'atome à un électron. - II: Action d'un champ magnétique et d'un champ électrique. - III: Spectroscopie de l'atome à deux électrons. - IV: Spectroscopie des atomes alcalins. - V: Termes électroniques et termes atomiques.*

BARGMANN, W. and SCHARRER, B. (eds.), **Aspects of Neuroendocrinology** (V. International Symposium on Neurosecretion, August 20th-23rd, 1969, Kiel), Springer-Verlag, Berlin, 1970, XII+380 pp., figs. and tables, DM 98.

CONTENTS. - *I: Neurosecretion in Invertebrates. - II: Adrenergic Neurons. - III: Mechanisms of Release of Neurohypophyseal Materials. - IV: Hypothalmic Control of Anterior Pituitary. - V: Various Problems of Neurosecretion.*

BASTIDE, R., **Le prochain et le lointain,** Editions Cujas, Paris, 1970, 302 pp., 32 F.

CONTENTS. - *I: La rencontre des hommes. - II: La rencontre des civilisations. - III: L'orage mystique.*

Beauté du Monde Animal, VOL. I: **Mammifères 1,** Librairie Larousse, Paris, 1970, 320 pp., figs., 59 F.

A l'heure où, dans le monde entier, la protection de la nature est à l'ordre du jour, nous sommes tous, jeunes ou adultes, habitants des villes ou des campagnes, concernés par les animaux. Aussi faut-il accorder une importance toute particulière à la publication chez Larousse d'une nouvelle collection, *Beauté du Monde Animal,* dont le premier volume, «*Mammifères 1*», consacré aux singes et lémuriens, aux insectivores, aux chauves-souris et aux rongeurs, vient de paraître. Cette collection, qui comprendra 11 volumes, constitue, en particulier pour les jeunes, la plus facile et la plus attrayante exploration du monde animal. Ses photographies spectaculaires, *toutes en couleurs à chaque page* (la collection complète comportera plus de 5.000 illustrations en couleurs), formeront par leur ensemble le plus extraordinaire safari-photo à domicile. Enfin, la lecture de *Beauté du Monde Animal* est à conseiller à tous ceux qui rêvent d'évasion, car ce magnifique ouvrage est une porte ouverte sur un monde souvent cruel, mais naturel, équilibré et sain, dont nous gardons tous la nostalgie. Il répond à notre besoin

d'échapper à l'univers concentrationnaire du XX° siècle, en nous faisant mieux connaître les animaux, pour mieux les aimer et pour mieux comprendre le rôle essentiel qu'ils jouent dans l'équilibre de la Nature.

BETH, E. W., **Aspects of Modern Logic** (Series: *Synthese Library*), D. Reidel Publishing Company, Dordrecht, 1970, XII+176 pp., Dfl. 42.

CONTENTS. - Part I. - *I: The Fundamental Criterion for the Soundness of Arguments.* - *II: Inferential and Classical Logic.* - *III: Proof by Contradiction.* - *IV: The Problem of Locke-Berkeley.* - *V: On the So-Called 'Thought Machine'.* - Part II. - *VI: The Paradoxes.* - *VII: Reason and Intuition.* - *VIII: Formalized Language and Common Usage.* - *IX: Considerations about Logical Thought.* - *X: Constants of Mathematical Thought.*

BETH, E. W., **Science a Road to Wisdom** (Collected Philosophical Studies), D. Reidel Publishing Company, Dordrecht, 1968, XIV+124 pp., Dfl. 28.

In this book Professor Beth deals with such widely divergent topics as the relation between metaphysics and science, the fatal influence of Aristotle's conception of science – characterized especially by the 'evidence postulate' – on traditional philosophy, the notion of causality, the alleged influence of mathematics on modern art, and the crisis of foundations, which has been instrumental in engendering *cultural fatalism.* This pessimism as to the viability of our civilization is found to be baseless. « On the contrary: abandonment of principles once considered unassailable is the price to be paid for the progress of science ». The author argues that "the 'foundational crisis' is really a crisis of prejudices, albeit respectable ones". These essays may be considered studies in a modern rationalism which has no more than a name in common with traditional philosophical rationalism. Professor Beth was convinced that such a modern rationalism is of vital importance, not only for scientific and philosophical discussion, but also for the continuance and development of democracy.

BOCKRIS, J. O'M. and REDDY, A. K. N., **Modern Electrochemistry,** (An Introduction to an Interdisciplinary Area), VOL. I and II, Macdonald, London, 1970, CXV+1432 pp., figs. and tables, £8.25 and £9.50.

Professor Bockris and Dr Reddy have collaborated to produce a comprehensive new text on electrochemistry, suitable for advanced undergraduate courses and graduate studies. The book is comprised of two volumes throughout which basic concepts are stressed before proceeding to advanced theory. The first volume discusses modern developments in the electrochemistry of ionic solutions including an advanced theory of solvation, the theory of transport phenomena and current views on the structure of molten salts, glasses and slags. The second volume contains a coherent analysis of the study of electrode kinetics dealing with the theory of interfacial charge transfer at a molecular level. It includes discussions of the structure of the double layer in terms of water molecules, the quantum mechanics of electron transfer processes, theory of multi-stop reactions, stability of metals, electrochemical energy conversion and storage. Special attention is given to the applications of electrochemical energy conversion and storage (fuel cells), corrosion and metal stability, extraction of metals from their ores, molecular biology, understanding of potential differences at the surfaces of membranes, and treatment of sea-water.

BÖHLKE, J. E. and CHAPLIN, CH. C. G., **Fishes of the Bahamas and Adjacent Tropical Waters,** Livingston Publishing Company, Wynnewood, 1968, XXXII+772 pp., figs. and plates, $29.50.

Fishes of the Bahamas is the first comprehensive work of fishes of the West Atlantic in more than 30 years. With the exception of Longley and Hildebrand's 1941 book on the fishes of the Tortugas, students have had to rely on Beebe and Tee-Van's *Shore Fishes of Bermuda,* published in 1933 and long out of print, and on two works of the last century (*Fishes of North and Middle America,* by Jordan and Evermann and *The Fishes and Fisheries of Puerto Rico*). Since the publication of these earlier works much new information has been gathered, and interest in the natural history of the Caribbean area has increased geometrically. Fast, economical transportation and greatly improved accommodations have drawn an ever-increasing number of skin divers, snorkelers and fishermen to the West Indies, in addition to serious ichthyologists. This new book has been designed to aid the professional and amateur alike in identifying the fishes from Bermuda to Brazil. There are 32 color plates, including 19 from paintings by Fritz Janschka and more than 700 detailed black-and-white drawings by Steven Gigliotti. In addition to these aids to identification, the book has many complete keys to families, genera and species.

BOLIS, L., KATCHALSKY, A., KEYNES, R. D., LOEWENSTEIN, W. R. and PETHICA, B. A. (eds.), **Permeability and Function of Biological Membranes** (Proceedings of the 1969 Meeting of the International Conference on Biological Membranes, NATO Advanced Study Institute), North-Holland Publishing Company, Amsterdam, 1970, x+364 pp., figs. and tables, Dfl. 51.

While numerous meetings were devoted to the chemical structures and organization of protoplasmatic membranes and intercellular reticulum, less attention has been paid to cell surfaces. This conference considered the mechanical properties of cellular covers, and devoted much of its time to the surface forces, intercellular contacts and to cell communication. Since it is realized that the surfaces of cells are not geometrical boundaries between phases but active structures endowed with immunological properties and responsible for cellular recognition, several sessions were devoted to the interaction of the membranes with anesthetics, to their immune reactions, their antigen recognition as well as to the role played by cell membranes in histocompatibility. In an attempt to interpret biomembrane behavior in physical terms, a couple of sessions were devoted to the thermodynamics analysis of membrane transport and to a discussion of the physical chemistry of monolayers and bilayers, liposomes and black films as models for membrane behavior. The last part of the meeting was devoted to the ever-exciting field of excitable membranes.

BORST, C. V. (ed.), **The Mind-Brain Identity Theory** (A Collection of Papers), Macmillan & Co. Ltd., London, 1970, 262 pp., 18s.

CONTENTS. - I. Statements of the Theory. *I: Mind-Body, not a Pseudo-Problem*, by H. FEIGL. - *II: Is Consciousness a Brain Process?*, by U. T. PLACE. - *III: Sensations and Brain Processes*, by J. J. C. SMART. - *IV: The Nature of Mind*, by D. M. ARMSTRONG. - II. Initial Criticism and Clarification. *V: Materialism as a Scientific Hypothesis*, by U. T. PLACE. - *VI: 'Sensations and Brain Processes': A reply to J. J. C. Smart*, by J. T. STEVENSON. - *VII: Further Remarks on Sensations and Brain Processes*, by J. J. C. SMART. - *VIII: Smart on Sensations*, by K. BAIER. - *IX: Brain Processes and Incorrigibility*, by J. J. C. SMART. - III. Location and Leibniz's Law. *X: Could Mental States be Brain Processes?*, by J. SHAFFER. - *XI: The Identity of Mind and Body*, by J. CORNMAN. - *XII: Shaffer on the Identity of Mental States and Brain Processes*, by R. COBURN. - *XIII: Mental Events and the Brain*, by J. SHAFFER. - *XIV: Comment: 'Mental Events and the Brain'*, by P. FEYERABEND. - *XV: Materialism and the Mind-Body Problem*, by P. FEYERABEND. - IV. The Smart-Malcolm Symposium on Materialism. *XVI: Materialism*, by J. J. C. SMART. - *XVII: Scientific Materialism and the Identity Theory*, by N. MALCOLM. - *XVIII: Professor Malcolm on 'Scientific Materialism and the Identity Theory*, by E. SOSA. - *XIX: Rejoinder to Mr. Sosa*, by N. MALCOLM. - V. Further Contributions to the Controversy. *XX: Mind-Body Identity, Privacy and Categories*, by R. RORTY. - *XXI: Physicalism*, by T. NAGEL. - *XXII: Mind-Body Identity, a Side Issue?*, by CH. TAYLOR. - *XXIII: Illusions and Identity*, by J. M. HINTON.

BOURNE, G. H. (ed.), **The Chimpanzee**, 3 vols., S. Karger, Basel, 1969/1970, VOL. 1: **Anatomy, Behavior and Disease of Chimpanzees**, xx+466 pp., figs. and tables, SFr. 115; VOL. 2: **Physiology, Behavior, Serology and Diseases of Chimpanzees**, x+418 pp., figs. and talbes, SFr. 115; VOL. 3: **Immunology, Infections, Hormones, Anatomy and Behavior**, x+394 pp., figs. and tables, SFr. 115.

The Chimpanzee will consist of a series of volumes dealing with the anatomy, neurology, physiology, welfare, diseases etc., of this animal. Together all the volumes will constitute a comprehensive study of this species. These subjects will not be segregated in single volumes, but each volume will have a selection of chapters dealing with a variety of aspects of the study of the chimpanzee. Volume 1 contains introductory chapters by Dr. W. C. Osman-Hill on the history and taxonomy of chimpanzees, anatomical chapters by Dr. A. Schultz, Dr. W. M. Krogman and Dr. J. Keleman. A chapter on nesting behavior is written by Dr. I. Bernstein and studies on a home reared chimpanzee by Dr. W. Kellog. Dr. R. E. Rewell deals with intestinal diseases and Dr. R. S. Bray with malaria in chimps, Dr. H. Rothberg on the effects of whole body radiation on these animals, and Drs. Manocha and Shanta contribute a chapter on the neurology of the chimpanzee. Volume 2 covers a wide range of subjects. It has chapters on vision, kidney functions, metabolism and temperature, mycoplasmas and viruses, behavior, neuroanatomy, taste, serology, and cytogenetics. There is also a section on postural propulsive and prehensible capabilities of Great Apes. The fifteen chapters in the volume are written by a distinguished group of authors and represent an important contribution to our knowledge of the chimpanzee. Volume 3 deals with aspects of the anatomy of the chimpanzee – including the histology of the eye and the skin; with behavior and communication – including experimentally modified behavior with infections of chimpanzees (hepatitis, filariasis and syphilis), with immunology and organ transplants, and with steroid hormones and reproductive physiology. Many of the subjects discussed in this volume are not well known in relation to chimpanzees and will naturally be of great interest.

BRIDGMAN, P. W., **La critica operazionale della scienza** (Series: *Biblioteca di Cultura Scientifica*), Boringhieri Editore, Torino, 1969, 454 pp., L. 6.000.

CONTENTS. - *I: Seconda reazione di un fisico alla teoria degli insiemi. - II: Idee dal campo della fisica. - III: Strumenti e procedimenti intellettuali. - IV: L'analisi operazionale. - V: Principi generali dell'analisi operazionale. - VI: Implicazioni del punto di vista operazionale. - VII: L'aspetto operazionale del significato. - VIII: Stato attuale dell'operazionalismo. - IX: Comprensione e descrizione. - X: Parole, significati e analisi linguistica. - XI: Ulteriori considerazioni metodologiche. - XII: Logica, probabilità e questioni connesse. - XIII: Alcuni aspetti delle scienze fisiche. - XIV: Quanto rigore è possibile in fisica?*

BRODY, B. A. (ed.), **Readings in the Philosophy of Science,** Prentice-Hall, Inc., Englewood Cliffs, 1970, XVIII+638 pp., No price stated.

This anthology is divided into three sections: Section I concerns scientific explanation and prediction; Section II, the structures and function of scientific theories; and Section III, the confirmation of scientific hypotheses. These problems have been chosen because they are clearly central to the philosophy of science. While further sections could have been added, it seemed more desirable to treat the central problems at some depth rather than to treat every problem superficially. Three principles have guided the author in choosing the selections: (a) as far as possible, every major position on a particular issue should be represented; (b) the various selections, besides presenting their own point of view, should also analyze other points of view; (c) selections should presuppose as little philosophy, logic, and science as is possible.

BURCHETT, W., **La guerra di popolo dell'Indocina** (Laos e Cambogia 1970), Edizioni Jaca Book, Milano, 1970, XVI+202 pp., L. 1.800.

W. Burchett, già autore di molte opere sui paesi del sud-est asiatico, continua in questo libro la sua informazione sulla situazione indocinese. Il suo racconto si vale di fonti originali: contatti personali coi protagonisti dei movimenti di liberazione nazionali, partecipazione agli avvenimenti, possibilità di usare dei bollettini di guerra di tutte le parti e infine possibilità di seguire da vicino le reazioni dell'opinione pubblica americana.

BURDESE, A., **Manuale di metallurgia,** Unione Tipografico-Editrice Torinese, Torino, 1969, VIII+426 pp., figs. and tables, L. 8.000.

La scienza dei metalli ha fatto negli ultimi decenni significativi progressi, tanto che oggi non è pensabile se non un'impostazione razionale e rigorosamente scientifica dei problemi metallurgici. Nel settore siderurgico nuove tecniche stanno profondamente modificando il quadro classico, da tempo sostanzialmente statico, della fabbricazione dell'acciaio. Di questa situazione si è cercato di tener conto nella stesura del « *Manuale di Metallurgia* », che deve essere riguardato come un tentativo di esporre con una trattazione rapida, incisiva e ragionata le nozioni oggi indispensabili anche a chi, nella professione, non deve affrontare problemi metallurgici strettamente specifici. Per chi si dedichi in modo particolare a questo campo di studi il « *Manuale di Metallurgia* » potrà costituire una base ed uno stimolo per un ulteriore approfondimento della materia.

CONTENTS. - Premessa: *Metalli e leghe metalliche.* - *I: Struttura dei metalli.* - *II: Metallografia rontgenografica.* - *III: Metallografia ottica e prove non distruttive sui metalli.* - *IV: Proprietà dei materiali metallici.* - *V: Teoria dei diagrammi di stato dei sistemi metallici.* - *VI: Diagrammi di stato di interesse pratico in metallurgia.* - *VII: Trattamenti termici degli acciai e delle leghe metalliche.* - *VIII: Trattamenti di indurimento superficiale.* - *IX: Acciaio.* - *X: Ghisa.* - *XI: Rame e leghe di rame.* - *XII: Metalli e leghe leggere.* - *XIII: Zinco, cadmio, mercurio.* - *XIV: Piombo, stagno ed elementi dei V gruppo.* - *XV: Metalli speciali.* - *XVI: Metalli preziosi.* - *XVII: Metalli alcalini e alcalino-terrosi.* - *XVIII: Lantanidi e attinidi.*

CARBONI, P., **Dizionario tecnico-scientifico Tedesco-Italiano e Italiano-Tedesco,** Casa Editrice Prof. Riccardo Pàtron, Bologna, 1965-1970, 706 and 752 pp., L. 7.000 and L. 8.000.

CASSIRER, E., **Determinismo e indeterminismo nella fisica moderna,** La Nuova Italia Editrice, Firenze, 1970, XVI+320 pp., L. 3.500.

CONTENTS. - *I: Considerazioni storiche preliminari.* - *II: Il principio causale della fisica classica.* - *III: Causalità e probabilità.* - *IV: I problemi della causalità nella teoria dei quanti.* - *V: Causalità e continuità.*

CECCATO, S., **Cibernetica per tutti,** 2 vols., Feltrinelli Editore, Milano, 1970, VOL. I: **I modelli meccanici del nostro pensiero, del linguaggio, di tutte le attività « superiori »,** 284 pp.,; VOL. II: **Gli orizzonti che un modello della mente offre alle scienze dell'uomo,** 240 pp., L. 900 and L. 1.000.

Questi due volumi raccolgono gli articoli che Silvio Ceccato ha pubblicato su « Il Giorno » dal 1964 al 1967 e parte di scritti apparsi durante lo stesso periodo su altre pubblicazioni. È stato quindi eseguito un « montaggio » compositivo al fine di dare all'esposizione un carattere più ordinato e lineare di quanto sarebbe risultato dalla pura e semplice sequenza in ordine cronologico.

CONTENTS. - **Vol. I.** - *I: Le tre cibernetiche: automazione, bionica, cibernetica della mente.* - *II: La tradizione filosofica.* - *III: L'attività mentale. Attenzione, categorizzazione, osservazione.* - *IV: La memoria.* - *V: Pensiero e linguaggio.* - *VI: Linguaggio scientifico e linguaggio comune.* - *VII: Gli atteggiamenti.* - *VIII: Estetica.* - *IX: La creatività.* - *X: Pedagogia e didattica.* - *XI: Note e commenti.* - *XII: Problemi della ricerca.* - **Vol. II.** - *I: L'errore filosofico.* - *II: Informazione e conoscenza.* - *III: Macchine verosimili e inverosimili.* - *IV: Macchine utili.* - *V: Il cibernetico a consulto.* - *VI: Scienza e consapevolezza operativa.* - *VII: La nuova didattica ed il sussidiario.* - *VIII: Questioni di aritmetica e di grammatica.* - *IX: L'istruzione programmata.* - *X: Note e commenti.* - *XI: Università.* - *XII: Il futuro dell'informazione.*

CIACAGLIA, G. E. O. (ed.), **Periodic Orbits, Stability and Resonances** (Proceedings of a Symposium Conducted by the University of São Paulo, the Technical Institute of Aeronautics of São José Dos Campos, and the National Observatory of Rio de Janeiro, at the University of São Paulo, São Paulo, Brasil, 4-12 September, 1969), D. Reidel Publishing Company, Dordrecht, 1970, XIV+530 pp., figs. and tables, Dfl. 95.

The purpose of this symposium was to congregate scientists from all over the world to discuss one of the most exciting topics in celestial mechanics. The problem of resonance and stability is closely related to that of the evolution of the solar system. It is a physically involving problem, and the mathematical methods available today seem unsatisfactory to produce non-linear ways of attack. The linearization process in both subjects is clearly of doubtful significance, so that numerical solutions are still the best and most valuable sources of information. The papers in the present work contribute a step forward to the comprehension of the problem concerned.

CICHA, I., **Stratigraphical Problems of the Miocene in Europe,** Geofond, Praha, 1970, 134 pp., figs. and plates, Kcs. 35.

CONTENTS. - *I: Introduction. - II: The Existing Stratigraphical Division of the Miocene. - III: Critical Remarks Concerning the Individual Stages of the Oligocene and Neogene. - IV: The Establishment of Neostratotypes and the Development of an open Chronostratigraphical Scale. - V: Foundations of the Biostratigraphical Division of the Miocene and Upper Oligocene. - VI: Outline of the Stratigraphy of the Upper Oligocene and Miocene in Europe. - VII: Correlation of Beds of the European Upper Oligocene and Neogene. - VIII: Notes on the Interrelationship of Upper Oligocene and Miocene Sedimentary Areas in Europe. - IX: Correlation of the European Upper Oligocene and Miocene with the Main World Bioprovinces of the Tertiary. - X: Conclusion and Remarks on the Absolute Age of Rocks. - XI: References. - XII: Czech Summary (Stratigrafické Problémy Miocénu Evropy).*

CROMBIE, A. C., **Da S. Agostino a Galileo** (Storia della Scienza dal V al XVII secolo), Feltrinelli Editore, Milano, 1970, VIII+590 pp., figs., L. 5.000.

CONTENTS. - *I: La scienza nell'Occidente cristiano fino alla rinascita del XII secolo. - II: La diffusione della scienza greco-araba nell'Occidente cristiano. - III: Il pensiero scientifico nel XIII secolo. - IV: Tecnica e scienza nel Medioevo. - V: Metodo scientifico e sviluppi della fisica nel tardo Medioevo. - VI: La rivoluzione nel pensiero scientifico del XVI e XVII secolo.*

CROMPTON, D. W. T., **An Ecological Approach to Acanthocephalan Physiology** (*Cambridge Monographs in Experimental Biology*, No. 17), Cambridge University Press, London, 1970, VIII+126 pp., figs., and tables, £2.80.

Acanthocephalan worms are parasitic throughout their life cycles and the environments they experience are to be found in the bodies of vertebrates, in which they achieve sexual maturity, and in the tissues of arthropods, in which they develop. This monograph aims to relate present knowledge of acanthocephalan physiology to the physical and biotic factors occurring in the environments occupied at different stages of the life cycle. This theme is illustrated by considering feeding, metabolism and reproduction of adult worms, certain aspects of the physiology of the eggs and developmental stages and the processes of infection of the hosts. The author not only indicates subjects requiring further work and those awaiting investigation, but also emphasizes the necessity of culturing acanthocephalans in *vitro* if their biology is to be understood in detail.

CULLMANN, G., **Recherche opérationnelle** (Théorie et pratique), Masson & C.ie, Paris 1970, VI+244 pp., figs. and tables, 80 F.

Le présent ouvrage s'adresse aussi bien aux étudiants, y compris ceux s'intéressant aux Sciences humaines, qu'aux Ingénieurs et aux Techniciens. Il n'est pas exigé du lecteur un niveau mathématique élevé, des exemples permettant de saisir rapidement l'esprit des méthodes pour pouvoir juger de leur intérêt et passer plus facilement à des applications pratiques.

DABONI, L., **Calcolo delle probabilità ed elementi di statistica,** Unione Tipografico-Editrice Torinese, Torino, 1970, XVI+388 pp., figs., L. 9.500.

L'opera, che si distingue per l'ampiezza e per l'equilibrio con cui sono presentati gli aspetti teorici e le questioni applicative, si propone di offrire al lettore una guida per lo studio dei fondamentali problemi e metodi del *calcolo delle probabilità* e delle sue applicazioni in campo economico e ingegneristico, nonchè alcuni spunti per l'approccio di problemi inferenziali nell'ambito della teoria delle decisioni.

CONTENTS. - I. Calcolo delle probabilità. *I: Nozioni fondamentali sugli eventi. - II: Probabilità. - III: Variabili aleatorie unidimensionali. - IV: Variabili aleatorie multiple. - V: Particolari distribuzioni. - VI: Teoremi limite del calcolo delle probabilità. - VII: Processi stocastici discreti a parametro discreto. - VIII: Catene markoffiane finite. - IX: Processi stocastici a parametro continuo.* - II. Elementi di statistica. *X: Procedimenti inferenziali. - XI: Procedimenti decisionali bayesiani.*

D'ANS-LAX, **Taschenbuch für Chemiker und Physiker,** 3rd ed., VOL. III: **Eigenschaften von Atomen und Molekeln,** Springer-Verlag, Berlin, 1970, VIII+670 pp., figs., DM 48.

CONTENTS. - *I: Grundkonstanten und Kerneigenschaften. - II: Periodensysteme und Spektren. - III: Grösse und elektronischer Aufbau von Atomen, Ionen und Molekeln. - IV: Elektrische und optische Konstanten. - V: Zusammenhang der molekularen Eigenschaften mit makroskopischen Phänomenen. - VI: Verzeichnis der Spektrallinien für analytische Zwecke.*

DE FINETTI, B., **Teoria delle probabilità** (Sintesi introduttiva con appendice critica), 2 VOLS., (Series: *Nuova Biblioteca Scientifica Einaudi,* No. 25), Giulio Einaudi Editore, Torino, 1970, XIV+348 and VIII+422 pp., figs., L. 12.000.

Quest'opera sviluppa integralmente una trattazione della teoria delle probabilità in tutti i suoi aspetti e con riferimento a tutte le applicazioni, sulla base, strettamente e coerentemente seguita, della concezione soggetivistica della probabilità. Le discussioni pro o contro le varie concezioni in fatto di probabilità (e le varianti sono molto più numerose e sfumate di quanto risulta da tali brevi cenni) sono interminabili e inestricabili, ma l'opera di De Finetti, non si propone di limitarsi ad esse, ma al contrario vuole esporre in modo coerente il calcolo delle probabilità e i suoi campi di applicazione, entro i limiti di una trattazione essenziale e riassuntiva. Tutto è ispirato a tale scopo di coerenza, per evitare oscurità ed equivoci derivanti dall'impiego promiscuo ed ambiguo di termini (o di significati per lo stesso termine) attinenti a concezioni diverse e contraddittorie. Particolari cure l'autore ha dedicato alla terminologia, richiamando l'attenzione (con Jeffreys) sulle insidie del linguaggio comune, opera di realisti (anzi di realisti molto primitivi), e quindi inadatto ad esprimere concetti soggettivi. La trattazione di argomenti matematici è sviluppata in modo esteso nelle parti iniziali e in quelle delicate per la comprensione dei fondamenti e del significato concettuale e applicativo della teoria. Nello studio di particolari argomenti gli sviluppi matematici sono limitati: l'autore ha cercato di mettere in luce l'essenziale nel modo più perspicuo ma evitando di portare a termine dimostrazioni lunghe e macchinose che rischiano di far perder di vista, tra gli artifici tecnicistici, il concetto informatore che merita di essere impresso nella mente. Analoga cura è posta nel discutere le implicazioni filosofiche spesso ravvisate nella teoria della probabilità o in certe sue applicazioni. La conclusione sostenuta – che si potrà dire filosofica o forse antifilosofica – è quella della totale neutralità della teoria delle probabilità (della logica dell'incertezza, come dell'altra logica, la logica ordinaria o della certezza) da ogni dato di fatto e da ogni concezione scientifica o filosofica.

DE JAGER, C. (ed.), **Reports on Astronomy** (Transactions of the International Astronomical Union, Vol. XIV/A), D. Reidel Publishing Company, Dordrecht, 1970, VIII+566 pp., tables, Dfl. 65.

The *Reports on Astronomy* contain essentially the tri-annual progress reports of the Presidents of the 38 Commissions of the IAU. These reports therefore represent a unique and complete overall review of the whole field of astronomy in all its fascinating aspects. Some of the Commission reports deal with 'classical' subjects like: 'Celestial mechanics', 'Positional astronomy', but also these disciplines have all undergone the

touch of latest developments, like the introduction of computing techniques, automatisation, application to artificial satellites. Other Commission reports deal with astrophysical subjects like 'Solar activity', 'Stellar spectra', 'Stellar constitution' and many others. Furthermore, there are Commission reports with technical aspects: 'Instruments and techniques', 'Radio astronomy', 'Space research'; others covering theoretical disciplines like: 'Magnetohydrodynamics and the physics of ionized gases', and there are Commission reports dealing with the 'History of astronomy', the 'Teaching of astronomy'. The *Reports on Astronomy* are essentially a combined undertaking in which nearly all of the 2000 members of the IAU participate, since the Reports are written by the Commission Presidents, on the basis of information collected by them from among the members of the various Commissions. The *Reports on Astronomy* appeared for the first time in 1922 under the title 'Transactions of the International Astronomical Union', and have since appeared regularly in connection with the tri-annual General Assemblies of the International Astronomical Union. The present Report is the fourteenth in this series.

DEJUSSIEU-PONTCARRAL, P. (ed.), **Encyclopédie de l'électricité** (Puissance, information, électronique), VOL. 2: **Applications**, Librairie Larousse, Paris, 1971, 1022 pp., figs. and plates, 152 F.

Cette encyclopédie scientifique et pratique, dont nous avons déjà annoncé dans nos pages la parution du Tome 1, s'adresse à tous les techniciens, ingénieurs, spécialistes de l'électricité et de l'électronique, mais elle a aussi sa place dans tous les lycées techniques, les écoles d'électricité et d'électronique, les centres d'apprentissage. Elle servira également d'instrument de travail et de référence aux industriels et à tous ceux qui ont des professions « technico-commerciales ». Ce second volume (« Applications »), qui vient de paraître, décrit toutes les utilisations pratiques de l'électricité: les techniques spéciales (des ultrasons aux explosions atomiques); les télécommunications; l'éclairage électrique; l'électricité comme élément de travail (au bureau) et de détente (sonorisation des salles de spectacle; magnétophones, électrophones,....); les apports de l'électricité à la médecine humaine; l'électricité en agriculture; les jouets; les calculateurs électroniques; les applications scientifiques (des accélérateurs de particules aux microscopes électroniques); les transports; l'industrie et les fabrications; les installations électriques de la maison,... L'ouvrage s'achève par un exposé des réalisations industrielles de demain. Les deux tomes sont très abondamment illustrés en couleurs et en noir.

DETERLINE, W. A., **Introduzione all'istruzione programmata,** Zanichelli Editore, Bologna, 1970, XVI+148 pp., L. 1.800.

CONTENTS. - *I: Introduzione.* - *II: Pressey e Skinner.* - *III: I princìpi dell'apprendimento.* - *IV: Variazioni nei programmi.* - *V: Risultati sperimentali.* - *VI: Insegnanti, studenti e auto-istruzione.* - Bibliografia. - Appendice: *Istruzioni. Esempi di programmi.*

DURO, F., **Analisi chimica quantitativa** (Con particolare riferimento ai prodotti iscritti nella farmacopea ufficiale italiana), 3rd ed., Casa Editrice Prof. Riccardo Pàtron, Bologna, 1970, XIV+334 pp., figs. and tables, L. 4.500.

CONTENTS. - *I: Analisi ponderale.* - *II: Metodi di analisi ponderale.* - *III: Analisi volumetrica.* - *IV: Metodi di neutralizzazione.* - *V: Metodi di analisi volumetrica. Alcalimetria.* - *VI: Metodi di precipitazione.* - *VII: Metodi di ossido-riduzione.* - *VIII: Metodi per estrazione.* - *IX: Costanti di grassi, olii grassi, cere, acidi grassi, balsami, resine, etc.* - *X: Alcaloidi.* - *XI: Peso specifico o densità.* - *XII: Metodi elettrometrici.* - *XIII: Rifrattometria.* - *XIV: Potere rotatorio.* - *XV: Spettrofotometria.* - *XVI: Metodi gasometrici.*

EIDMANN, H. and KÜHLHORN, F., **Lehrbuch der Entomologie,** 2nd ed., Verlag Paul Parey, Hamburg, 1970, 634 pp., figs., DM 68.

CONTENTS. - *I: Die systematische Stellung der Insekten.* - *II: Die Grundzüge der Organisation des Insektenkörpers.* - *III: Habitus, Grösse und Zahl der Insekten.* - *IV:*

Morphologie des Insektenkörpers und seiner Anhänge. - V: Die Organe des Insektenkörpers und ihre Leistungen. - VI: Die Fortpflanzung. - VII: Die Entwicklung. - VIII: Die Beziehungen der Insekten zu ihrer Umwelt (Ökologie). - IX: Das System der Insekten.

ESPIR, M. L. E. and ROSE, F. C., **The Basic Neurology of Speech,** Blackwell Scientific Publications, Oxford, 1970, 134 pp., figs. and tables, £2.00.

There is a vast literature on the many disciplines with which speech is concerned, but this book is intended as a simple presentation of the neurological approach to its physiology and pathology. Although based on a course of lectures given by the authors in three of the schools of speech therapy in Great Britain, it should prove helpful not only to speech therapists, both for their final examinations and for practice afterwards, but to medical students and others in disciplines concerned with communication, e.g. paediatrics, psychiatry, psychology, phonetics, linguists and education. The neurological mechanisms of speech and its disorders are complex and because of the pragmatic simplification a selected bibliography is given.

ESSLER, W. K., **Induktive Logik** (Grundlagen und Voraussetzungen), Verlag Karl Alber, Freiburg, 1970, 378 pp., figs., No price stated.

CONTENTS. - *I: Die geschichtliche Entwicklung des Induktionsbegriffs. - II: Analyse des logischen Wahrscheinlichkeitsbegriffs. - III: Logische Grundlagen der Wahrscheinlichkeit. - IV: Das Kontinuum der induktiven Methoden. - V: Das System der Adäquatheitsbedingungen. - VI: Induktive Logik und Wahrscheinlichkeitslehre. - VII: Über die Voraussetzungen der Induktion.*

ESSLER, W. K., **Wissenschaftstheorie I** (Definition und Reduktion), Verlag Karl Alber, Freiburg, 1970, 162 pp., No price stated.

CONTENTS. - *I: Traditionelle und moderne Definitionslehre. - II: Definitionskriterien und Definitionsregeln. - III: Über Definierbarkeit und Undefinierbarkeit. - IV: Andere Formen von Definitionen. - V: Über partielle Definitionen.*

FILIPPI, F. (ed.) (E. PERUCCA, founder), **Dizionario di Ingegneria.** Vols. 1 and 2. 2nd ed., Unione Tipografico-Editrice Torinese, Torino, 1968/1969, **Vol. I: A-BA,** XX+974 pp., figs.; **Vol. II: BB-CH,** XVI+1026 pp., figs. and tables, L. 30.000 each volume.

Questo nuovo *Dizionario d'Ingegneria* di complessive 9.000 pagine con 20.000 voci e 15.000 illustrazioni, diretto dal Prof. Ing. Federico Filippi, Ordinario di Macchine e Motori nel Politecnico di Torino, rappresenta il secondo completo rifacimento dell'opera originaria in due volumi *Dizionario Tecnico Industriale,* diretto dai proff. Albenga e Perucca, pubblicato dalla Utet nel 1937. Ai settori tradizionali dell'ingegneria si affiancano non solo quelli delle nuove tecnologie, ma altresì l'aggiornamento della scienza in generale, verso cui la materia di quest'opera generosamente trabocca. Così ammodernato, questo classico « dizionario » si presenta come uno strumento di lavoro quanto mai prezioso per ogni persona che, più o meno direttamente, abbia che fare con « l'universo in espansione » della tecnica.

FINÓ, J.-F., **Forteresses de la France médiévale** (Construction, attaque, défense), 2nd rev. ed., Editions A. & J. Picard, Paris, 1970, X+512 pp., figs., 58 F.

L'architecture militaire ne fut pas une fin en soi mais un moyen de défense ou de domination. Son évolution dessine une courbe capricieuse parce que tout en suivant le progrès continu des techniques, elle s'est modelée avant tout sur le flux mouvant des événements de l'histoire. La longue histoire de l'architecture militaire pendant le Moyen Age est comme renouvelée par M. Finó, parce qu'il n'en sépare pas l'étude des techniques de l'armement et de son emploi, et, surtout, l'examen des effectifs nécessaires à la défense et à la prise des places.

FLORESCU, F. B., **Die Trajanssäule,** Akademie-Verlag, Bukarest, 1969, 296 pp., figs. and plates, Lei 62.

CONTENTS. - *I: Die Forschungsgeschichte der Säule. - II: Das Forum Traiani. - III: Die Bedeutung des Denkmals. - IV: Die Inschrift. - V: Die Gliederung des Denkmals. - VI: Die Säule auf den Münzen Trajans. - VII: Die Sachkultur. - VIII: Nachklänge der Dakischen Kriege. - IX: Nachahmungen.*

FLORKIN, M. and STOTZ, E. H. (eds.), **Comprehensive Biochemistry,** VOL. 21: **Metabolism of Vitamins and Trace Elements,** Elsevier Publishing Company, Amsterdam, 1971, XVI+298 pp., figs. and tables, Dfl. 65.00.

This volume is in two parts, the first dealing with the metabolism of water-soluble vitamins and the second with the metabolism and metabolic function of trace elements. Two concise treatments on the biosynthesis of thiamine and of pantothenic acid and coenzyme A are authored by Gene M. Brown. G. W. E. Plaut has provided a rather complete 33-page review on the biosynthesis of riboflavin, and E. E. Snell and Betty E. Haskell have jointly written the section on the metabolism of the various forms of vitamin B_6. The metabolic origins of the parts of the biotin molecule, with particular coverage of the most recent work, has been reviewed by D. B. McCormick and L. D. Wright, while T. Shiota has provided a section of 36 pages and 244 references entitled "The Biosynthesis of Folic Acid and 6-Substituted Pteridine Derivatives". L. Mervyn has discussed the absorption, tissue distribution, turnover, and excretion of vitamin B_{12} under the title "The Metabolism of the Cobalamins". Chapter II of the volume is entitled "Trace Elements: Metabolism and Metabolic Function", authored by B. L. O'Dell and B. J. Campbell. The chapter, in 75 pages and with 476 references, is a modern and comprehensive treatment of the known functions of iron, copper, zinc, cobalt, manganese, chromium, molybdenum, selenium, iodine, and fluoride.

FOUCHIER, J., BILLET, F. and EPSTEIN, H., **Chemical Dictionary,** 3rd rev. ed., Netherlands University Press, Amsterdam, 1970, 1500 pp., Dfl. 130.

Since the first edition that appeared in 1953, this dictionary is almost known now to every scientist working in the field of chemical and industrial engineering. Although the emphasis is on the industrial and engineering side of chemistry, this work has proved to be of great value to each chemist in general and also to many physicists. As the greater proportion of chemical publications is written in English, French or German, this dictionary has been kept trilingual. Another practical point is that the authors, in order to solve the difficulties involved in nomenclature, sometimes refer to commercial names (trade marks) or to a chemical formula which should permit the reader to recognize the product as such. This third edition contains now over 20.000 entries in each language which means that it exceeds the previous edition by 200 pages. Most of the new entries that have been added, fall into the category of the following fields: metallurgy, electronic engineering, nuclear physics, biochemistry and pharmaceutical chemistry, petrochemistry and plastic materials. Due to the usefulness of this dictionary in every-day-practice, it still remains incomparable with any other dictionary in this field.

GARIN, E., **Dal Rinascimento all'Illuminismo** (Studi e ricerche), (Series: *Saggi di Varia Umanità,* No. 11), Nistri-Lischi, Pisa, 1970, 294 pp., L. 2.500.

Il nesso Rinascimento-Illuminismo, già sostenuto e difeso dagli storici settecenteschi, ha trovato una conferma nella tesi di Delio Cantimori e di altri, di una possibile periodizzazione della storia della cultura moderna « da Petrarca a Rousseau ». L'intento delle pagine qui riunite è di richiamare l'attenzione sulla persistenza, pur nella varietà, di alcuni temi costanti dal XV al XVIII secolo; non offrire dei risultati, ma soltanto avanzare delle proposte di ricerca.

GIERLOFF-EMDEN, H. G., **Mexico** (Eine Landeskunde), Walter De Gruyter & Co., Berlin, 1970, XXIV+634 pp., figs. and tables, DM 136.

Obwohl es zahlreiche Publikationen über Mexico gibt – in der Bibliographie dieses Buches werden 1420 ausgewählte Titel nachgewiesen – stellt die Landeskunde von Mexico, die GIERLOFF-EMDEN vorlegt, innerhalb des geographischen Schrifttums des 20. Jhdts. die erste grosse Länderkunde dieses Subkontinents dar, die auf Grund eigener Studien und unter Auswertung des statistischen Materials und der Literatur erarbeitet worden ist. Das Buch enthält 148 Abbildungen, 93 Bilder, aus einem Bestand von 3000 Bildern ausgewählt, und zahlreiche Tabellen.

GIBERT, R. †, **Thermodynamique chimique,** Editions Eyrolles, Paris, 1970, 282 pp., figs. and tables, 35 F.

Cet ouvrage présente les bases de la thermodynamique chimique classique d'une manière logique, à l'usage des ingénieurs, des physiciens et des chimistes.

GINGUAY, M., **Dictionnaire d'informatique, anglais-français** (Series: *Les techniques de base de l'informatique*), Masson & C.ie, Paris, 1970, 140 pp., 36 F.

GRASSÉ, P.-P. (ed.), **Traité de Zoologie. Anatomie, Systématique, Biologie,** VOL. XIV: **Reptiles. Glandes endocrines, embryologie, systématique, paléontologie,** ISSUE III, Masson & Cie, Paris, 1970, 748 pp., figs. and plates, 240 F.

Le fascicule 3 du Tome XIV du Traité de Zoologie complète les chapitres consacrés à l'anatomie, l'embryologie, l'éthologie et la systématique des Reptiles. Les glandes endocrines, qui tiennent un rôle de premier ordre dans la physiologie et le comportement des Reptiles, ont été l'object d'exposés rédigés par d'éminents spécialistes. M. Gabe a donné une mise au point des connaissances actuelles sur le pancréas endocrine; Hubert Saint-Girons est l'auteur du chapitre qui traite de l'hypophyse; M. Gabe et H. Saint-Girons ont écrit celui qui concerne la thyroïde et les parathyroïdes; Michèle Martoja a rédigé les pages consacrées à la glande surrénale. Les chromosomes sexuels, les garnitures chromosomiques polyploïdes et la cytogénétique dans ses rapports avec la taxonomie et l'évolution sont les principales matières composant le chapitre du Professeur Robert Matthey. Les phénomènes de reproduction font l'objet d'un exposé exhaustif dont Jean Guibé est l'auteur. Jean Pasteels a rédigé les pages concernant le développement embryonnaire des Reptiles. Jean Guibé a donné le tableau systématique des ordres actuels. La classification qu'il adopte tient compte des plus récentes acquisitions faites dans le domaine de la taxonomie. Toutes les familles sont définies et les principaux genres mentionnés. Enfin, Léonard Ginsburg a retracé l'histoire et l'évolution des Reptiles fossiles.

HABING, H. J. (ed.), **Interstellar Gas Dynamics** (Proceedings of the Sixth Symposium on Cosmical Gas Dynamics Organized Jointly by the International Astronomical Union and the International Union of Theoretical and Applied Mechanics, Yalta, The Crimea, U.S.S.R., 8-18 September 1969), D. Reidel Publishing Company, Dordrecht, 1970, XXIV+368 pp., figs. and tables, Dfl. 75.

The main purpose of the conference was to bring together aerodynamicists and astrophysicists for discussions on the physics of interstellar gases. Consequently a large part of the text is taken by the discussions. The book is divided in two parts, which actually are linked in various ways: (I) Description and General Theory of the Interstellar Medium. (II) Interaction of Stars and Interstellar Medium. In Part I there are reviews on the present status of theory and observations of the interstellar gas, interstellar plasma physics, the interstellar magnetic field and gas flow around stars and galaxies (authors: H. C. van de Hulst, H. F. Weaver, G. B. Field, B. B. Kadomtsev, V. N. Tsytovich, G. L. Verschuur, E. N. Parker, E. A. Spiegel). In Part II reviews are found on overall mass exchange between stars and the interstellar medium, on the role of supernovae, mass loss from stars (solar wind, novae, planetary nebulae, OB stars), on grains, on molecules, and on protostars (authors: E. E. Salpeter, L. Woltjer, R. Lüst, S. R. Pottasch, A. A. Boyarchuk, J. M. Greenberg, T. P. Stecher, P. G. Mezger).

HALBWACHS, F., **Précis de Physique** (M.P.-P.C. - 2e année), 2 vols., (Series: « *Le Physicien* », No. 3-4), Presses Universitaires de France, Paris, 1969-1970, **Vol. I: Rappels d'optique. Électromagnétisme. Physique relativiste,** 336 pp., figs.; **Vol. II: Physique quantique,** 348 pp., figs., 20 F. each volume.

Contents. - **Volume I.** - *I: Ondes progressives et ondes stationnaires à une dimension. - II: Compléments d'optique classique. - III: Equations fondamentales de l'électromagnétisme dans le vide. - IV: Conséquences des équations de Maxwell dans le vide. - V: Electromagnétisme des milieux matériels. - VI: Notions de relativité restreinte. - VII: Optique et électromagnétisme relativiste.* - **Volume II.** - *VIII: Propriétés quantiques du rayonnement lumineux. - IX: Mécanique ondulatoire du corpuscule libre. - X: Equation de Schrödinger. - XI: Marche de potentiel et barrière de potentiel. - XII: Théorie élémentaire de la diffusion. - XIII: Les états liés du puits de potentiel. - XIV: L'oscillateur harmonique. - XV: Rotateur sphérique. Théorie du moment cinétique. - XVI: Atome d'hydrogène et classification des éléments.*

HANDLER, Ph. (ed.), **Biology and the Future of Man,** Oxford University Press, London, 1970, xxiv+936 pp., figs. and tables, £5.5.0.

This unique survey of the current status of all the life sciences was sponsored by the National Academy of Sciences, which serves as the official adviser on science and technology to the federal government. Beginning with the origin of life and ending with the role of biology in the future of human society, the study brings together in a single volume the reports of twenty-one scientific panels made up of authorities in particular disciplines. At the invitation of the Committee on Science and Public Policy of the Academy, these panels reviewed the present state of knowledge and the major questions facing researchers in their various fields. Starting at the molecular level, the book offers a logical intellectual structure for the life sciences, dealing in turn with subcellular organelles, cells, tissues and organs, organisms, species, and ecological systems. The book's scope is indicated by its chapter headings, which include: The Materials of Life and Their Transformation, The Biology of Development, Biological Structures, The Nervous System, The Biology of Behavior, Heredity and Evolution, The Diversity of Life, What Is Ecology?, Biology and Industrial Technology. The population explosion is treated in the final chapter. A separate chapter is concerned with the computer's application to the life sciences.

HELLWEGE, K. H., **Einführung in die Festkörperphysik II** (Series: *Heidelberger Taschenbücher,* Vol. 34), Springer-Verlag, Berlin, 1970, x+250 pp., figs., DM 12.80.

Contents. - I. Ionen in Kristallfeldern. *I: Qualitative Beschreibung eines Ions im Kristallgitter. - II: Termschema eines Ions im Kristallfeld. - III: Zeeman-Effekt von Ionen in Kristallen. - IV: Ionen in schwingenden Kristallen: Elektron-Phonon-Wechselwirkung. - V: Spektren von Ionen in Kristallfeldern.* - II. Magnetismus von Kristallen. *VI: Masssysteme. Grundlagen. - VII: Diamagnetismus von Isolatoren. - VIII: Paramagnetismus von Ionenkristallen. - IX: Kopplung magnetischer Momente untereinander und mit den Gitterschwingungen. - X: Paramagnetische Relaxation. - XI: Ferromagnetismus. - XII: Antiferromagnetismus. - XIII: Kompliziertere magnetische Strukturen. Ferrimagnetismus. - XIV: Ferrimagnetische und antiferromagnetische Spinwellen.*

HERBIG, G. H. (ed.), **Spectroscopic Astrophysics** (An Assessment of the Contributions of Otto Struve), University of California Press, Berkeley, 1970, x+462 pp., figs., tables and plates, No price stated.

Otto Struve's scientific insight, intellectual powers, and organizational skill pervade almost every domain of observational astrophysics as we know it today. This volume is a memorial to a great scientist: ten areas in which he either pioneered or made contributions of lasting significance are introduced directly from the pages of the original journals. Then, each area is taken up by a leading specialist who discusses the field as Struve left it, where it stands today, and where it may go next. These commentaries have been written by Struve's former associates or pupils from the great years when he served successfully as Director of three major observatories (Yerkes, McDonald, National Radio) and head of two leading departments of astronomy (Chicago and California at Berkeley).

HERMES, H., **Term Logic with Choice Operator** (Series: *Lecture Notes in Mathematics*, Vol. 6), Springer-Verlag, Berlin, 1970, IV+56 pp., DM 6.

CONTENTS. - *I: Introduction. Foreword the Revised Edition. - II: First Order Predicate Logic with Choice Operator. - III: First Order Term Logic with Choice Operator. - IV: Embedding of Predicate Logic in Term Logic. - V: Embedding of Term Logic in Predicate Logic. - VI: Free Occurrence of a Variable. Rank of a Term. Elementary Substitution. - VII: A Term Calculus. - VIII: Soundness of the Rules. - IX: Survey of the Completeness Proof. - X: Some Theorems concerning Deducibility. - XI: Substitution. - XII: Further Theorems concerning Deducibility. - XIII: Survey of the Proof of the Theorem on Satisfiability. - XIV: Details of the Proof. - XV: Completeness of Restricted Term Calculus.*

HILGARD, E. R. and BOWER, G. H., **Le teorie dell'apprendimento,** Franco Angeli Editore, Milano, 1970, 850 pp., figs. and tables, L. 14.000.

CONTENTS. - *I: Natura delle teorie dell'apprendimento. - II: Il connessionismo di Thorndike. - III: Il condizionamento classico di Pavlov. - IV: Il condizionamento contiguo di Guthrie. - V: Il condizionamento operante di Skinner. - VI: La teoria sistematica del comportamento di Hull. - VII: L'apprendimento per segni di Tolman. - VIII: La teoria della Gestalt. - IX: La psicodinamica di Freud. - X: Funzionalismo. - XI: Teoria matematica dell'apprendimento. - XII: Teorie comportamentali basate su schemi per l'elaborazione di informazione. - XIII: Neurofisiologia dell'apprendimento. - XIV: Sviluppi recenti: I. Le condizioni fondamentali dell'apprendimento e della ritenzione. - XV: Sviluppi recenti: II. Apprendimento della discriminazione e attenzione. - XVI: Apprendimento e tecnologia dell'istruzione.*

HOFSTÄTTER, P. R., **Dinamica di gruppo** (Critica della psicologia di massa), Franco Angeli Editore, Milano, 1970, 202 pp., figs. and tables, L. 4.000.

CONTENTS. - *I: Massificazione ed isolamento. - II: L'invenzione del gruppo. - III: Il gruppo in laboratorio. - IV: L'affermazione della propria libertà e il rispetto della libertà altrui.*

HÖHLER, G. (ed.), **Springer Tracts in Modern Physics** (Ergebnisse der exakten Naturwissenschaften), **Volume 52: Weak Interactions** (Invited Papers presented at the Second International Summer School for Theoretical Physics, University of Karlsruhe, July 14 - August 1, 1969), Springer-Verlag, Berlin, 1970, VI+214 pp., figs., DM 58.

CONTENTS. - *I: A Survey of the Weak Interactions*, by S. GASIOROWICZ. - *II: Semileptonic Decays*, by V. F. MULLER. - *III: Non Leptonic Decays*, by B. STECH. - *IV: Current Algebra and Weak Interactions*, by B. RENNER. - *V: The Decays of the K_0-K_0 System*, by H. G. DOSCH. - *VI: Questions Raised by CP-Nonconservation*, by P. K. KABIR. - *VII: Relations for Semileptonic Weak Interactions Involving Photons*, by W. KUMMER. - *VIII: Radiative Corrections to Weak Decays Involving Leptons*, by RIAZUDDIN. - *IX: Radiative Corrections to Weak Interactions*, by J. ROTHLEITNER. - *X: Unconventional Models of Weak Interactions*, by G. SEGRÈ. - *XI: Weak Interactions at Small Distances*, by H. PIETSCHMANN. - *XII: Physical Symmetries in the Framework of Quantum Field Theory*, by J. T. LOPUSZAŃSKI.

HÖHLER, G. (ed.), **Springer Tracts in Modern Physics** (Ergebnisse der exakten Naturwissenschaften), **Vols. 53 and 54,** Springer-Verlag, Berlin, 1970, IV+106 and VI+176 pp., figs. and tables, DM 38 and DM 56.

CONTENTS. - **Volume 53.** - *I: On the S-Matrix Theory of Weak Interactions*, by A. O. BARUT. - *II: Weak Interactions in Nuclear Physics*, by H. PRIMAKOFF. - *III: Weak Interactions at High Energies*, by G. v. GEHLEN. - *IV: Cabibbo Angle and SU_2 X SU_2 Breaking*, by R. GATTO. - **Volume 54.** - *I: Magnetic Impurities in Metals: The s-d Exchange Model*, by K. FISCHER. - *II: Optical Constants of Solids by Electron Spectroscopy*, by J. DANIELS, C. v. FESTENBERG, H. RAETHER and K. ZEPPENFELD. - *III: Depolarisierte Rayleigh-Streuung und Strömungs-Doppelbrechung in Gasen*, by S. HESS.

I numeri interi (Progetto Nuffield per la matematica, No. 4), Zanichelli Editore, Bologna, 1970, 68 pp., figs. and tables, L. 1.000.

CONTENTS. - *I: Estensione della numerazione posizionale.* - *II: Aritmetica modulare.* - *III: Gli interi.* - *IV: Applicazione degli interi.* - *V: Grandi numeri ed esponenti.*

JACOB, F., **La logique du vivant** (Une histoire de l'hérédité), (Series: *Bibliothèque des sciences humaines*), Editions Gallimard, Paris, 1970, 354 pp., No price stated.

Il n'y a pas une organisation du vivant, mais une série d'organisations emboîtées les unes dans les autres comme des poupées russes. Derrière chacune s'en cache une autre. Au-delà de chaque structure accessible à l'analyse finit par se révéler une nouvelle structure, d'ordre supérieur, qui intègre la première et lui confère ses propriétés. On n'accède à celle-ci qu'en bouleversant celle-là, en décomposant l'espace de l'organisme pour le recomposer selon d'autres lois. A chaque niveau d'organisation ainsi mis en évidence répond une manière nouvelle d'envisager la formation des êtres vivants. Ce qu'on a cherché à décrire dans ce livre, ce sont les conditions qui, depuis le XVIe siècle, ont permis l'apparition successive de ces structures. C'est la manière dont la génération, création chaque fois renouvelée exigeant toujours l'intervention de quelque force externe, s'est transformée en reproduction, propriété interne de tout système vivant. C'est l'accés à ces objets de plus en plus cachés que constituent les cellules, les gènes, les molécules d'acide nucléique. La découverte de chaque poupée russe, la mise en évidence de ces dénivellations successives ne résultent pas simplement d'une accumulation d'observations et d'expériences. Le plus souvent, elles expriment un changement plus profond, une transformation dans la nature même du savoir. Elles ne font que traduire, dans l'étude du monde vivant, une manière nouvelle de considérer les objets.

JACOBS, P. A., PRICE, W. H. and LAW, P. (eds.), **Human Populations Cytogenetics** (*Pfizer Medical Monographs*, No. 5), University Press, Edinburgh, 1970, 326 pp., figs. and tables, £3.75.

Human Population Cytogenetics is an increasingly significant area of bio-medicine and related life-sciences. Much of the recent impetus given to its advance originated with the late Professor Court Brown in Edinburgh. This international seminar was initiated by Dr Court Brown, and this volume is a memorial to the internation stimuli he created. The first series of papers deals with meiosis in man, and the pattern of segregation of different types of translocations. These contributions are followed by a number of papers dealing with the frequency of sex chromosome and autosome aberrations in spontaneously aborted foetal material, and neonatal and adult populations. Data are provided on the influence of physical, chemical and biological agents on the production of chromosome aberrations. Two contributions place the recent advances in human population cytogenetics in a broader context, one a comparative study dealing with population cytogenetics of other animal species and one considering the operation of selection on human chromosome abnormalities. A final series of papers deals with the most recent advances in automated procedures for the recognition and karyotyping of human metaphase chromosomes.

Journal de l'année (1er Juillet 1969-30 Juin 1970), Librairie Larousse, Paris, 1970, 416 pp., figs. and tables, 58 F.

L'un des mérites du *Journal de l'année* (1er juillet 1969 - 30 juin 1970), publié sous la direction de Maurice Barrois par la Librairie Larousse, est d'avoir « mis en mémoire » pour nous tout ce que nous avons oublié - ou qui nous a échappé - sur 365 jours de la vie du monde: la première année « Pompidou », les conflits sociaux, les grands problèmes internationaux de l'heure (Viêt-Nam, Proche-Orient, etc.), mais aussi l'expansion économique de la Nouvelle-Calédonie, l'affaire des vedettes de Cherbourg, l'accord sur la mensualisation, le succès des conservateurs en Grande-Bretagne, les organisations de fedayin, le bilan des tremblements de terre du Pérou, le point sur la pilule, la protection de la nature, l'agitation universitaire, etc.

KINNE, O. (ed.), **Marine Ecology** (A Comprehensive, Integrated Treatise on Life in Oceans and Coastal Waters), VOLUME I: **Environmental Factors,** PART 1, John Wiley & Sons, Ltd., London, 1970, XVI+682 pp., figs. and tables, £12.50.

Neither a compendium nor an encyclopaedia, the treatise is intended as an exhaustive systematic exposition summarizing and evaluating information obtained thus far on living systems in the sea and littoral areas. It has been conceived with the growing number of individuals in mind who are professionally concerned with life in the marine environment.

CONTENTS. - I. Oceans and Coastal Waters as Life-supporting Environments, by A. W. COLLIER. - II. Light. *0: General Introduction*, by N. G. JERLOW. - *I: Bacteria, Fungi and Blue-green Algae*, by W. GUNKEL. - *II: Plants*, by J. A. HELLEBUST. - *III: Animals: Invertebrates*, by E. SEGAL; *Fishes*, by J. H. S. BLAXTER. - III. Temperature. *0: General Introduction*, by O. KINNE. - *I: Bacteria, Fungi and Blue-green Algae*, by C. H. OPPENHEIMER. - *II: Plants*, by F. GESSNER. - *III: Animals: Invertebrates*, by O. KINNE; *Fishes: Functional Responses*, by J. R. BRETT; *Structural Responses*, by E. T. GARSIDE.

KLIR, J. and VALACH, M., **Cybernetic Modelling,** Iliffe Book Ltd., London, 1967, 438 pp., figs., £3.50.

The book begins at a fundamental level with a discussion of systems. The cybernetic approach is introduced, and this leads to a study of the cybernetic systems which have been described by Ashby. The second section introduces some problems in biology which seem amenable to the methods of cybernetics. Homeostatic systems (which maintain parameters constant despite environmental disturbances) and information processing systems (especially the central nervous system) are given special treatment. The last section discusses a range of studies in cybernetic modelling with excursion into the realms of psychology, linguistics, biology and philosophy.

KLOPFER, P. H., **Ökologie und Verhalten** (Psychologische und ethologische Aspekte der Ökologie), Gustav Fischer Verlag, Stuttgart, 1968, XII+98 pp., figs. and tables, DM 18.

CONTENTS. - *I: Warum rotten Raubtiere ihre Beute nicht aus? - II: Wie werden Nahrung und Lebensraum zwischen Arten aufgeteilt? - III: Warum variiert die Mannigfaltigkeit der Arten? - IV: Wie wird die Mischung von Arten verhindert? - V: Wie sind Tiergemeinschaften organisiert?*

KORMONDY, E. J., **Concepts of Ecology,** Prentice-Hall, Inc., Englewood Cliff, 1969, XIV+210 pp., figs. and tables, £2.00.

The significant concepts of modern ecology are emphasized in this book, with specific mention of the areas that are still unknown or yet to be fully understood. The author's unifying theme in *Concepts of Ecology* is the structure and function of ecosystems, with reference to energy flow, nutrient cycling, population growth and regulation, and community organization and dynamics. The descriptions are presented in an interesting manner, stressing theory and observation rather than the mathematical.

CONTENTS. - *I: The Nature of Ecosystems. - II: Energy Flow in Ecosystems. - III: Biogeochemical Cycles and Ecosystems. - IV: Ecology of Populations. - V: The Organization and Dynamics of Ecological Communities. - VI: Ecology and Man.*

KREBS, H., **Grundzüge der anorganischen Kristallchemie,** Ferdinand Enke Verlag, Stuttgart, 1968, XII+376 pp., figs. and tables, DM 45.

Das vorliegende Werk bringt eine auch für einen grösseren Leserkreis verständliche Einführung in die grundlegenden Theorien über den Aufbau der kristallinen Materie, die ihrerseits wieder bei der Deutung der Kristallstrukturen herangezogen werden. Sind die Bindungselektronenpaare vorwiegend lokalisiert, so ist das Koordinationsverhalten der Atome im Gitter ähnlich dem in Molekülen, Molekülionen oder komplexen Ionen. Darüber hinaus wird — basierend auf Forschungsergebnissen des Verfassers — gezeigt, wie die Symmetrie der Atomorbitale, ihre unterschiedliche Ausdehnung und ihre Besetzung mit Elektronen, weitgehend Struktur und Eigenschaften der Kristalle bedingen. Besondere mesomere Bindungen, die nur in Gittern möglich sind, bestimmen häufig die Kristallstruktur und verursachen wie bei organischen konjugierten Systemen tiefe Farbe und elektrisches Leitvermögen. Die theoretischen Zusammenhänge werden ausführlich an wenigen einfachen Gittertypen wie dem des NaCl und des CaF_2 entwickelt. Dafür kam dann bei der Beschreibung der grossen Vielzahl der Kristallgitter die Theorie der Bindung kürzer gefasst werden. Besonderer Wert wurde gelegt auf die eingehende Erörterung von Gittertypen, die z.Z. im Vordergrund des wissenschaftlichen und technischen Interesses stehen.

KOYRÉ, A., **Dal mondo chiuso all'universo infinito** (Series: *Storia della scienza*), Feltrinelli Editore, Milano, 1970, 216 pp., figs., L. 2.500.

CONTENTS. - *I: Il cielo e i cieli. N. da Cusa e M. Palingenio. - II: La nuova astronomia e la nuova metafisica. N. Copernico, Th. Digges, G. Bruno, W. Gilbert. - III: La nuova astronomia contro la nuova metafisica. Il rifiuto dell'infinità da parte di Keplero. - IV: Cose mai viste e pensieri mai prima concepiti: la scoperta di nuove stelle nello spazio e la materializzazione dello spazio. Galileo e Descartes. - V: Estensione indefinita e spazio infinito. Descartes e H. More. - VI: Dio e spazio, spirito e materia. H. More. - VII: Spazio assoluto, tempo assoluto e loro relazioni con Dio. Malebranche, Newton e Bentley. - VIII: La divinizzazione dello spazio. J. Raphson. - IX: Dio e il mondo: spazio, materia, etere e spirito. I. Newton. - X: Spazio assoluto e tempo assoluto: il contesto dell'azione di Dio. Berkeley e Newton. - XI: Il Dio dei giorni feriali e il Dio del Sabbath. Newton e Leibniz. - XII: L'Artefice divino e il Dieu fainéant.*

KRISHNA, K. and WEESNER, F. M. (eds.), **Biology of Termites**, VOL. I and II, Academic Press Inc., New York, 1969/1970, XIV+598 and XIV+644 pp., figs. and tables, $32.00 each volume.

This modern treatise deals with the fundamental aspects of termite biology. Areas considered include anatomy, ecology, physiology, behavior, caste determination, colony development, defense, foraging, and swarming. Also discussed are taxonomy, origins, fossil records and distribution, and associations with other organisms such as termitophiles, fungi, bacteria, and protozoa. A large number of field and laboratory observations are cited, with discussions on the maintenance of experimental groups, biochemical studies, and radioisotope techniques. In addition, the role of man in termite dispersion is assessed, and the ecology and general biology of the termite faunas of the various zoogeographical regions are presented.

Landolt-Börnstein (Zahlenwerte und Funktionen aus Naturwissenschaften und Technik), New Series, III: **Kristall- und Festkörperphysik**, VOL. 4, PART a, Springer-Verlag, Berlin, 1970, XVI+368 pp., figs., DM 218.

CONTENTS. - *I: Fe Oxides and Fe-Me-O Compounds*, by R. A. LEFEVER. - *II: Compounds with Lanthanide and Actinide Elements of Some Special Structure Types - Lanthaniden- und Aktiniden-Verbindungen einiger spezieller Strukturtypen*, by F. HOLTZBERG, T. R. MCGUIRE and S. METHFESSEL. - *III: Crystallographic and Magnetic Properties of Perovskite and Perovskite-Related Compounds*, by J. B. GOODENOUGH and J. M. LONGO. - *IV: Yttrium and Rare Earth Iron Garnets*, by D. L. HUBER.

LAUDET, M., LACOMBE, D., NOLIN, L. and SCHUTZENBERGER, M. (eds.), **Symposium on Automatic Demonstration** (Held at Versailles/France, December 1968), (Series: *Lecture Notes in Mathematics*, Vol. 125), Springer-Verlag, Berlin, 1970, VI+310 pp., DM 20.

CONTENTS. - *I:* M. LAUDET, *Allocution d'ouverture*. - *II:* A. ARNOLD, *Présentation d'un langage de formalisation des démonstrations mathématiques naturelles*. - *III:* N. G. DE BRUIJN, *The Mathematical Language* AUTOMATH, *its Usage, and some of its Extensions*. - *IV:* E. ENGELER, *Proof Theory and the Accuracy of Computations*. - *V:* R. FRAISSE, *Aspects du Théorème de complétude selon Herbrand*. - *VI:* A. GRZEGORCZYK, *Decision Procedure for Theories Categorical in Alef*. - *VII:* HAO WANG, *On the Long-range Prospects of Automatic Theorem Proving*. - *VIII:* R. KOWALSKI, *The Case for using Equality Axioms in Automatic Demonstration*. - *IX:* G. KREISEL, *Hilbert's Programme and the Search for Automatic Proof Procedures*. - *X:* D. W. LOVELAND, *A Linear Format for Resolution*. - *XI:* D. LUCKHAM, *Refinement Theorems in Resolution Theory*. - *XII:* Z. PAWLAK, *Definitional Approach to Automatic Demonstration*. - *XIII:* J. PITRAT, *Heuristic Interest of using Metatheorems*. - *XIV:* D. PRAWITZ, *A Proof Procedure with Matrix Reduction*. - *XV:* G. ROBINSON and L. WOS, *Axiom Systems in Automatic Theorem Proving*. - *XVI:* D. SCOTT, *Constructive Validity*. - *XVII:* L. WOS and G. ROBINSON, *Paramodulation and Set of Support*.

LOOMIS, W. F., Jr. (ed.), **Papers on Regulation of Gene Activity during Development,** Harper & Row, Publishers, New York, 1970, XII+442 pp., figs. and tables, DM 28.50.

CONTENTS. - Section I: *Endogenous Control of Enzyme Synthesis in Eukaryotic Microorganisms*. - Section II: *Differential Synthesis of Proteins in Developing Organs*. - Section III: *Chromosomal Differentiation*. - Section IV: *Control of Enzyme Content in Liver Cells*. - Section V: *Hemopoesis*.

LYNCH, O. M., **The Politics of Untouchability** (Social Mobility and Social Change in a City of India), Columbia University Press, London, 1969, XX+252 pp., figs. and tables, $10.00.

This study deals with the effects of independence and the adoption of parliamentary democracy as a system of government on an urban group in India—the Jatavs, a community of leather workers who live in the city of Agra. Professor Lynch analyzes their efforts to rise in terms of the political, economic, and religious aspects of their social life.

MABRY, T. J., MARKHAM, K. R. and THOMAS, M. B., **The Systematic Identification of Flavonoids,** Springer-Verlag, Berlin, 1970, XII+354 pp., figs. and tables, DM 98.

CONTENTS. - I. The Isolation, Purification and Preliminary Identification of Flavonoids. *I: The Two-Dimensional Paper Chromatographic Analysis of Flavonoids*. - *II: The Separation of Flavonoids by Column and Thin Layer Chromatography*. - *III: The Aglycone and Sugar Analysis of Flavonoid Glycosides*. - II. The Structure Analysis of Flavonoids by Ultraviolet Spectroscopy. *IV: Reagents and Procedures for the Ultraviolet Spectral Analysis of Flavonoids*. - *V: The Ultraviolet Spectra of Flavones and Flavonols*. - *VI: The Ultraviolet Spectra of Isoflavones, Flavanones, and Dihydroflavonols*. - *VII: The Ultraviolet Spectra of Chalcones and Aurones*. - III. The Structure Analysis of Flavonoids by Proton Nuclear Magnetic Resonance Spectroscopy. *VIII: The Determination and Interpretation of NMR Spectra of Flavonoids*. - *IX: The NMR Spectra of Flavonoids*.

MARMOR, J. (ed.), **L'inversione sessuale** (I diversi aspetti dell'omosessualità), Feltrinelli Editore, Milano, 1970, 350 pp., tables, L. 4.000.

Questo volume rappresenta un tentativo di chiarire uno dei problemi più stimolanti della psichiatria, quello dell'omosessualità. Il criterio basilare seguito nel raccogliere i presenti saggi è stato suggerito dalla costatazione che in questo settore, non meno che in quasi tutti gli altri settori della psicopatologia, il rapporto di causa ed effetto non può essere ricercato in un fattore singolo ma è il risultato di una molteplicità di fattori. Si è cercato quindi di mettere insieme le informazioni fornite relativamente al problema da ogni branca delle scienze biologiche e sociali in grado di chiarire i complessi aspetti del problema stesso: storia, zoologia comparata, genetica, endocrinologia, sociologia, antropologia, diritto, psicologia, psichiatria e psicoanalisi.

MAXWELL, J. C., **A Treatise on Electricity and Magnetism,** Unabridged 3rd ed., 2 Vols., Dover Publications, Inc., New York, xxxii+506 and xxiv+500 pp., figs. and plates, $3.00 each volume.

Contents. - **Volume I.** - Preliminary: *On the Measurement of Quantities.* - I. Electrostatics. *I: Description of Phenomena. - II: Elementary Mathematical Theory of Electricity. - III: On Electrical Work and Energy in a System of Conductors. - IV: General Theorems. - V: Mechanical Action between Two Electrical Systems. - VI: Points and Lines of Equilibrium. - VII: Forms of Equipotential Surfaces and Lines of Flow. - VIII: Simple Cases of Electrification. - IX: Spherical Harmonics. - X: Confocal Surfaces of the Second Degree. - XI: Theory of Electric Images. - XII: Conjugate Functions in Two Dimensions. - XIII: Electrostatic Instruments.* - II. Electrokinematics. *I: The Electric Current. - II: Conduction and Resistance. - III: Electromotive Force between Bodies in Contact. - IV: Electrolysis. - V: Electrolytic Polarization. - VI: Mathematical Theory of the Distribution of Electric Currents. - VII: Conduction in Three Dimensions. - VIII: Resistance and Conductivity in Three Dimensions. - IX: Conduction through Heterogeneous Media. - X: Conduction in Dielectrics. - XI: Measurement of the Electric Resistance of Conductors. - XII: Electric Resistance of Substances.* - **Volume II.** - III. Magnetism. *I: Elementary Theory of Magnetism. - II: Magnetic Force and Magnetic Inductions. - III: Magnetic Solenoids and Shells. - IV: Induced Magnetization. - V: Particular Problems in Magnetic Induction. - VI: Weber's Theory of Induced Magnetism. - VII: Magnetic Measurements. - VIII: On Terrestrial Magnetism.* - IV. Electromagnetism. *I: Electromagnetic Force. - II: Ampère's Investigation of the Mutual Action of Electric Currents. - III: On the Induction of Electric Currents. - IV: On the Induction of a Current on Itself. - V: On the Equations of Motion of a Connected System. - VI: Dynamical Theory of Electromagnetism. - VII: Theory of Electric Circuits. - VIII: Exploration of the Field by Means of the Secondary Circuit. - IX: General Equations of the Electromagnetic Field. - X: Dimensions of Electric Units. - XI: On Energy and Stress in the Electromagnetic Field. - XII: Current-Sheets. - XIII: Parallel Currents. - XIV: Circular Currents. - XV: Electromagnetic Instruments. - XVI: Electromagnetic Observations. - XVII: Comparison of Coils. - XVIII: Electromagnetic Unit of Resistance. - XIX: Comparison of the Electrostatic with the Electromagnetic Units. - XX: Electromagnetic Theory of Light. - XXI: Magnetic Action on Light. - XXII: Ferromagnetism and Diamagnetism Explained by Molecular Currents. - XXIII: Theories of Action at a Distance.*

MILLOT, G., **Geology of Clays** (Weathering, Sedimentology, Geochemistry), Springer-Verlag, Berlin, 1970, xvi+430 pp., figs., tables and plates, DM 61.

Contents. - *I: The Clay Minerals. - II: Argillaceous Rocks. - III: Geochemistry of Ions in the Hydrosphere. - IV: The Place of Clays in the Geochemical Cycle. - V: Weathering and Soil Clays. - VI: Clays of Continental Sediments. - VII: Clays of Marine Sediments. - VIII: Evolution of the Clay Fraction in Some Great Sedimentary Series. - IX: Silicifications, Flint and Growth of Crystals. - X: Genesis of Clay Minerals: Inheritance and Transformation. - XI: Genesis of Clays: Neoformation and Syntheses. - XII: Superficial Geochemistry and the Silicate Cycle.*

MINELLE, J., **Les fondements de la vie** (Biologies), Librairie Maloine S.A., Paris, 1970, 280 pp., 43 F.

L'auteur aborde l'étude des Fondements de la Vie, remontant à la formation de notre planète qui fut une masse incandescente évoluant dans l'espace, probablement détachée du Soleil et possédant les constituants essentiels dont l'hydrogène représente la vedette. Un vaste panorama passe en revue les périodes de l'histoire terrestre jusqu'à l'époque où la matière s'est stabilisée, atteignant l'apogée de sa perfection physique et vivante.

MLYNARSKI, M., **Fossile Schildkrotën** (Die neue Brehm-Bücherei, No. 396), A. Ziemsen Verlag, Wittenberg Lutherstadt, 1969, 128 pp., figs., DM 10.70.

Contents. - I. Einführung. - II. Einleitender Teil. *I: Biologie. - II: Morphologie. - III: Taxonomie des fossilen Schildkröten.* - III. Allgemeiner Teil. *I: Vorfahren der Schild-*

kröten. - *II: Hypothetischer Entwicklungsweg.* - *III: Phylogenetische Grundlagen der Systematik.* - IV. Systematischer Teil. - V. Literaturverzeichnis. - VI. Index der systematischen Namen.

MORANDO, B. (ed.), **Dynamics of Satellites (1969),** (Proceedings of a Symposium held in Prague, May 20-24, 1969), Springer-Verlag, Berlin, 1970, VIII+312 pp., figs. and tables, DM. 84.

The present volume contains 38 communications presented at an international symposium in Prague, Czechoslovakia, from the 20th to the 24th of May 1969. This symposium is a continuation of a similar symposium organized in Paris in 1965. An important part of the symposium has been devoted to Geodesy as the study of the motion of artificial satellites has greatly improved the determination of the shape and of the potential of the Earth.

MORIAU, M., **Inhibiteurs naturels de la fibrinolyse et phénomènes thrombohémorragiques,** Editions Arscia S.A., Bruxelles, 1970, 286 pp., figs. and tables, 560 F.B.

CONTENTS. - *I: Généralités sur le processus normal de fibrinolyse.* - *II: Les antiplasmines physiologiques: état de la question.* - *III: Techniques.* - *IV: Essais d'isolement et étude des caractères physicochimiques et immunologiques des antiplasmines plasmatiques et plaquettaire.* - *V: Variations physiologiques et pathologiques des activités antiplasminiques plasmatique et plaquettaire.* - *VI: Etude des inhibiteurs naturels de la fibrinolyse dans les processus expérimentaux de coagulation intravasculaire, de fibrinolyse et de thrombophilie.* - *VII: Etude des inhibiteurs naturels de la fibrinolyse au cours des processus cliniques de coagulation intravasculaire disséminée, de fibrinolyse et de thrombophilie.* - *VIII: Rôle des inhibiteurs naturels de la fibrinolyse dans la pathogénie, le diagnostic et le traitement des phénomènes thrombohémorragiques rencontrées en pathologie.*

MOROSON, H. L. and QUINTILIANI, M. (eds.), **Radiation, Protection and Sensitization** (Proceeding of the Second International Symposium on Radiosensitizing and Radioprotective Drugs, Istituto Superiore di Sanità, Rome, 6-8 May 1969), Taylor & Francis Ltd., London, 1970, XVI+524 pp., figs. and tables, £8.00.

This volume contains the proceedings of the Second International Symposium on Radiosensitizing and Radioprotective Drugs held at the Istituto Superiore di Sanità, Rome, Italy, May 1969. The purpose of the meeting, which was attended by scientists from twenty countries, was to provide an opportunity for the exchange of information and discussion on recent advances in the field of radiation protection and sensitization. Five review lectures and sixty eight invited and contributed papers deal with the different aspects of the subject, from the purely physico-chemical level to clinical application. The Symposium was held under the sponsorship of the European Society for Biochemical Pharmacology and Comitato Nazionale per l'Energia Nucleare.

MÜLLER, A. H., **Lehrbuch der Paläozoologie,** VOL. III: **Vertebraten,** ISSUE 3: **Mammalia,** Veb Gustav Fischer Verlag, Jena, 1970, XVI+856 pp., figs., DM 95.70.

O. Stamm Vertebrata (Wirbeltiere), Fortsetzung zu Band III, Teil 2. Klasse Mammalia: Docodonta - Monotremata - Multituberculata - Triconodonta - Symmetrodonta - Eupantotheria - Marsupialia - Carnivora - Cetacea - Artiodactyla - Condylarthra - Litopterna - Notoungulata - Astrapotheria - Tubulidentata - Pantodonta - Dinocerata - Pyrotheria - Xenungulata - Proboscidea - Barytherioidea - Embrithopoda - Hyracoidea - Sirenia - Desmostylia - Perissodactyla - Tillodontia - Taeniodonta - Edentata - Pholidota - Lagomorpha - Simplicidentata - Insectivora - Dermaptera - Chiroptera - Primates.

NAUTA, W. J. H. and EBBESSON, S. O. E. (eds.), **Contemporary Research Methods in Neuroanatomy,** Springer-Verlag, Berlin, 1970, VIII+386 pp., figs. and tables, DM 98.

This work is the result of a conference held in San Juan, Puerto Rico in January 1969, in which two dozen leading scientists from the United States and Europe participated. It contains descriptions of the research methods used in neuroanatomical studies. Each chapter contains a historical sketch, a detailed description of the method, abilities and limitations of the technique, sources of misinterpretations, and a bibliography. A verbatim record of the discussion following each presentation is also included.

NICOLINI, P., **Gîtologie des concentrations minérales stratiformes,** Gauthier-Villars, Paris, 1970, XLII+792 pp., figs. and tables, No price stated.

CONTENTS. - I. Approches sédimentologiques, paléogéographiques et géométriques en gîtologie. *I: Les faciès. Notion de faciès. Méthodes d'étude. - II: Aspect dynamique des dépots. Origine et transport. - III: Eléments de paléogéographie du milieu gîtologique. - IV: Paléotopographie et géomorphologie. - V: Paléoclimatologie. - VI: Aspect géochimique du milieu. - VII: Aspects biologiques du milieu. - VIII: Synthèses et conclusions paléogéographiques. - IX: Géométrie des milieux gîtologiques et des concentrations minérales.* - II. Evolution du milieu gîtologique. *I: Evolution d'un faciès, transformations. - II: Evolution d'une série sédimentaire: successions.* - III. Les événements extérieurs au milieu sédimentaire et influences sur ce milieu. *I: Aspect tectonique. - II: La notion socle-couverture et association de types de gisements. - III: Le magmatisme.* - IV. Les données conduisant à une interprétation des faits. *I: Notions de minéralogie des concentrations stratiformes. - II: Répartition des concentrations minérales dans l'espace. Provinces et « monstres » gîtologiques. - III: Répartition des concentrations minérales dans le temps. Epoques gîtologiques. - IV: Le milieu actuel. Vers une gîtologie océanique. - V: Problèmes de genèse. - VI: Classement des gîtes stratiformes.* - V. Utilisation des données gîtologiques dans l'exploration minérale. *I: Les métallotectes. - II: Diagrammes prévisionnels. - III: Cartes minérales et cartes prévisionnelles. - IV: Enquête sur l'organisation de l'exploration minérale.* - Conclusions générales.

NORBECK, E., **Religion and Society in Modern Japan: Continuity and Change,** Tourmaline Press, Houston, 1970, VIII+232 pp., No price stated.

CONTENTS. - *I: Introduction. - II: The New Religious Sects. - III: The Old Religions: Shinto, Buddhism, and Christianity. - IV: Japanese Society: Traditional Modes and New Departures. - V: The Roles of Religion. - VI: Manners, Morals, and Social Sanctions. - VII: Religion and Achievement. - VIII: Prospects.*

O'DONNELL, J. H. and SANGSTER, D. F., **Principles of Radiation Chemistry,** Edward Arnold (Publishers) Ltd., London, 1970, VIII+176 pp., figs. and tables, £2.25.

This book was written because there was a need for a brief and simple introduction to radiation chemistry. It is intended for readers with some knowledge of chemistry who wish to make their first acquaintance with radiation chemistry. It will serve as a basis for a short introductory course. Though not intended as a laboratory manual, the authors have incorporated useful practical information not readily available elsewhere.

OLSSON, I. U. (ed.), **Radiocarbon Variations and Absolute Chronology** (Proceedings of the Twelfth Nobel Symposium held at the Institute of Physics at Uppsala University), (Nobel Symposium 12), Almqvist & Wiksell, Stockholm, 1970, 658 pp., figs., tables and plates, No price stated.

The Twelfth Nobel Symposium was mainly devoted to discussions of the magnitude and the causes of the secular variations of the C14/C12 ratio in the atmosphere.

CONTENTS. - *I: C14 and Archaelogy. - II: Pottery Analyses. - III: C14 and Varve Chronology. - IV: C14 and Dendrochronology. - V: Ice-Core Analyses. - VI: Exchange Rates and Radiocarbon in Different Reservoirs. - VII: Radiogenic Isotopes in the Atmosphere and in Meteorites. - VIII: Causes of Secular C14/C12 Variations. - IX: Dates of C14/C12 Variations with Respect to Dendrochronology. - X: Open Session. - XI: Closing Session.*

PARENZAN, P., **Carta d'identità delle conchiglie del Mediterraneo,** VOL. I: **Gasteropodi,** Bios Taras Editrice, Taranto, 1970, 284 pp., figs. and tables, L. 5.500.

Il titolo del presente lavoro indica le sue finalità: il riconoscimento delle specie, basato ovviamente solo sulle conchiglie. Il testo comprende osservazioni originali e descrizioni di alcune specie e varietà nuove.

PASCAL, P. † (ed.), **Nouveau Traité de Chimie Minérale,** Vol. XV, Issue V, Masson & C.ie, Paris, 1970, xliv+812 pp., figs. and tables, 220 F.

Ce cinquième fascicule (du tome XV) complète le troisième, consacré aux « Transuraniens », c'est-à-dire qu'il a pour objet: le Neptunium, le Plutonium, l'Américium, le Curium et les curides, ainsi que les composés connus de tous ces éléments. Pour chaque élément transuranien, après une étude de l'isotopie et des propriétés nucléaires, les auteurs ont insisté sur les méthodes de séparation et de purification.

PEREIRA DE QUEIROZ, M. I., **Riforma e rivoluzione nelle società tradizionali** (Storia e etnologia dei movimenti messianici), (Series: *Saggi*, No. 27), Edizioni Jaca Book, Milano, 1970, 274 pp., L. 2.200.

Questo libro esamina i movimenti messianici in quanto « capitoli della dinamica sociale ». Lo studio di Maria Isaura Pereira de Queiroz rappresenta il tentativo di enucleare dei punti di approccio del fenomeno che finora non erano stati particolarmente approfonditi. Da un lato specifica la genesi dei messianismi in relazione unicamente ad un determinato tipo di struttura sociale, dall'altro fissa nell'ultima parte del libro una serie di elementi che tendono a mettere in luce il tipo di dinamismo sociale caratteristico dei movimenti messianici.

PIDDINGTON, J. H., **Cosmic Electrodynamics** (*Interscience Monographs and Texts in Physics and Astronomy*, Vol. XXIII), John Wiley & Sons, London, 1969, xii+306 pp., figs. and tables, £8.75.

Contents. - *I: Introduction.* - *II: Principles of Cosmic Electrodynamics.* - *III: Cosmic Plasmas.* - *IV: Electrodynamic Effects of Universal Occurrence.* - *V: The Sun and Solar Activity.* - *VI: The Interplanetary Medium and Geomagnetic Cavity.* - *VII: The Earth's Magnetosphere and Tail.* - *VIII: Geomagnetic Disturbance and Related Effects.* - *IX: Theory of the Earth's Radiation Belt, Aurora and Ionospheric Currents.* - *X: Planets, Satellites and Comets.* - *XI: Stars and the Interstellar Medium.* - *XII: Galactic Forms and Activity.* - *XIII: Radio Galaxies, Quasars and the Universe.*

POSESCU, Al. (ed.), **Aspecte din Filozofia Contemporana,** Editura Academiei Republicii Socialiste România, Bucuresti, 1970, 322 pp., Lei 20,50.

Contents. - *I: Realismul Critic al lui Nicolai Hartmann,* by Al. Posescu. - *II: Fenomenologia - sau lupta limbajului cu sine însusi,* by H. Wald. - *III: De la fenomenologie la existentialism,* by Al. Boboc. - *IV: Existentialism si axiologie marxista,* by C. I. Gulian. - *V: Din istoria existentialismului american si spaniol,* by N. Maris. - *VI: Structura fiintei la Louis Lavelle,* by I. Ivanciu. - *VII: Intuitie, expresie, limbaj în Estetica lui B. Croce,* by M. Piscoci. - *VIII: Gindirea estetica a lui Ch. Lalo,* by M. Brucar. - *IX: Conditia filozofiei si a istoriei sale la Etienne Gilson,* by Gh. Vladutescu. - *X: Jean Wahl - istoric al filozofiei si metafizician,* by Gh. Cazan.

Proceedings of the Second International Congress of Primatology, Atlanta 1968, 3 vols., S. Karger, Basel, 1969, Vol. 1: **Behavior,** by C. R. Carpenter, viii+282 pp., figs. and tables, SFr. 75; Vol. 2: **Recent Advances in Primatology,** by H. O. Hofer, vi+224 pp., figs. and tables, SFr. 62.50; Vol. 3: **Neurology, Physiology, and Infectious Diseases,** by H. O. Ofer, vi+204 pp., figs. and tables, SFr. 62.50.

The variety of papers that were presented during the Second International Congress symbolizes the great diversity of research and development work of contemporary primatologists. In addition, these papers give evidence of efforts to understand man's evolution and that of his close relatives and show active interest of many scientists in both conserving and using monkeys and apes to advance the biological and behavioral sciences as well as the medical arts and requirements for the health of human populations.

QUINE, W. V. O., **Parola e oggetto** (Series: *La Cultura,* No. 31), Il Saggiatore, Milano, 1970, xlviii+354 pp., L. 4.000.

Contents. - *I: Linguaggio e verità.* - *II: Traduzione e significato.* - *III: L'ontogenesi del riferimento.* - *IV: Capricci del riferimento.* - *V: Regimentazione.* - *VI: Fuga dall'intensione.* - *VII: Decisione ontica.*

RAIKOV, B. E., **Karl Ernst von Baer 1792-1876** (Sein Leben und sein Werk), (Series: *Acta Historica Leopoldina*, No. 5), Johann Ambrosius Barth, Leipzig, 1968, 516 pp., figs., DM 68.

CONTENTS. - *I: Lehrjahre. - II: Professor an der Universität Königsberg/Pr. - III: Leben und gelehrte Tätigkeit in Russland. - IV: Biologische und philosophische Anschauungen K. E. Von Baers.*

RAMSEY, P., **Fabricated Man** (The Ethics of Genetic Control), Yale University Press, London, 1970, 174 pp., £3.45.

The author examines the ramifications of genetic control, not only the now common measures of birth control and artificial insemination, but also the effect on the genetic pool of the increasing numbers of genetically poor individuals. His concern for humanistic values informs each argument as he tackles such issues as asexual reproduction of men, frozen semen banks, and the breeding of human beings for special purposes.

RAVAULT, M.-P., **Syndromes en Ophtalmologie** (Guide-Dictionnaire), Editions Doin, Deren & C.ie, Paris, 1970, 420 pp., 72 F.

Dans une première partie, ce livre se présente essentiellement comme un *Dictionnaire Clinique* où ont été répertoriés par ordre alphabétique de l'auteur ou des auteurs les différents syndromes, maladies ou signes intéressant l'ophtalmologiste. Une description de la maladie ou du syndrome précède toujours les signes oculaires qui ont été rapportés dans la littérature. La deuxième partie du livre comprend deux chapitres distincts, destinés à faciliter les recherches et l'exploitation de ces nombreuses données, cliniques, analytiques, très polymorphes.

RIEDL, R., **Fauna und Flora der Adria** (Ein systematischer Meeresführer für Biologen und Naturfreunde), 2nd rev. ed., Verlag Paul Parey, Hamburg, 1970, 702 pp., figs., DM 64.

Der « Riedl » ist 1963 als neuer Typ eines systematischen Meeresführers zum erstenmal erschienen. Er hat sich in kürzester Frist als zweckmässiger Führer durchgesetzt und von der Adria aus nachhaltig die Erforschung des Mediterrans beeinflusst. Die dabei gewonnenen Erkenntnisse sind nicht ohne Rückwirkung auf die nun vorliegende zweite, neubearbeitete Auflage geblieben. Die bewährte Struktur des Buches ist beibehalten worden. Inhaltlich entspricht die Neuauflage durch umfassende Überarbeitung dem jüngsten Stand der Adriaforschung. Alle inzwischen für diesen Meeresteil nachgewiesenen grossen Tiergruppen sind vollständig aufgenommen worden. Sie reichen von der Familie bis zur Klasse. Manche Kapitel mussten hierfür stark erweitert, andere hinzugefügt werden. Viele hundert neue Beobachtungen über Vorkommen, Biologie und Verhalten der Arten, deren Zahl sich insgesamt um etwa 10% erhöht hat, waren zu berücksichtigen. Bei einigen Arten hat ein Austausch stattgefunden, ween es sich gezeigt hat, dass andere Repräsentanten häufiger sind oder eine Gruppe typischer vertreten. In grösserem Masse werden in der Neuauflage auch die jahreszeitlich bedingten Erscheinungen berücksichtigt und durch Monatszeichen I- XII hervorgehoben. Farbtafeln haben nun auch die Hauptgruppen der Algen erhalten. Schliesslich konnten durch Tausch unter den Bearbeitern die kapitelweisen Einzeldarstellungen noch besser aufeinander abgestimmt werden. Der grosse Erfolg des « Riedl » beruht vor allem darauf, dass er einen sonst fast unübersehbaren Stoff nach natürlichen Verwandtschaftsgruppen ordnet. Dabei bedient er sich eines Leitzahlen benutzenden Übersichtssystems. Es führt zu den Gattungen und Arten über Systemgruppen, die für Forscher und Praktiker bedeutsamer sind als die niemals vollständig beschreibbaren Repräsentanten. Auf diese Weise werden mehr als 1 500 Arten mit über 30 000 Merkmalen geordnet. Zugleich wird auch die Bestimmung der nicht namentlich aufgeführten, aber zu den gleichen Gruppen gehörenden Arten ermöglicht. Auch die Tafeln mit insgesamt fast 3000 Abbildungen fassen jeweils die systematischen Gruppen zusammen. Jedem Namen sind die wichtigsten Synonyme sowie deutsche, kroatische und italienische Vulgärnamen begegeben. Schliesslich werden selbst Fragen der Untersuchungs-, Versand-, Hälterungs- und Präparationsmethoden behandelt.

ROSE, J. (ed.), **Progress of Cybernetics,** 3 vols., (Proceedings of the First International Congress of Cybernetics, London, 1969), Gordon and Breach Science Publishers, London, 1970, 1420 pp., figs. and tables, set £20.15.

CONTENTS. - **Volume I.** - *Main Papers.* - Section I: *The Meaning of Cybernetics (Definition and Philosophy).* - Section II: *Neuro- and Biocybernetics.* - **Volume II.** - Section III: *Cybernetics and Industry (Automation).* - Section IV: *Social and Economic Consequences of Cybernetics (including Management).* - Section V: *Cybernetics and Artifacts.* - **Volume III.** - Section VI: *Cybernetics and Natural Sciences.* - Section VII: *Cybernetics and the Social Sciences.*

ROSE, J. (ed.), **Survey of Cybernetics** (A Tribute to Dr. Norbert Wiener), Iliffe Books, Ltd., London, 1969, 392 pp., figs., £5.25.

The book is divided into five parts, to cover these diverse topics. The first part, 'The Nature of Cybernetics', describes the basic concepts, the tools of the cybernetic age (computers, feedback-controlled automatic machines) and the future possibilities. Part 2, 'Cybernetics and Living Systems', is concerned with behavioural, medical and physiological aspects. Broad principles involved in intelligent behaviour are analysed, the properties of the living brain are described and medicine itself is studied in terms of a teaching system. The section 'Cybernetics and Artifacts' deals with information, learning machines, behavioural models and artificial intelligence control. Learning control systems are studied and cybernetic modelling is described with respect to the growth and development of systems. The fourth part 'Cybernetics and Industry', covers the various industrial applications involving control and man-machine systems, automation and the organisation of relevant information. The final section 'Cybernetics and Society' is devoted to management, education, social prospects, and the pathology of automation.

ROSS ASHBY, W., **Progetto per un cervello,** Casa Editrice Valentino Bompiani, Milano, 1970, 378 pp., figs., L. 4.500.

Quando Ashby scriveva questo libro le ricerche di teoria dell'informazione, di cibernetica e le applicazioni dei modelli cibernetici al cervello umano e ai processi nervosi in genere stavano appena prendendo forma. I risultati della ricerca condotta in *Progetto per un cervello* dovevano confluire con quelli delle altre discipline per disegnare quel campo di indagine interdisciplinare sulla comunicazione che oggi coinvolge insieme matematica, neurofisiologia, semiotica, biologia, teoria degli automi eccetera. Apparso nel 1952 questo libro è stato rivisto in seguito dall'autore.

ROSSEL, J. and E. JEANNET, **Physique générale,** Editions du Griffon, Neuchâtel. Diffusion Dunod, Paris, 1970, 582 pp., figs., 69 F.

CONTENTS. - *I: Notions fondamentales et constitution de la matière.* - *II: Mécanique.* - *III: Chaleur et thermodynamique.* - *IV: Phénomènes ondulatoires.* - *V: Electricité et phénomènes atomiques.* - *VI: Théorie quantique des systèmes atomiques. Interactions entre matière et rayonnement.*

ROSSI, B. and OLBERT, S., **Introduction to the Physics of Space,** McGraw-Hill Book Company, Düsseldorf, 1970, XIV+454 pp., figs. and tables, £9.35

This book describes and analyzes the physical phenomena occurring in space and gives special attention to those that involve charged particles (ions, electrons, and cosmic rays). The first of the two major sections deals with the individual behavior of charged particles; the second deals with their collective behavior. Between the two is a review of binary interactions (collisions of one particle with another particle or with a photon).

SALT, G., **The Cellular Defence Reactions of Insects** (*Cambridge Monographs in Experimental Biology,* No. 16), Cambridge University Press, London, 1967, VI+118 pp., figs., tables and plates, £2.50.

The principal mechanisms in insects to counteract infection operate at cellular level. This monograph examines these cellular reactions, especially phagocytosis of micro-organisms and encapsulation of larger parasites.

SCHOFIELD, R. E., **Mechanism and Materialism** (British Natural Philosophy in An Age of Reason), Princeton University Press, Princeton, 1970, 336 pp., $9.50.

By integrating the developments in all the areas of experimental natural philosophy, describing their connections and the influences on them of Continental scientific work, natural theology, and to a lesser degree social and institutional changes during the eighteenth century, Robert Schofield demonstrates that mechanistic concepts dominated interpretations from about 1687 to 1740. Thereafter they were replaced by materialistic concepts, particularly including theories about imponderable fluids. A revival of the mechanistic approach in the early nineteenth century made England a fertile field for ideas on the dynamic interaction of forces, a field usually associated with Kant and German naturphilosophie. Basing his work on both manuscripts and published materials, the author has also enriched his study with the observations of representative lesser figures of the period along with the major ones.

SEGRÈ, E., **Enrico Fermi Physicist,** The University of Chicago Press, London, 1970, XII+276 pp., plates, £3.15.

ENRICO FERMI (1901–54) was one of the greatest theoretical and experimental physicists of the twentieth century. He was the discoverer of the Fermi statistics (for an ideal gas composed of particles of half-integral spin), author of the Fermi beta ray theory, and the leading force in the experimental neutron work that culminated in the famous chain reaction at the University of Chicago on December 2, 1942. Unlike former biographies of Fermi, Emilio Segrè gives an image of the scientist, his methods, intellectual history, and achievements, in the framework of contemporary science. It is not excessively technical, but explains in layman's terms the scientific problems faced or solved by Fermi. It contains some especially illuminating previously unpublished material concerning Fermi's youth in Italy and the development of his scientific style.

SIEFKES, D., **Büchi's Monadiac Second Order Successor Arithmetic** (Series: *Lecture Notes in Mathematics*, Vol. 120), Springer-Verlag, Berlin, 1970, XII+130 pp., DM 12.

CONTENTS. - I. Decidability and Completeness of SC. *I: The Sequential Calculus SC. - II: Infinite Processes. - III: The Proof of the Normal Form Theorem. - IV: Inquiring into the Decision Procedure. - V: Derivations.* - II. Benefits of the Decision Procedure. *I: Definitions Involving Free Predicate Variables. - II: Definitions Involving Free Individual Variables. - III: Variants of SC.*

SPERANZA, F., **Relazioni e strutture** (Series: *Matematica Moderna*, No. 8), Zanichelli, Bologna, 1970, 224 pp., figs., L. 1.300.

CONTENTS. - *I: Nozioni sugli insiemi. - II: Le relazioni. - III: Applicazioni. - IV: Prodotto di relazioni. - V: Relazioni e grafi. - VI: Strutture algebriche. - VII: Strutture topologiche. - VIII: Strutture matematiche.*

STEWARD, J. W., **The Tailed Amphibians of Europe,** David & Charles Ltd., Newton Abbot (Devon), 1969, 180 pp., figs., plates and maps, £2.10.

CONTENTS. - *I: Evolutionary History of the Class Amphibia. - II: General Survey of the European Tailed Amphibians. - III: The Family* Hynobiidae. - *IV: The Family* Salamandridae. - *V: The Family* Plethodontidae. - *VI: The Family* Proteidae.

SUCHET, J. P. (ed.), **Influence des changements de phase sur les propriétés physiques des corps solides** (Séminaire de Chimie de l'Etat Solide, No. 3) Masson & C.ie, Paris, 1970, VI+194 pp., figs. and tables, 65 F.

Le lecteur trouvera ici les textes d'exposés faits aux Séminaires de Chimie de l'Etat Solide durant l'hiver 1968-1969. Un effort particulier a été fait pour réunir des travaux effectués dans des domaines aussi différents que: la métallurgie classique, les alliages magnétiques, les substances luminescentes, les semiconducteurs, les ferroélectriques, les verres.

SVELTO, O., **Principi dei Laser,** Tamburini Editore, Milano, 1970, VIII+262 pp., figs., L. 3.200.

Questo volume, che raccoglie gli argomenti svolti nel corso di elettronica quantistica del Politecnico di Milano, rappresenta il frutto di 5 anni di insegnamento ed espone i principi fisici e le caratteristiche più interessanti dei Lasers. Scritto in forma piana e scorrevole, costituisce il punto di partenza per la conoscenza dei concetti di base di questa nuova tecnica e richiede, per la sua comprensione, solo la conoscenza elementare della meccanica quantistica.

TAMBIAH, S. J., **Buddhism and the Spirit Cults in North-East Thailand** (Series: *Cambridge Studies in Social Anthropology,* No. 2), Cambridge University Press, London, 1970, XII+388 pp., figs., £5.25.

Dr Tambiah describes the religious practices and beliefs of the people of a remote village in North-east Thailand, relating them to the wider context of the civilization in which they are embedded. The village religion is described primarily through ritual. The author examines four main ritual complexes: (1) the rites performed by Buddhist monks, (2) *Sukhwan* ritual, concerned with recalling the escaped spirit essence of persons, (3) the cult of the guardian spirits or deities of the village, and (4) rites addressed to malevolent spirits which cause individual illness. The symbolism of each complex, the relationship between the four complexes in terms of hierarchy, opposition, complementarity and linkage, and the connections between the ritual system and the social structure are major issues examined.

TATEWSKI, W. M., **Quantenmechanik und Theorie des Molekülbaues** (Series: *Mathematisch-Naturwissenschaftliche Bibliothek,* No. 41), B. G. Teubner Verlag, Leipzig, 1968, 126 pp., figs., DM 14.

CONTENTS. - *I: Die grundlegenden Vorstellungen und Postulate der Klassischen chemischen Strukturtheorie. - II: Die Übergangsperiode in der Theorie des Molekülbaus. - III: Elementare quantenmechanische Vorstellungen und experimentelle Daten über den Molekülbau. Kritische Analyse einer Reihe von Vorstellungen der Strukturtheorie. - IV: Einige Grundprinzipien der Quantenmechanik und ihre Anwendung auf Probleme des Molekülbaues. - V: Der Aufbau molekularer Systeme nach der Quantenmechanik und die quantenmechanische Interpretation der grundlegenden Vorstellungen der Klassischen chemischen Strukturtheorie. - VI: Moleküle und Stoffe (Makrökorper).*

The Insects of Australia (A Textbook for Students and Research Workers), Melbourne University Press, Carlton, 1970, XIV+1030 pp., figs., $19.80 (Australian price).

This book, extensively illustrated in colour and black and white, provides an authoritative treatment of the present knowledge of the Australian insect fauna. Incorporating the work of thirty authors and with 2,400 illustrations, this volume is sponsored and arranged by the Division of Entomology, Commonwealth Scientific and Industrial Research Organization.

VALZELLI, L., **Elementi di psicofarmacologia sperimentale e clinica,** Carlo Manfredi Editore, Milano, 1970, XII+282 pp., figs. and tables, L. 8.400.

CONTENTS. - *I: Il cervello. - II: Il neurone. - III: I mediatori cerebrali. - IV: Metodi di studio in psicofarmacologia. - V: Correlazioni neurochimiche del comportamento. - VI: Gli psicofarmaci. - VII: I sedativi. - VIII: I tranquillanti maggiori o neurolettici. - IX: I tranquillanti minori. - X: Gli antidepressivi. - XI: Gli psicostimolanti.*

VEROLE BOZZELLO, V., **Le grotte d'Italia** (Guida al turismo sotterraneo), Bonechi Editore, Firenze, 1970, VIII+320 pp., figs., L. 4.500.

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speleologica. - XII: Glossario. - XIII: Distribuzione delle grotte turistiche sul territorio italiano. - XIV: Piemonte. - XV: Lombardia. - XVI: Venezia Tridentina. - XVII: Veneto. - XVIII: Friuli e Venezia Giulia. - XIX: Liguria. - XX: Emilia e Romagna. - XXI: Repubblica di San Marino. - XXII: Toscana. - XXIII: Marche. - XXIV: Umbria. - XXV: Abruzzo e Molise. - XXVI: Lazio. - XXVII: Campania. - XXVIII: Lucania. - XXIX: Puglia. - XXX: Calabria. - XXXI: Sicilia. - XXXII: Sardegna.

WAISMANN, F., **Analisi linguistica e Filosofia** (Una nuova prospettiva), (*Collana di Filosofia e Epistemologia*), Ubaldini Editore, Roma, 1970, 270 pp., L. 3.800.

La serie di saggi raccolti in questo volume costituiscono lo sforzo di Waismann per superare il programma della dissoluzione dei vecchi problemi metafisici, e raggiungere una nuova visuale penetrante. In questo volume sono trattati i problemi della distinzione tra analitico e sintetico, della verificabilità, degli strati linguistici, delle logiche alternative e della causalità.

WEINGARTNER, P. and ZECHA, G. (eds.), **Induction, Physics, and Ethics** (Proceedings and Discussions of the 1968 Salzburg Colloquium in the Philosophy of Science), D. Reidel Publishing Company, Dordrecht, 1970, x+382 pp., figs., Dfl. 65.

These Proceedings are devoted primarily to the three topics *induction and probability, foundations of physics* and *science and ethics*. The papers and discussions of the first section are concerned with probability theory in general and with the difference between subjective and objective probability, with problems of inductive logic, and with the relation between logical probability and mathematical statistics. Section Two contains contributions to the General Theory of Relativity, to questions of simultaneity in the Special Theory of Relativity, to the foundations of Quantum Theory, and to the Intertheory Relations. The third section deals with the moral responsibility of the scientist, with modern science and social responsibility, and with the positive and negative aspects of the effect of science and technology on ethical values.

WILSON, L. G. (ed.), **Sir Charles Lyell's Scientific Journals on the Species Question,** Yale University Press, London, 1970, LXII+572 pp., figs., £7.85.

Lyell's "Scientific Journals" reveal the intricate web of evidence drawn from geology, paleontology, anthropology, and the geographical distribution of species that finally convinced him that species had changed and evolved during time. In addition they help to explain the impact of Darwin's *Origin of Species*, published in 1859. The introduction by Leonard G. Wilson traces the development of Lyell's ideas on species prior to the beginning of the first journal and describes the years of Lyell's life during which he wrote his entries.

ZAPPAROLI, G. C., **La perversione logica** (I rapporti tra sessualità e pensiero nella tradizione psicoanalitica), Franco Angeli Editore, Milano, 1970, 146 pp., L. 2.500.

CONTENTS. - *I: Alcuni rilievi clinici. - II: La perversione sessuale e la perversione logica. - III: Alcuni problemi generali connessi con i meccanismi percettivi. - IV: La percezione motivata. - V: Il periodo della latenza sessuale e il perido della latenza aggressiva. - VI: Considerazioni conclusive.*

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HYPNOSIS: ITS PLACE IN PSYCHOLOGY

There has been a rebirth of interest in the scientific study of hypnosis since the Second World War. Although hypnotic phenomena are widely familiar, their interpretation is still unsettled. The interpretation as heightened suggestibility is insufficient because there are types of suggestibility (impersonal suggestion, social conformity, and placebo effects) that do not correlate with hypnosis. Explanation according to imaginative involvement holds promise. Hypnotically susceptible persons can be characterized in part according to EEGs and eye deflection in mental problem-solving; studies of twins show that there may be a genetic component. Distributions of hypnotic susceptibility indicate that a few unusually « gifted » hypnotic subjects may compose an upper portion of the distribution of susceptibility with some qualitative differences from ordinarily high subjects. Controversies go on between iconoclasts and open inquirers over the interpretation of hypnosis, but there are substantial areas of agreement. The dangers of hypnosis have been exaggerated.

1. Introduction. — The royal commission of scientists appointed by the king of France to study Franz Anton Mesmer's claims regarding « animal magnetism » reported in 1784 that there was no scientific basis for animal magnetism, and that the hypnotic-like behaviors that Mesmer and his associates produced could be the result of imagination. "Imagination without magnetism", the commission concluded, was enough to produce the effects, and "magnetism without imagination produces nothing". The commission was correct in its denunciation of animal magnetism, and it was correct in assigning a role to imagination. It might, however, have gone on to recommend a more thorough study of imagination, but that was not its assignment, and scientific psychology was not then ready to follow up on this opportunity. Hence one thing hypnosis is not is animal magnetism, and one thing it may be is a product of the imagination, but what more can we say about it?

If hypnotic phenomena were actually both insubstantial and capricious, the scientific world would have no obligation to bother with them. But that is not the case. Too many pregnant women have had comfortable deliveries with the help of hypnosis, too many terminal cancer patients have been relieved of pain prior to their final coma, too many teeth have been extracted under hypnosis without the aid of chemicals, for the facts to be ignored. Because hypnotic responses are genuine, even though illusive, they commend themselves to scientific study. The first reason to study hypnosis is one common to most basic scientific inquiry: to gain a deeper understanding of natural events, in this case the functioning of the human mind. Hypnotic phenomena belong centrally within psychological science — these include amnesia, hallucinations, sensory and motor distortions — and any complete system of psychology must attempt to understand them and include them within its scope. The second reason for studying hypnosis is also a reason that guides many other scientific investigations, the opportunity to learn more about the possible applications of scientific knowledge for the benefit of mankind.

Following a period of neglect after its heyday in the nineteenth century, hypnotic research has had a rebirth since the Second World War, and there are now a number of laboratories in various parts of the world seeking to establish its place in normal psychological science, and its appropriate applications in the medical sciences. There are now several speciality journals reporting research findings, and investigators have joined together in scientific societies, national and international, to provide for exchange of information and for mutual encouragement by those at work in the field.

2. THE DOMAIN OF HYPNOSIS. — One way to begin the study of hypnosis is to map out its domain, that is, the kinds of behavior hypnotized people engage in, and the ways in which they describe their experiences within hypnosis so that we can come to a general picture of what hypnosis is like. Many of the phenomena are widely familiar. For one thing, the subject may lose control over his normal voluntary behavior, so that he is unable to open his closed eyes, or to utter his name. He may gain some control over normally involuntary processes; he may learn to control the blood flow in his hands, so that, by withdrawing blood from his right hand it may become several degrees cooler than the left hand which he may not have controlled, or which he may have deliberately suffused with blood (Zimbardo, Maslach and Marshall, 1972). In addition, he may develop positive hallucinations, such as seeing a television program on a non-existent receiver or hearing music played on a non-existent record player. He may also develop "negative hallucinations", that is, blot out sensory effects from stimuli actually present. He may not feel normally painful stimuli, or he may not hear a pistol shot fired nearby. One characteristic of hypnotic performances is that the hypnotized person may forget when aroused from hypnosis, all (or most) of what was done under hypnosis; such posthypnotic amnesia was once considered to be the hallmark of the highly susceptible subject, called a *somnambulist* (after the name for a 'sleep-walker' who also, on awakening, does not remember what was done). The hypnotized subject may engage in other kinds of posthypnotic behavior, if a signal has been implanted within hypnosis calling for that particular behavior when the signal later occurs. For example, when the hypnotist takes off his glasses, the subject, although out of hypnosis, may feel an irresistible impulse to open a window, or to place a chair on a table — whatever the hypnotist told him he would do. These illustrations do not exhaust hypnotic phenomena. The subject may dream under hypnosis, and tell his dream to the hypnotist, he may show age-regres-

sion, and, though an adult, may again act as if he were a child. He may also experience various emotions, or show alterations from his normal personality. These behaviors have been known for over a century, and, in fact, little new has been discovered about that area which I am calling the domain of hypnosis. The behaviors are so varied that it becomes extremely difficult to discover any invariant that is to be called the essence of hypnosis. Although the metaphor of sleep is commonly used in inducing hypnosis, the hypnotic subject is not asleep either by phenomenal measures or by physiological indices of sleep; he may actively perform laboratory tasks, such as memorizing nonsense syllables. When we observe his overt behavior we may find him slumped passively in a chair, or actively delivering an oration. These varied behaviors do, however, have some common aspects.

a) *Responsiveness to suggestions.* Because most of the behavior just described is brought about through suggestions given by the hypnotist, one interpretation of hypnosis is that it is a state of heightened suggestibility. This seems so obvious that a number of books on hypnosis carry subtitles indicating that they are studies of suggestion or suggestibility. To test the suggestibility interpretation more precisely, it is necessary to study a variety of suggestions in order to find those that intercorrelate, and hence cluster together, and those that are unrelated to the others. Those that belong together, and are related to familiar hypnotic behavior, will remain within the domain of hypnosis, while those that do not relate will fall outside the domain. In describing a domain, it is important to know what territory lies outside as well as what territory lies inside the domain.

When unselected subjects are exposed to hypnotic procedures, and then scored for their successful responses to a variety of suggestions, it is found that those who are responsive to some suggestions tend to be responsive to others, and, correspondingly, those who are resistant to one kind of suggestion tend, in general, to be resistant to others. Expressed quantitatively, this means that the responses are positively correlated throughout, and in the language of factor analysis this says that there is a common factor running through the familiar hypnotic responses, just as there is a common factor running through many of the subtests composing an intelligence test.

If the same kind of study is carried out, but with behaviors less familiar in hypnosis, it turns out that there are kinds of responsiveness to suggestion that are not represented in this common factor. An old test of suggestibility, introduced by the French psychologist Alfred Binet, consists in a series of progressively heavier weights all of the same size. Each member of the pair is slightly heavier than the one preceding it, except, however, that eventually the weights are all equally heavy. A suggestible subject, comparing each weight with the one before will report "heavier", as he should, but he continues to say "heavier" after the weights are no longer increasing. How long he continues provides a measure of his responsiveness to an impersonal kind of suggestibility. This kind of impersonal suggestibility is not characteristic of the hypnotizable person, and thus helps to delimit the domain of hypnosis (Hull, 1933; Eysenck and Furneaux, 1945).

Another kind of suggestion is produced by the influence of other people who, along with the subject, are responding to some common feature in the environment. If a person's preferences or judgments are determined by those about him, the response is said to show social conformity. In one form of experiment, originated by Asch (1956), two lines of clearly unequal length are exposed, and each of a group of people is asked, in turn, to state which line is the longer. The experiment has been pre-arranged so that the members of the group first called upon all report that line as longer which is actually shorter; then the "innocent" member of the group is asked for his judgment. If he is compliant he will go along with the others, not believing his own eyes. Another kind of social influence is that of a persuasive speaker, especially if he is identified as an expert or a person of high prestige in the community. There is an element of gullibility in such socially influenced responses. It turns out that suggestibility of the kinds just described is not related to hypnotic suggestibility (Moore, 1964).

A third kind of suggestibility is that familiar to physicians as response to a placebo, that is to a neutral substance believed by the patient to contain some form of active medication. For example, in pain reduction through morphine, about a third of post-surgical patients find their pain reduced as much by a placebo as by morphine (when they believe that the placebo contains morphine) (Beecher, 1959). This surely looks like suggestibility, yet it is also unrelated to hypnosis. In a careful study of hypnotic pain reduction and pain reduction through placebo, it was found that those responding to each of the treatments were not the same people. Although there were, of course, some subjects responsive to both treatments, there were some responsive to the placebo and not responsive to hypnosis, and some responsive to hypnosis and not to placebo (McGlashan, Evans, and Orne, 1969).

The results of these and related experiments show that it is not a sufficient statement to identify hypnosis with suggestibility. To be sure, the hypnotic subject is responsive to suggestions of a particular kind and under particular conditions, but hypnosis is incompletely defined by suggestibility because there are several kinds of suggestion that lie outside its domain.

b. *Imaginative involvement.* If suggestibility is insufficient to characterize hypnosis, we need to find some supplementary basis for describing what the hypnotized subject experiences.

One candidate for consideration (going back to the French commission) is the role of an active imagination. We undertook in our laboratory a protracted study in which students about to be hypnotized were interviewed prior to hypnosis; many of them were interviewed again after they had been hypnotized. The results have been published by my wife, who was primarily responsible for the project (Josephine R. Hilgard, 1970). What she found, in brief, was that the more highly hypnotizable university students were capable of a high degree of imaginative involvement, and they had had a history of such involvements in childhood, often encouraged by parents who themselves enjoyed a life of imagination.

By imaginative involvement is meant an absorption in some kind of fantasy so real that ordinary reality is set aside; the experience is felt as actually being lived, and is savored as such. This involvement differs from a pathological condition in that it is a temporary involvement, and reality ties are readily restored. The scientist who enjoys science fiction, without any detriment to his career as a critical scientist, furnishes an illustration of this flexibility. The areas of involvement differ from person to person — reading, music, religion, an esthetic interest in nature, extensive daydreaming, some forms of physical activity (skiing, skin-diving, cave-exploring) — and any of these areas may provide a preparation for and a path into hypnotic experience.

Sarah was one of the avid readers who related her involvement in reading to her experiences in hypnosis. She began her reading with her mother, an origin common to many who developed this involvement. "She read to me a lot when I was two, three, four — the children's classics: *Heidi*, *Little Women*. I learned to read between three and five, the little picture books. She used to tell me stories of her childhood..." Sarah's enthusiastic absorption in reading has continued.

Sarah saw hypnosis as being, for her, very similar to her enjoyment of reading. Just as she identified with the experiences of the characters in reading, so in hypnosis she felt what she was supposed to feel. "You can take an attitude, establish a new viewpoint. You can feel something that isn't there (like hallucinated heat), or you can lose something that is there (like your hand). You can visualize what the author says — that's taking something on; you may visualize something gone, too...".

In a recent experiment conducted with some highly susceptible hypnotic subjects, the task that confronted the subject was to become analgesic to the pain of circulating ice water in which his hand was to be placed (Hilgard, 1971). Their successes were very high, as shown by the accompanying Figure 1. Although the hypnotist gave rather conventional suggestions that the hand and arm would become numb and insensitive, upon later questioning it was found that many of the subjects had introduced their own manner of achieving pain reduction. They did this commonly with the help of imagination, in which they removed themselves from the stressful situation. One young man imagined himself singing a duet with a friend, and was totally oblivious to the circumstances normally so painful that he could scarcely tolerate them in the waking state for even a few seconds. A young woman who had visited Paris the preceding summer recalled the Venus de Milo, without arms, and identified herself with the statue; without arms she could feel no pain. (She did report a slight tingling in her shoulder!)

The capacity to imagine vividly may have something to do with hypnotic hallucinations, and with the splitting of consciousness as in automatic writing, in which a hand may write something while the person is carrying on a conversation, unaware of what the hand is doing. This splitting is some-times described as dissociation, and is another way of thinking about aspects of hypnosis (Hilgard, 1972).

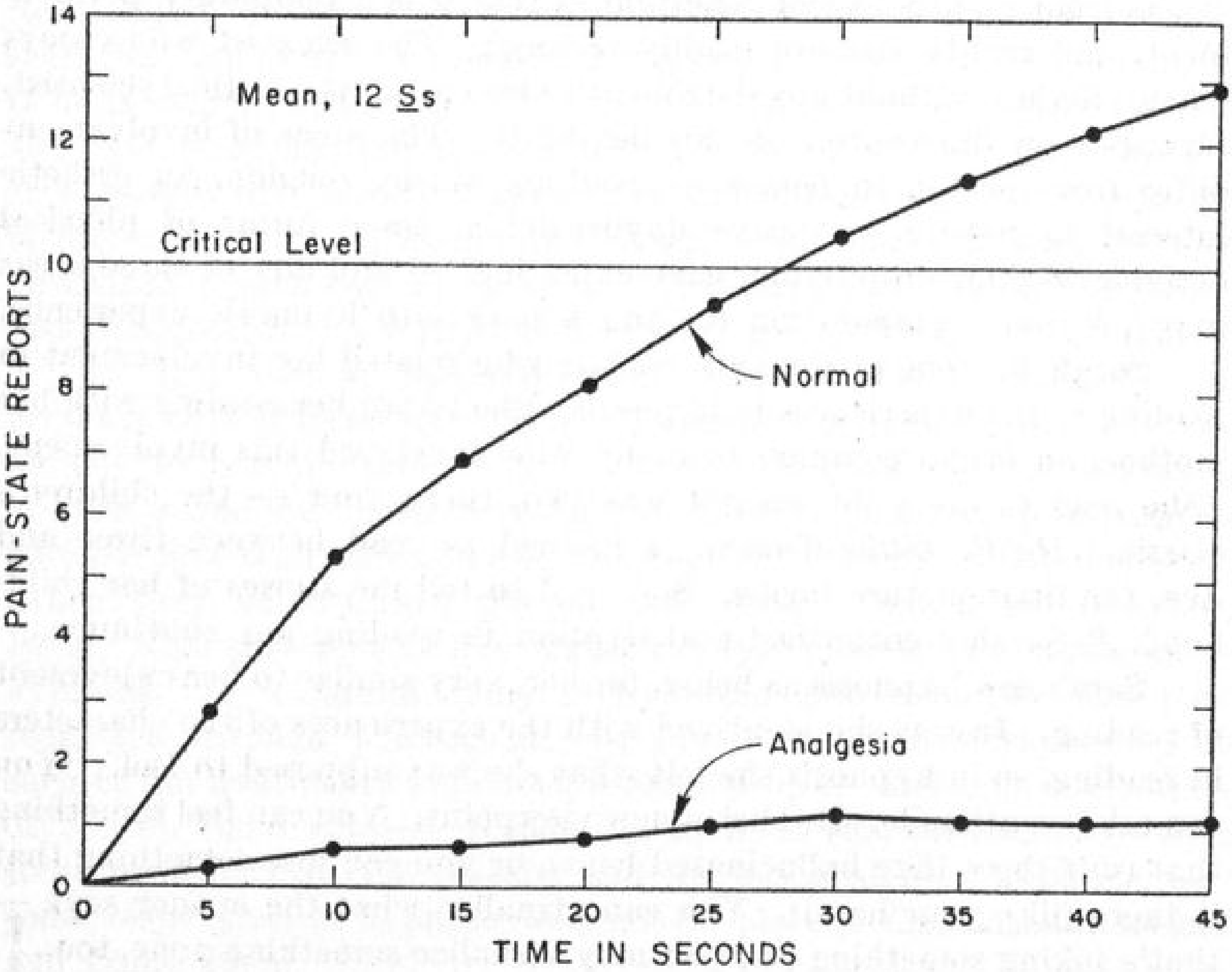

Fig. 1

Reduction of pain through hypnotic analgesia. Pain is reported on a scale from 0 as no pain to 10 as a quite severe pain which the subject would prefer to terminate, but he tolerates pain beyond this (in the normal state) and continues to count as shown in the upper curve. The same subjects when hypnotized and given the suggestion that they will not feel pain feel nothing but occasional mild tingling. The pain is induced by immersing the hand and forearm in circulating ice water. After Hilgard, 1971.

c) *Physiological correlates of hypnotic susceptibility.* It would be a great help if, in defining hypnosis, it were possible to turn to some physiological sign, similar to the manner in which the electroencephalogram (EEG) and rapid eye movements (REMs) have aided the study of sleep and dreaming. Unfortunately, no such indicator of the hypnotic state exists.

There are some hints, however, as to physiological signs that characterize the more highly susceptible person and distinguish between him and the one more resistant to hypnosis. These signs are available in the waking state, however, and do not define the hypnotic state.

The first of these is the relative amount of slow-wave EEG activity known as EEG-alpha. Such activity is commonly found when the eyes are closed and the person is relatively relaxed, as, for example, in a meditative state rather than in an analytical problem-solving one. People vary in the proportion of time that they show EEG-alpha in the ordinary resting state while sitting quietly in a chair with their eyes closed. It turns out in studies by others and confirmed in our laboratory that the higher the amount of alpha, the more responsive the person is likely to be to hypnotic procedures (London, Hart, and Leibovitz, 1968; Nowlis and Rhead, 1968).

The second sign is a rather stange one, whose relation to hypnosis was first studied in our laboratory. When a person is facing another, and is asked to solve a mental problem, such as to count the number of letters in the word "Minnesota", he tends to deviate his eyes while engaged in the task, looking slightly up and either to his right or to his left. Not surprisingly, he tends to look more consistently in one direction than in the other, some people being left-lookers, and some right-lookers. It turns out that left-lookers are more hypnotizable than right-lookers, an unexplained phenomenon doubtless related to something having to do with the unlike functions of the two hemispheres (Bakan, 1969).

Because of new knowledge that the two hemispheres have somewhat different functions (beyond one being dominant over the other) some of the workers in our laboratory have made a careful study of the alpha activity as recorded separately from the two hemispheres. They have confirmed the findings of others regarding differential alpha activity in the two hemispheres depending upon the mental task, alpha being reduced on the left in analytic, computational tasks, and on the right when more patterned or perceptive activity is undertaken; the relationship to differential hypnotic susceptibility was not established (Morgan, McDonald, and MacDonald, 1971).

If something having to do with brain structure affects hypnotizability, it would not be surprising if there might also be some hereditary component to hypnotic susceptibility. We have begun a study of twins and their families, and find a high correlation between the hypnotic performances of pairs of monozygotic but not between dizygotic pairs (Morgan, Hilgard, and Davert, 1970). The issues of heredity and environment are too complex to be settled by a simple correlation of this kind, but it does mean that the effects of heredity cannot be ruled out.

3. THE DISTRIBUTION OF HYPNOTIC SUSCEPTIBILITY. — Studies such as those just described could not have been performed without a method of measuring with some degree of accuracy the differences in hypnotic susceptibility of one person compared with another. For this purpose a number of hypnotic susceptibility scales have been constructed, including several from our laboratory. These scales usually require a standard attempted hypnotic induction, followed by a number of tests to determine whether or not the individual responds like a hypnotized person. Suppose there are 12 such test items; then the person can "pass" 0 to 12, and his "score" is the total passed. The kinds of items are illustrated in Table 1, which gives the items and the criteria of passing for Form C of the Stanford Hypnotic Susceptibility Scale. When such a test is given to a number of persons, a few score at the low end of the scale, a few at the high end, and most in the middle, as in most psychological tests. This simple method of measuring hypnotic susceptibility, while satisfactory for many purposes, is subject to some limitations.

TABLE 1

Items of Stanford Hypnotic Susceptibility Scale (SHSS), form C, with scoring criteria (Weitzenhoffer and Hilgard, 1962)

Item	Criterion of passing
1. Hand lowering (right hand)	Lowers at least 6 inches in 10 seconds
2. Moving hands apart	Hands 6 inches or more apart after 10 seconds
3. Mosquito hallucination	Any acknowledgement of effect
4. Taste hallucination (sweet, sour)	Both tastes experienced and one strong or with overt movements
5. Arm rigidity (right arm)	Less than 2 inches of arm bending in 10 seconds
6. Dream	Dreams well; experience comparable to a dream
7. Age-regression (school fifth and second grades)	Clear change in handwriting between present and one regressedage
8. Anosmia to ammonia	Odor of ammonia denied and overt signs absent
9. Arm immobilization (left arm)	Arm rises less than 1 inch in 10 seconds
10. Hallucinated voice	Subject answers voice realistically at least once
11. Negative visual hallucination (two of three boxes)	Reports seeing only two boxes
12. Posthypnotic amnesia	Subject recalls three or fewer items before "Now you can remember everything"

a) *A possible bimodality in hypnotic scores.* Sometimes the distribution of individual differences in hypnotic susceptibility does not conform to the usual unimodal curve approximating the normal or Gaussian distribution, but is distinctly bimodal, as in the distribution for amnesic responses shown in Figure 2. It appears that the left hand portion of the curve may result from ordinary differences in forgetting, possibly exaggerated by hypnotic procedures, while those who score on the right (those who have forgotten most and are most amnesic) may represent a distinct group of the more highly hypnotizable. This problem of a possible inherent bimodality in hypnotic scores has troubled me for some time, and I discussed it in a book that appeared several years ago (Hilgard, 1965, p. 221-228). Those of us who work with hypnosis find that most people are only moderately susceptible to hypnotic procedures, and the unusually high subject (the "somnambule") is quite rare. He may in some sense differ from the others, perhaps as

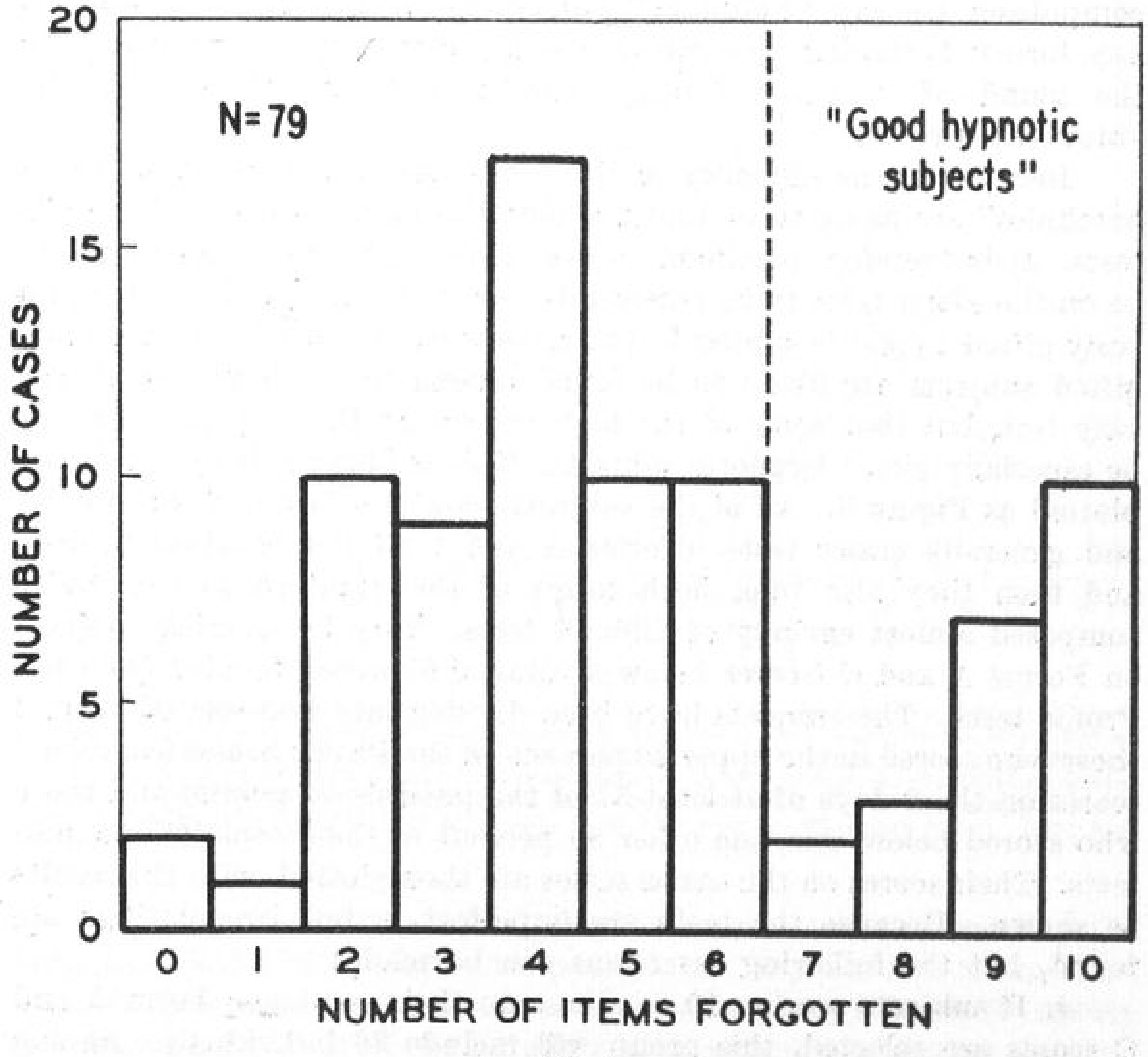

Fig. 2.

Bimodality in posthypnotic amnesia. Before being aroused from hypnosis the subject is told that he will not remember what the tasks were that he was asked to perform in hypnosis, until a release signal is given. His score then consists of the number of tasks forgotten, with the more forgotten the higher his amnesia. The scores plotted consist of items forgotten in a single session, with 10 items possible. The bimodality indicates the possibility of two overlapping distributions of slightly different hypnotic abilities. Data from Stanford laboratory.

a concert musician differs from the ordinary performer who has had a good teacher and has been conscientious about practicing. Perhaps the truly "gifted" hypnotic subject, like the concert musician or the mathematical genius, crosses some sort of threshold that distinguishes him from the usual high-scoring subject on the test.

Our tests are fallible, but I believe I can make the point by a demonstration, which while somewhat arbitrary, is based on actual data. Our most advanced test is called the Stanford Profile Test of Hypnotic Susceptibility (Weitzenhoffer and Hilgard, 1967). It is designed to locate the special abilities among very highly susceptible subjects, and yields several scores representing such abilities. Because of this purpose, many of its items are quite difficult, in the sense that very few unselected subjects yield the performances called for. These are such things as hallucinating a second light with the eyes open, when seeing a box with only one light on it; impairment of the ability to do simple arithmetic; carrying out an irrational posthypnotic verbal compulsion (to say "Febraury" whenever the number 3 appears in any form); becoming anosmic to strong ammonia; becoming deaf to the sound of a loudly ticking watch; carrying out posthypnotic automatic writing.

Because of the difficulty of the profile test items, the true "somnambules" are likely to be found among the high scorers on the profile tests. It is therefore pertinent to ask: How high does a score have to be on the easier tests to be reasonably sure that the test has selected a truly gifted hypnotic subject? The answer is, of course, that the more gifted subjects are likely to be found among the high scorers on the easy test, but that some of the high scorers on the easy test will not be especially gifted hypnotic subjects. This is illustrated by the scores plotted as Figure 3. All of the subjects took two forms of the simpler and generally easier tests, (Forms A and C of the Stanford Scales), and then they also took both forms of the Stanford Profile Scales composed almost entirely of difficult tests. Very low-scoring subjects on Forms A and C (scores below a total of 6) were excluded from the Profile tests. The subjects have been divided into two sets of people: those who scored in the upper 20 percent on the Profile Scales (combined scores on the 2 days of at least 37 of the possible 54 points) and those who scored below this, the other 80 percent of the sample of 135 subjects. Their scores on the easier scales are then plotted, with the results as shown. Because the tests are imperfect, a few irregularities are found, but the following assertions can be made:

i. If subjects scoring 19 or above on their combined Form A and C scores are selected, this group will include 36 individuals. Among them will be 23 of the total of 27 in the upper 20 percent of the more advanced profile scale scores; hence the original tests will have served as a useful screening device. At the same time there will be included 13 who will *not* score in the upper 20 percent of the profile scale scores, so that high scores on the easier tests are no guarantee of high performance on the more difficult scales.

ii. Any lower criterion of selection will draw in many more of the low scorers on the profile scales. If, for example, all scores of 17 or above are chosen, this will include 54 individuals. They will include 25 of the 27 high profile scorers, a satisfactory selection from this point of view, but they will include 29 low profile scorers, thus being less efficient in selecting high subjects for further experimentation. The price paid for an additional two high scorers has been to add 16 low ones.

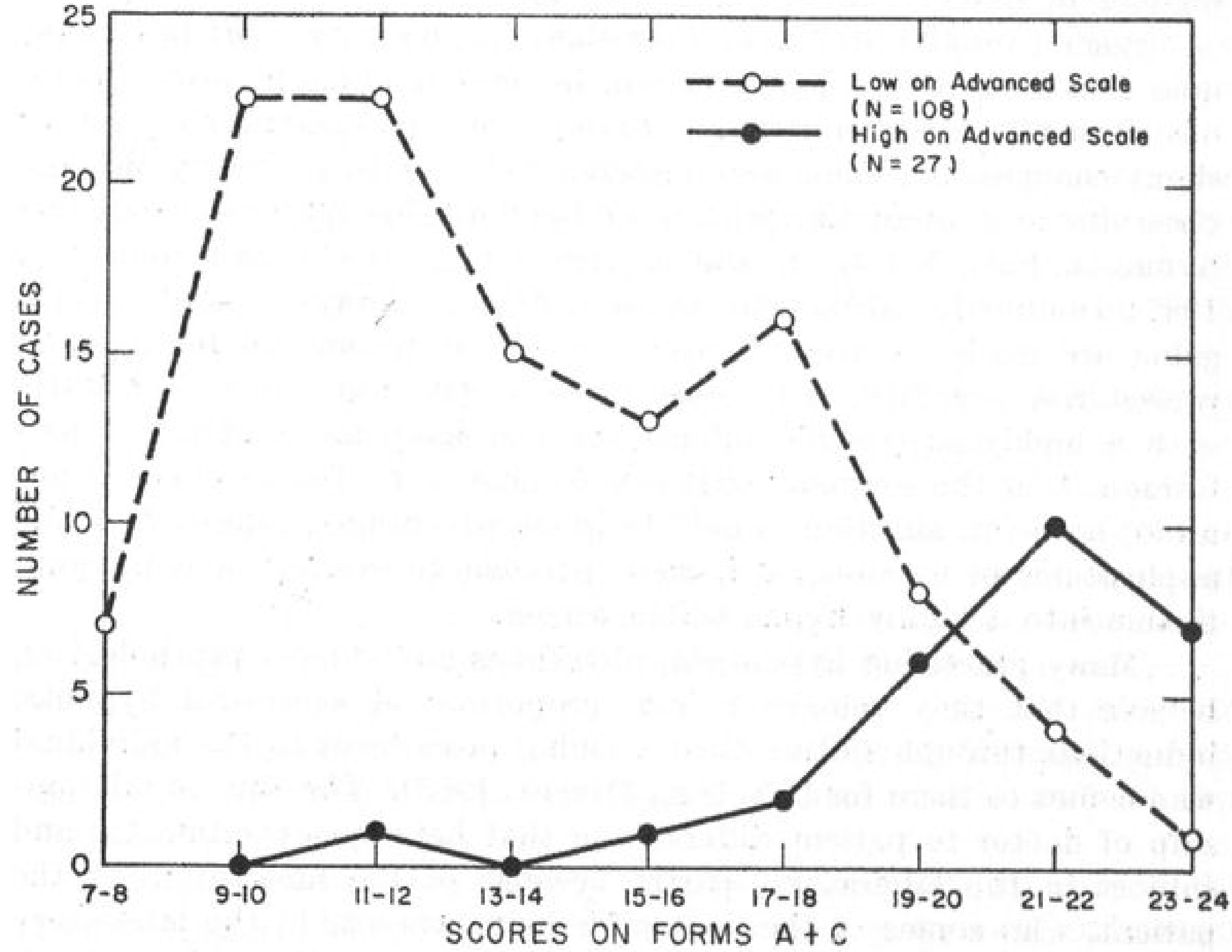

Fig. 3

Bimodality in hypnotic susceptibility scores. Subjects who had been tested on the easier scales (Forms A and C) were divided into two groups on the basis of their scores on the more difficult Profile Scales, Forms I and II. Those in the upper 20 percent of the profile scores form a distribution on Forms A and C quite distinct from those who score lower, although the distributions overlap as shown. It is possible that the subjects who score consistently high on all of the scales represent a group qualitatively different from those who score high only on the easier scales. Data from Stanford laboratory.

While the two curves of Figure 3 are not decisive regarding the basic bimodality of the hypnotic population, they are not inconsistent with it. The point is that some of those of moderate hypnotic ability can reach all the way on the simpler scales, without exhibiting the kind of hypnotic virtuosity that leads to very high profile scale scores. More subtle analyses have to be made to determine whether or not the hypothesis of special characteristics for the very gifted hypnotic subjects can be substantiated.

b) *The relative stability of hypnotic susceptibility scores, and attempts to increase susceptibility.* Under ordinary circumstances hypnotic susceptibility scores are remarkably stable, yielding correlations on retests over short periods of days or weeks of the order of r = .85, even though the tests are given by different hypnotists and in alternate forms on the two occasions. The stability is further attested by retests over periods of years. We have recently sought out graduates of Stanford University who were tested 8 to 10 years ago, and are now young adults in communities at some distance from the university. Their scores, even after this time, and despite the changes in their manner of living, still correlate r = .65.

Such a relative stability, when there has been no effort to change, does not mean that changes cannot be brought about by proper methods. Most of our earlier attempts to improve hypnotizability led to very slight changes, but some new methods introduced, especially through observing and interacting with more hypnotizable subjects undergoing hypnosis, have led to greater increases (e.g., Sachs and Anderson, 1967; Diamond, 1970). The usual finding is, however, that greater gains are made by those already somewhat responsive to hypnotic procedures, and there is little evidence to date that unless one starts with a highly susceptible subject the genuinely more advanced performances of the somnambulist can be achieved. The evidence is not all in, however, and there would be great advantages, especially in the applications of hypnosis, if it were possible to convert a nonhypnotizable into a highly hypnotizable person.

Many practicing hypnotists, physicians and clinical psychologists, believe that they achieve a high proportion of successful hypnotic inductions through suiting their training procedures to the individual who comes to them for help (e.g., Dorcus, 1963). The clinical relationship of doctor to patient differs from that between experimenter and subject in the laboratory, partly because of the motivation of the patient, who comes to the doctor for help, whereas in the laboratory the subject comes to serve science (or perhaps to be paid for his services). One thing that we do know is that for many of the therapeutic uses of hypnosis a very low degree of measured susceptibility suffices. Hence, unless the therapist is motivated to do some testing, he may be increasing the cooperation of his patient without in fact increasing his hypnotizability in a way that would be reflected in standardized scales of measurement.

4. CONTEMPORARY CONTROVERSY OVER HYPNOSIS: ICONOCLASTS VERSUS OPEN INQUIRERS. — Two American psychologists in recent years have gained considerable prominence through their attacks on the concept of hypnosis — placing hypnosis as a concept that psychology might well get rid of as chemistry got rid of phlogiston and physics

got rid of ether. These and their followers I call the *iconoclasts*.

The first of these is Theodore R. Sarbin, who espouses a general socialpsychological outlook known as role theory. According to him, much of behavior — and this includes hypnosis — is best described as enacting a role to which the person has assented (Sarbin and Allen, 1968). He believes that "hypnosis" and "trance" are unnecessary metaphors, and is in this respect iconoclastic (Sarbin, 1950; Sarbin and Andersen, 1967). While I am not prepared to go along with his interpretation of hypnosis, it is at least coherent with his interpretation of other psychological facts. For example, he denies the usefulness of the concept of anxiety (Sarbin, 1968), and does not accept the evidence for hallucinations in schizophrenia (Sarbin, 1967). Hypnosis seems to be caught in the same wide net that includes many other concepts. Much of hypnosis can readily be interpreted as role enactment (e.g., Sarbin and Lim, 1963); my main criticism is that once one has asserted that hypnosis is "nothing but" role enactment, many persistent problems appear to have been solved. Curiosity about the unknown is reduced because there is little left to puzzle over or to explain.

A second attack on hypnosis is that by another psychologist, Theodore X. Barber and those who have become aligned with him in advocating his views (Chaves, 1968; Johnson, 1970; Spanos, 1970; Spanos and Chaves, 1970). Barber commonly writes "hypnosis" in quotation marks to indicate his disapproval of the concept. He sees his viewpoint as the truly scientific one, as reflected in the title of his book, *Hypnosis: A scientific approach* (1969). He stakes his claim as a methodologist by proposing that the methods he follows are the only adequate ones for conducting investigations (Barber, 1967). Through a long series of papers under his authorship (69 of them cited in his book) his views have reached a wide audience.

What is this new paradigm that he recommends to correct the errors of others? The analysis that he proposes is in terms of antecedent and consequent variables, with the antecedent variables largely under control of the experimenter (independent variables), and the consequent variables behavior or subjective reports that follow (dependent variables). Among the antecedent variables that he proposes as important are (a) the subject's attitudes toward the test situation, (b) his expectations concerning his own performances, (c) his motivation to cooperate and not to resist, (d) the tone and wording of suggestions, and (e) the tone and wording of inquiries regarding subjective states. These variables are said to account for "a substantial proportion of the variance" in "hypnotic" behaviors.

He says also:

"When the effects of these variables have been systematically assayed, investigators may conclude that it is unnecessary and unparsimonious to postulate a 'hypnotic trance state' to explain response to suggestions of analgesia, time distortion, color blindness, deafness, amnesia, hallucinations, and so on". (Barber, 1967, page 474).

The problem with his list of variables is not that they are unimportant, but that they are intrusive in all sorts of psychological experiments, and experimenters have had to be very ingenious to control and circumvent them. Thus the problems are troublesome in the study of sense-perception and it is well known that results in threshold measurement differ if the subject is asked to guess, or if he responds only when his confidence is high. The somewhat elaborate methods of signal detection are designed to *evade* the influences of motivation and varying standards of confidence, in order to find something invariant in sensitivity (Green and Swets, 1966). Testimony experiments have been carried out for many years to determine the circumstances in which "witnesses" vary their testmony. It is well known that testimony varies under oath or when freely given, and the form of the question influences it ("Was there a gun?" "Did you see the gun?").

Barber has shown that the same variables are troublesome in hypnosis. He points out, for example, that under very liberal criteria of what is called hallucination, some 50 percent of subjects will yield auditory hallucinations, and some 33 percent visual ones; if, however, the subject testifies that he believes the hallucinated object is actually there, the figures drop to some 3 to 5 percent of the subjects (Barber, 1969, pages 167-168).

These facts can be interpreted in two ways. The one that he has adopted is to make hallucinations a function of the question asked, and not to seek for any firm criterion of actual hallucination; the other is of course to exercise some ingenuity in looking for what about hallucinations is sufficiently invariant as to make them appropriate objects of study. In other words, in any complex field of psychological study there are always disturbing variables, such as the subject's expectations, motivations, and resistances; these constitute what in information theory is called "noise" in the system, and these variables have to be circumvented as best they can. Barber has turned this around and assigned to the noise in the system the essential role; that is why he is able to explain away anything substantial about hypnosis according to his list of variables.

A curious effect upon the design of experiments by Barber and his associates is an underplaying of the individual differences that are so ubiquitous in hypnosis experimentation. It is not that he is unaware of them, for he has his own scale (the Barber Suggestibility Scale), and has done studies such as age changes in suggestibility. The reason for this underplaying is, however, that individual differences are measured as *consequents* of some antecedent condition (such as a standardized test), and so do not fit easily into an antecedent-consequent system in which the antecedents should be under the control of the experimenter. The way that the individual differences are usually disposed of by Barber and his followers is to assign unselected subjects to various experimental conditions by chance, thus throwing the individual differences into the background where they do not have to enter the data. It happens, however, that so many effects are different for low susceptible subjects and high susceptible ones that this design often misses essential data.

The rigidity of design procedures produces many experiments that yield nonsignificant differences between hypnotic conditions and other conditions, thus supporting the iconoclastic position. When the analysis is carried deeper, it is often found that the design was actually inappropriate to the underlying events, so that what appears very scientific is instead a kind of 'scientism'. The result is that inappropriately collected data are entered into the elegant framework of modern statistical devices such as analysis of variance.

As a small illustration of how the antecedent-consequent position can lead to a certain blindness on the part of the investigator, I may refer to a recent doctoral dissertation done under Barber's close supervision, he having acted the part of the co-experimenter with the student (Johnson, 1970). One of the groups to be used in an experiment on the quality of hallucinations was to be a "trained high suggestibility group" to be compared with an "untrained high suggestibility group". Each group consisted of 10 subjects randomly selected from the upper half of scores on a group test of hypnotizability. A random selection from the upper half is insufficient to assure highly susceptible subjects, as indicated earlier (Figure 3). Ordinarily, if one were to train a group of subjects for a particular performance, such as producing hallucinations sufficient to inquire about their quality, one would set some kind of criterion for success in training, and then choose for the next step those subjects who met the criteria. But this would violate the simple antecedent-consequent logic, for now the subjects would have been selected according to some outcome instead of according to some input. So all the selected subjects were *exposed* to training; nothing further is said of the success of the training, and all those selected for training are counted as "having been trained". It turned out that the trained high subjects yielded only two hallucinators among the 10, while the untrained yielded six of 10. This is not only an upset regarding the expected effects of training, but is wasteful in that it leaves for the study of the quality of their hallucinations only two trained subjects to be compared with six untrained ones, groups so small as almost necessarily to yield differences not significant statistically. These facts elicit no sign of questioning on the part of the investigator; his obligation is to state the conditions and to report the consequences. (He was misled by his approach to the data, and did not report the number who hallucinated in each group, but furnished me the information when I requested it of him). A position that seems impeccable may in fact mislead the investigator as well as those who read his reports.

It comes as no surprise that Barber assigns the "trance-theory" position to Orne and me (among others) because we continue to express some curiosity about hypnosis and the hypnotic state (or states). Barber and his followers, as already shown, are committed to operational definitions, to specifying the antecedent conditions, and to observing the outcomes or consequents of these conditions. The Barber position can readily be recognized as a derivative of American behaviorism, in a form that was popular for a decade or two after Bridgman announced the operational definition of concepts in physical science (Bridgman, 1927). Although modern experimental psychology has gone well beyond this simplified model, and is concerned with all manner of mediating processes, the older model of scientific method persists in some parts of the psychological community in America. Having myself been considerably influenced by this outlook in my youth, I am aware of its advantages from the point of view of clarity in communicating what was done and what was found out, when the model is properly used, but I am also aware of its pitfalls.

Those who adopt more conventional attitudes toward hypnosis are classified by Barber as state-theorists, on the assumption that they are preoccupied with explaining hypnotic phenomena as the inevitable products of the special state in which the hypnotic subject finds himself. Rather than contrast his antecedent-consequent approach with a state approach, I prefer to make a distinction between the iconoclasts and the *open inquirers*. The iconoclasts have a *thesis* to be defended, and the reader usually knows in advance how their experiments will turn out, or how the results will be interpreted; the open inquirers instead propose *hypotheses* to be explored, perhaps to be accepted, perhaps to be rejected. There is too much unknown about hypnosis to make it all accord with role-theory, or all dissolve into fragmented antecedent-consequent relationships.

One who through the years has well represented the position of the open inquirer is Martin T. Orne, who runs one of America's most active hypnosis research laboratories. He is very critical of any glib interpretation of hypnosis. In fact, he is best known for two major critical features that he has introduced into hypnotic experimentation. The first of these is the notion that the *demand characteristics* of an experiment may influence what is found, so that subjects who appear to be hypnotized may in fact simply be compliant, responding as they think they are expected to (Orne, 1962). The second is a rigorous method that he has introduced to distinguish between true hypnosis and sham role-playing, through a design that has come to be known as the simulator-real design (Orne, 1959, 1972). In this design, the "reals" are highly susceptible hypnotic subjects, and the "simulators" are nonhypnotizables who have consented to do their best to imitate hypnotic behavior. There are special requirements for the design to be properly used. The simulator must be relatively naive about the specific phenomena to be studied, and so should the real. Otherwise, few hypnotic events could not be simulated successfully. The instructions must be conveyed to the simulator by someone other than the hypnotist, and in such a manner that the hypnotist has no clues as to which is which. The simulator must know that the hypnotist is trying to detect simulation, and, if he does, that the experiment with that subject will end then and there. There are subtleties in interpretation. For example, if simulators and hypnotized subjects behave alike, it does not disprove hypnosis; it means that the experiment has to be redesigned if it is to furnish a critical comparison. Sometimes the simulators overdo matters, and score higher on hypnotic-like performances than the reals, and that is evidence of difference; sometimes the simulators do not do the things that the hypnotized subjects do. For example, in a study of posthypnotic suggestion, the simulators thought the experiment completed when they left the laboratory room, and did not give the posthypnotic response on a signal from the office secretary as they were leaving. The fact that the really hypnotized subjects did respond to the secretary's signal gave assurance that there was reality to the posthypnotic response, because the subjects (simulators and reals) had been treated exactly alike (Orne, Sheehan, and Evans, 1968). I mention these contributions by Orne to show that he puts hypnotic phenomena under severe strain through his experimental controls.

For those thoroughly familiar with the literature of hypnosis, the differences between some of the actual observations collected by Barber and by the rest of us are not very substantial. Barber is a prodigious worker, and has covered much the same territory as other workers on hypnosis, but always with a rigidity of interpretation, particularly in the abstracts at the beginning of his papers, or in the summaries at the end, so that those less familiar with the details are not made aware of considerable areas of agreement. For example, in a study by Barber and Calverley (1969) a large number of the characterizations of the hypnotic trance state were confirmed, and these observations are reported as significant empirical generalizations in Barber's book (Barber, 1969):

"Subjects who are exposed to a hypnotic induction procedure and subjects who are asked to place themselves in hypnosis manifest the following behaviors to a greater extent that control subjects: 'hypnotic' appearance (as indicated by limpness-relaxation, 'trance stare' and psychomotor retardation); reports of certain types of unusual experiences (such as feelings of unreality); and testimony of having been hypnotized" (page 112).

The following is added as another empirical generalization:

"A formal induction procedure is slightly more effective than the statement 'Place yourself in hypnosis' in facilitating response to test-suggestions and in eliciting a 'hypnotic' appearance" (page 113).

Anyone interested, as I am, in making more specific the domain of hypnosis, is encouraged by a further statement:

"Hypnotic subjects who obtain above average scores on one of the behavioral dimensions (response to test-suggestions, 'hypnotic' or 'trancelike' appearance, reports of unusual experiences, or testimony of having been hypnotized), also tend to obtain above average scores on other dimensions" (page 117).

At this point, however, Barber then proceeds to explain away the empirical findings as evidence for hypnosis, deciding that it would be more fruitful to distinguish among the four dimensions given in the last quotation, treating each as a distinct dependent variable, and doing away with such terms as *hypnotized*, *hypnotic state*, and *trance* (page 118). The same line of argument, based on the lack of more nearly perfect correlation among these criteria, could of course be used to dispose of such psychological concepts as memory, intelligence, or emotion, because in practice each of these concepts depends on several dimensions that correlate imperfectly with each other.

In the end, evidence for a sensible interpretation of hypnosis will accumulate and become accepted. The iconoclasts will have served some purpose in forcing others to state their assumptions more clearly. At present, compared with the open inquirers, the iconoclasts do little to increase our deeper understanding of the complex phenomena of hypnosis.

5. THE ALLEGED DANGERS OF HYPNOSIS. — Those asking for the first time about scientific studies of hypnosis commonly express fears about the dangers of hypnosis either to the person hypnotized, or to others who might be damaged by things a hypnotized subject would do to them if that subject followed the suggestions of an unethical hypnotist. The Svengali picture of hypnosis has been enhanced by such motion pictures as "The Manchurian Candidate", in which powerful control is achieved through hypnosis.

Let us look first at what may happen to the hypnotized person. The first stereotype, that he becomes passive and loses will or the capacity for decision, is patently false. Highly hypnotizable people in our laboratory studies are characterized by "strong" egos rather than by "weak" ones, and neurotics are less hypnotizable than those in the normal range. An experienced hypnotic subject can place himself in hypnosis when he wishes to, and can also bring himself out of hypnosis when he wishes to; he may find hypnosis a useful way in which to increase his self-control rather than to weaken it. There are occasional sequelae to hypnosis that are unfavorable, such as transient headaches, or periods of drowsiness or listlessness following hypnosis; these occasional episodes are enough to indicate that hypnosis ought to be done by competently trained psychologists or physicians familiar with personality dynamics (Hilgard, Hilgard, and Newman, 1961). At the same time there is no reason to become alarmed about the dangers of hypnosis; in some 7,000 cases of hypnotic induction in the Stanford laboratory over the years we have not had a single unfortunate incident that could not be cleared up very promptly. The question of abuse of a patient by an unethical hypnotist is another matter, but should not be blamed upon hypnosis any more than such abuse by any other form of psychotherapy in which the therapist-patient relationship is violated. Just because something happens within hypnosis does not mean that hypnosis is responsible.

The second question concerns the possibility that anti-social conduct can be produced through the implanting of posthypnotic suggestions by a designing hypnotist. There are some famous cases in the literature that have reached the courts. In the best known case a male subject was induced to rob a bank twice, bringing the money to the hypnotist. He killed two men in the course of the second bank robbery (Reiter, 1958). The courts found that the subject was insane at the time of the crime, and the hypnotist was incarcerated as an accomplice to the crime. The facts of the case are very complex. Both subject and hypnotist had been cell-mates in prison, and they shared a quasi-psychotic delusional system designed to save Denmark from the communists. Orne, in reviewing the case, has summarized the results as follows: "While hypnosis played some role in this case, it is by no means clear whether it accounted for the behavior of the subject or was, in fact, quite incidental it it" (Orne, 1962, page 173). Arguments among experimenters have gone on for many years, some believing that antisocial behavior can be induced through hypnosis, others denying it. The bulk of the evidence supports the belief that subjects under hypnosis are unlikely to carry out antisocial acts that they would not otherwise carry out. In agreement with Orne, Barber concludes his review of the court cases with the remark: "If 'hypnosis' played a role in these cases, this role may have consisted... in providing the subject with a rationale for justifying the behavior to himself and to others" (Barber, 1961). What this adds up to, as before, is that hypnosis provides no protection against criminal or anti-social tendencies that already exist, but merely because something happens within hypnosis, hypnosis is not necessarily the cause of the behavior.

6. Conclusions: The Place of Hypnosis in Psychological Science. — The large amount of laboratory and clinical work on hypnosis undertaken in the spirit of modern psychological science has resulted in a substantial body of data and a large number of quantitative observations leading to a firmer understanding of hypnosis. The findings throw light on a number of significant psychological problems of perception, learning, memory, emotions, personality, and social behavior, all problems lying at the heart of psychology.

The controversies within the field are not as disruptive as may appear when polemical statements are taken out of context. There is considerable agreement on the underlying findings, with enough still unknown that there is room for disagreements regarding interpretation. Hopefully future investigators will approach the topics of study with an open mind, and not permit their presuppositions to lead them to premature conclusions in what remains a puzzling field of inquiry.

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THE GEOMETRIC SUBSTRATUM OF PHYSICS

A PRERELATIVISTIC QUALITATIVE ARGUMENT AGAINST ABSOLUTE TIME

Contemporary physics is strongly dominated by the quantitative method, perhaps to an exten that one may speak of an unbalance between decision-making on the basis of a quantitative argument versus decision-making on the basis of a qualitative argument. This essay illustrates the nature of physical problems that permit and perhaps benefit from a more qualitative method of reasoning. The present objective is that of discussing a purely qualitative proof a of well-known fact: the untenability of the concept of absolute time. The line of argument is purely kinematic and independent of manifold structure, such as metric properties, except that the manifold should be differentiable. One finds that the qualitative approach features an extreme austerity in underlying assumptions, while on the other hand, more demanding requirements have to be met in the sense of a more discerning mathematical delineation of physical situations.

It is a common practice of man to make geometric displays of quantitative relations of quite arbitrary nature. Depending on the number of parameters, the display can be two-dimensional, three-dimensional or three + one-dimensional if we include kinematic displays.

The method of displaying hinges on the obvious possibility of replacing positional geometric variables by variables that have an entirely different meaning and no relation to geometry. The technique of displaying thus illustrates the preference of the human mind to order and classify nongeometric quantities in a geometric fashion.

It can happen that the number of variables to be displayed exceeds that what we can accomodate visually in our three-dimensional physical space. One may then take recourse to displaying families of curves or families of surfaces so as to condense more information in a visual diagram.

There is, of course, a limit to the usefulness of an overloaded visual display. As an alternative for accommodating a multivariable relation, one may also consider extending the dimensionality of the configuration space beyond that of our three-dimensional physical space. Yet, one retains the language of geometry in the sense of a pseudo visualization.

The deeper reason for taking recourse to such a process of pseudo visualization in a multi-dimensional domain is one that has to do with our basic need and faculties for ordering and classification. The human mind attempts to retain and extend the methods of ordering that are so quite naturally presented to man through the physical world of his sensory perception. After a little exercise it becomes easy to think in terms of positional and dynamical configurations even if the variables involved don't have the meaning of positional quantities and even if their number exceeds the dimensionality of physical space.

From physical geometry, mathematics thus made the step to the underlying and more abstract principles of ordering and classification that are presently available to us in the body of knowledge known as algebraic geometry, set theory and group theory. Conversely, armed with these new and improved concepts, one can also reexamine the ordering structure of our physical space which largely generated these very ideas. What are the new insights that have resulted from this inverse procedure?

By and large, the use of multi-dimensional configurations in physics preceded the critical reexamination of the physical space related to our sensory perception. Phase-space in statistical mechanics is a striking example of utilizing the ordering potential of multi-dimensional geometry.

A critical reexamination of physical space occurred later when it was discovered as a physical reality that time was an ordering parameter that could not be considered as completely independent of the spatial ordering parameters: the coordinates of an object in three-dimensional physical space. Prior to this discovery it had been postulated by Newton that time was *absolute* in the sense that it was completely independent of the spatial ordering parameters.

While from the beginning the transformational interdependence of the spatial ordering parameters was an obvious and immediate result of Euclid's physical geometry, the idea that time partook in this interdependence came as a shock to the world of science.

Historically, this change of mind came about through a number of optical interferometer experiments, some of which were motivated by the quest for establishing an absolute motion of the electromagnetic medium, then called « aether ». The impossibility of experimentally establishing any (uniform) motion subsequently led to the discovery of a family of transformations known as the Lorentz group. These transformations account for the physical equivalence of all inertial frames; they require that time transforms along with positional coordinates.

This first failing of the concept of absolute time came about through a quantitative argument. The Lorentz transformations permit very definite quantitative conclusions that can and have been carefully checked experimentally.

It was then found that electromagnetic theory, as it stood at that time, already met many of the requirements to be demanded from a description that treats position and time on an equal footing.

The more dramatic results of this conceptual revolution, however, came about when the same ideas were extended into the realm of mechanics. The discrepancies with respect to the Newtonian mechanics based on the concept of absolute time, show up primarily for extremely high velocities that are a substantial fraction of the free-space light velocity.

It is of interest to note that no qualitative kinematic criterion exists on the basis of which the absolute time hypothesis can be discarded without having to invoke the experimental need for extremely high velocities.

Is it possible to eliminate this quantitative aspect from the arguments that lead us to abandoning the concept of abosulute time? It is not unreasonable to ask this question, because we are confronted here with a purely conceptual decision: there is no immediate reason to expect that it is essential for such a decision to depend critically on a quantitative argument.

Philosophically, it would be more satisfying if an argument for discarding absolute time could be extracted from a purely qualitative observation of a simple every day kinematic situation. The question of whether or not absolute time exists would then be further reduced to its epistemological roots.

In the following I will attempt to present a purely qualitative argument to prove that the concept of absolute time is untenable. The argument may be a qualitative one, yet it is rather mathematical in nature. *Physically* this argument requires the existence of a continuous fluid flow; an assumption that is hardly outrageous. A good part of fluid dynamics operates on the basis of this assumption. Mathematically the argument requires the use of a theorem concerning the representation of a (one-parameter) Lie group as a continuous transformation in some multi-dimensional manifold. This theorem is a corollary of what is known as Lie's first fundamental theorem on finite continuous groups.

The key to the argument rests on the fact that a continuous flow of fluid may be seen as a physical realization of a point transformation in three-dimensional physical space. The point transformations associated with a steady flow can be shown to have a group property. In fact, one concludes on this group property by examining the properties of its group representation in three-dimensional space.

A nonsteady continuous flow can also be represented by a family of transformations in three-dimensional space; however, these transformations do not exhibit the group property. While the concept of stationarity depends on the choice of reference, the group property should certainly not depend on that choice. If the group property is to be retained, one finds that a nonsteady flow can only be given a four-dimensional group representation in space-time.

The mathematical essentials of the argument can be more precisely delineated if we first formulate the mentioned corollary of Lie's first fundamental theorem on infinite continuous groups,

Let

$$x^\lambda = f^\lambda (\underset{\circ}{x}^\sigma ; s) \qquad (\lambda, \sigma = 1, 2 \dots n) \tag{1}$$

denote a family of transformations from $\underset{\circ}{x}^\sigma$ to x^γ, depending continuously on the single parameter s.

Now consider the $\underset{\circ}{x}^\varkappa$ as constants of integration that can be eliminated between (1) and $dx^\lambda/ds = df^\lambda/s$. One thus obtains a system of n differential equations

$$\frac{dx^\lambda}{ds} = F^\lambda (x^\sigma ; s) \tag{2}$$

which has as general solution the family of transformations (1). The corollary of Lie's first fundamental theorem[1] on finite continuous groups now claims that the group porperty[2] of the family (1) is uniquely related to a structural feature of equations (2):

The family (1) has the group property if and only if the righthand member of (2) has the form

$$F^\lambda (x^\sigma ; s) = u^\lambda (x^\sigma) A(s) \tag{3}$$

where u is a function of x^σ only and A is a function of s only.

The group parameter s permits transformations of scale and origin. Hence, if

$$d\tau = A(s)\, ds \tag{4}$$

substitution of (3) in (2) gives the following alternate forms of equations (2)

$$\frac{dx^\lambda}{ds} = u^\lambda (x^\sigma) A(s), \text{ (5a); or } \quad \frac{dx^\lambda}{d\tau} = u (x^\sigma) \tag{5b}$$

The systems of equations (5a) and (5b) are of the prototype that have solutions of the form (1) which will obey the group property when considered as families of transformation.

A fluid flow, stationary in the classical Newtonian sense, is given by a system (5b) for $\lambda, \sigma = 1, 2, 3$. The spatial vector u^λ is the velocity of the flow. Its components are functions of position only, but not of time. It thus follows that a steady flow has the group property. The absolute time may be taken as the parameter of the group.

Now examine this very same flow from an arbitrarily moving frame of reference. This motion of the reference frame can be also described in terms of the Newtonian absolute time. The old velocity field u^λ

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[1] A proof of the related statement for a multi-parameter group can be found in: J. A. Schouten, Ricci Calculus, sec. ed., Springer, Berlin (1954) see p 202, eq. 3.13 and p 203, eq. 3.18. Also L. P. Eisenhart, Continuous Groups, Dover, New York (1961) p 20, eq. 5.15 and p 24. Specialization to the case of one parameter groups gives the stated corollary.

[2] Consecutive applications of (1) are again expressible in the form (1) etc.

now becomes $u^{\lambda'}(x', y', z', t)$. Hence the transformed system of differential equation (5b) now has the form

$$\frac{dx^{\lambda'}}{dt} = u^{\lambda'}(x', y', z', t), \qquad \lambda' = 1, 2, 3 \tag{6}$$

which fails to meet the group criterion stated in the corollary of Lie's theorem. The reason being that the form (6) resembles the form (2) rather than the form (5a) or (5b), because the right-hand member depends on the group parameter: the absolute time.

We are thus faced with the following contradiction. A steady fluid flow exhibits the group property. This group property permits a purely spatial three-dimensional transformational representation. The three-dimensional spatial representation fails when the same flow is viewed from a moving reference with respect to which the flow is not any longer steady.

To resolve this contradiction one has no alternative but to abandon the concept of absolute time. An examination of the functional dependence of the velocity vector $u^{\lambda'}(x', y', z', t)$ of a nonsteady flow shows that the assumed absolute time t is now lumped in with the spatial parameters x, y, z, thus reducing its status as a single and absolute ordering parameter to that of a parameter that has only ordering potential in conjunction with the positional parameters x, y, z. The functional dependence of $u^{\lambda'}$ on t does not, in general, permit the separation (such as in eq. 3) of a common factor A(t) for all three components simultaneously. Hence, no possibility exists to salvage the absolute time concept through the use of Lie's theorem. To distinguish between absolute time and the demoted time variable, the latter is frequently called the coordinate time.

Having established that absolute time is untenable we may now turn to the step of removing the oddity of a group representation that works only for steady flow but not for a frame of reference with respect to which this same flow is nonsteady. By seeking a group representation in the space of position variables plus time variable one is able to include nonsteady flow in the representational description of the group. Such a four-dimensional space-time group representation entails the definition of a new group parameter. Lie's theorem demands that this new group parameter is neither a function of the position variables nor a function of the coordinate time. In fact, this new group parameter should play the same role for space-time as the role assigned to absolute time in the three-dimensional case of steady flow.

Where the absolute time was assumed to have a unique meaning throughout the domain of physical space, the new group-parameter is required to have a unique meaning throughout the domain of space-time in which the continuous flow is assumed to take place. These are requirements imposed by the corollary of Lie's theorem. It thus follows that the new space-time group-parameter is a unique sequential ordering parameter that tells us how the nonsteady flow evolves in the same way as the absolute time served the purpose of a unique sequential ordering parameter to tell us how a steady flow evolves.

It is not the purpose of this essay to further identify the physical role and meaning of this new space-time group parameter. The original objective was to prove, on the basis of a simple but realistic kinematic situation, that the concept of absolute time is physically untenable.

The line of argument was based on the priority of the group property of a continuous fluid flow, steady or nonsteady. A minimum of structural requirements had to be assumed about the nature of space-time. All that is needed to complete the argument is that space-time is a differentiable manifold. No assumption had to be made about a metric or affine space-time structure.

The existence of a metric permits the definition of a space-time invariant proper-time. The space-time group parameter cannot, in general, be identified as the proper time associated with the moving point particles of the fluid, because the proper time as a dynamical variable normally depends on space-time position; a condition that would be imcompatible with the requirements of Lie's theorem.

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SPECIFICITÀ DEI MUTAGENI CHIMICI A LIVELLO DEL DNA[1]

Nell'ambito degli studi effettuati allo scopo di identificare sia il meccanismo d'azione dei mutageni chimici capace di provocare alterazioni stabili nella molecola del DNA che la natura di queste alterazioni, si sono succeduti tre momenti di diverso interesse; una prima fase che prevedeva tra il mutageno chimico ed il DNA un tipo di reazione semplice ed essenzialmente chimica, suscettibile di essere individuata anche in esperimenti di reazione diretta « in vitro »; una seconda fase che metteva in luce l'importanza della conformazione del DNA propria dell'organismo sottoposto a trattamento mutageno o dello stadio metabolico durante il quale un organismo veniva trattato; una terza fase, quasi completamente in corso di sviluppo, secondo la quale la conseguenza di un'azione mutagena sul DNA dipende da una serie di attività metaboliche cellulari, molte delle quali non sono state ancora sufficientemente chiarite. Attraverso questi tre momenti, si può ritenere oggi come sia possibile una più chiara definizione e caratterizzazione del tipo di reazione che interviene tra mutageno ed organismo vivente e quindi della possibile specificità di azione di una sostanza mutagena.

Gli effetti genetici dei mutageni chimici furono dimostrati nel 1943 da parte dei ricercatori inglesi C. AUERBACH e J. M. ROBSON e, indipendentemente, anche da parte del ricercatore tedesco F. OEHLKERS: da allora lo sviluppo delle ricerche sui mutageni è aumentato insieme al numero delle nuove sostanze capaci di tale attività.

L'impiego dei mutageni nella ricerca genetica è stato di grande interesse teorico, in quanto ha permesso lo sviluppo delle conoscenze sulla struttura e sulla funzione del materiale genetico.

Gli effetti dei mutageni chimici sul DNA dei cromosomi sono estremamente vari: essi sono rappresentati, in alcuni casi, da una distruzione estesa del materiale genetico oppure da alterazioni molto meno drastiche, quali la sostituzione di una coppia di nucleotidi con un'altra. Questa ultima classe rappresenta un tipo di alterazione sperimentale di grande interesse in quanto permette lo studio dei cambiamenti dell'informazione genetica contenuta nel gene e le conseguenze a livello dell'espressione di questo gene (mutazione genica).

Tra i mutageni chimici, i composti più importanti sono le sostanze alchilanti, l'acido nitroso, l'idrossilamina, gli analoghi delle basi nucleiche, i nitroso-composti, le acridine, la formaldeide, ecc.: per la maggior parte di queste sostanze sono state studiate le reazioni « in vitro » con il DNA, cercando di identificarne le alterazioni chimiche, si da permettere delle conclusioni circa le possibili alterazioni molecolari quali si realizzano « in vivo », a carico del DNA presente nelle cellule.

Da questi studi si è cercato di definire *la specificità* di una data sostanza mutagena, cioè la possibilità di indurre un tipo particolare di alterazione molecolare nel DNA che provochi un cambiamento nel fenotipo dell'organismo.

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[1] Pubblicazione no. 43 del Laboratorio di Mutagenesi e Differenziamento del C.N.R., Pisa (Italia).

L'estesa serie di ricerche sperimentali sugli effetti genetici dei mutageni chimici ha portato allo sviluppo di teorie della mutagenesi sviluppate da Freese (1959) [7] e, successivamente, da Brenner *et al.* (1961) [2]: queste teorie presentano alcune interpretazioni delle possibili alterazioni molecolari che si verificano nella molecola del DNA dopo il trattamento di una cellula con un mutageno chimico.

I possibili cambiamenti di tipo più semplice a livello della molecola del DNA sono rappresentati dalla sostituzione di un nucleotide con un altro (sostituzione di base = BS, transizione o transversione)[1] oppure da una inserzione o una delezione di un nucleotide (addizione o delezione di base = AD): il primo tipo di alterazione (BS) produce una mutazione missenso o nonsenso, a seconda della particolare tripletta di nucleotidi che si è prodotta nel sito in cui è avvenuta l'alterazione (triplette nonsenso: UAG, UAA, UGA; triplette missenso le rimanenti altre 61 combinazioni). A livello della proteina codificata da quel gene entro cui si è verificata l'alterazione noi potremo avere la sostituzione di un amminoacido con un altro nella sequenza peptidica corrispondente al segmento del gene entro cui è avvenuta l'alterazione, oppure, nel caso delle mutazioni nonsenso la sintesi di un frammento più o meno esteso della proteina, a seconda della posizione della tripletta nella sequenza nucleotidica del gene in cui si è verificata l'alterazione (Sarabhai *et al.*, 1964) [16][2]. Il secondo tipo di alterazione molecolare (AD) produce, con l'inserzione o la delezione di un nucleotide nella sequenza genica, una scivolamento delle unità di codice, per cui si avrà a livello della proteina corrispondente una sequenza in amminoacidi completamente alterata a partire dalla posizione in cui è avvenuta la mutazione[3].

Come è stato sottolineato da Krieg (1963) [9] nell'ambito delle ricerche sui mutageni si cercò di realizzare due tipi di evidenze sperimentali, una *mutazionale*, e l'altra *chimica*:

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[1] Si ha una « transizione » quando nella molecola del DNA una purina viene sostituita con una purina, oppure una pirimidina con una pirimidina: es., $A \leftrightarrows G$, $T \rightleftarrows C$; si ha invece una « transversione » quando nella molecola del DNA una purina viene sostituita con una pirimidina e viceversa: es. $A \rightleftarrows T$, $A \rightleftarrows C$, $G \rightleftarrows T$, $G \rightleftarrows C$.

[2] Nel 1964, Sarabhai *et al.* [16] identificarono il numero ed il tipo di peptidi della proteina di rivestimento del fago T4D sintetizzati dal fago « in vivo », allorquando vengono infettate cellule di *E.coli* con mutanti del fago contenenti delle mutazioni nonsenso nel gene strutturale della proteina di rivestimento: attraverso l'analisi di peptidi si vide che veniva sintetizzata una parte della proteina tanto lunga quanto distante era la mutazione nonsenso (tripletta nonsenso) dall'origine del gene. In mutanti nonsenso per il gene strutturale della β-galattosidasi di *E.coli* Fowler e Zabin (Science 154, 1027-1029, 1966) hanno trovato frammenti peptidici della proteina a peso molecolare tanto più basso quanto più vicina all'origine si era realizzata, attraverso la mutazione, una tripletta nonsenso.

[3] L'evidenza sperimentale di questo meccanismo è stata fornita dall'analisi della sequenza di aminoacidi nel lisozima del batteriofago T4 Tsugita, A. *et al.*: J. Mol.Biol. (1969) 41, 349-364); istidinoldeidrogenasi di *Salmonella typhimurium* (Yourno, J.: J. Mol. Biol. (1970) 48, 437-442); triptofano-sintetasi di *Escherichia coli* (Bramar, W. J. *et al.*: Proc. N.A.S. (1967) *58* 1499-1506). Nei geni strutturali di queste proteine sono state indotte in due punti, lungo il segmento genetico, due alterazioni, tipo « addizione » o « delezione », di tipo contrapposto, in modo che la sequenza di nucleotidi intermedia ai due punti in cui era avvenuta la mutazione fosse alterata per lo scivolamento delle triplette: nella proteina isolata da questi mutanti, contenenti nel gene strutturale di quella proteina una addizione di un nucleotide in un punto ed una delezione di un nucleotide in un altro punto del segmento genetico, nel tratto peptidico compreso tra le due mutazioni è stata trovata una sequenza di aminoacidi diversa da quella del ceppo selvatico. Qui di seguito un caso analizzato in *E.coli* per l'enzima triptofanosintetasi:

(1) l'evidenza *mutazionale* si basava essenzialmente sull'analisi di mutazioni *in avanti*[1] e di mutazioni di *ritorno*[1] indotte da una particolare sostanza mutagena, allo scopo di stabilire se il modello di mutazione fosse in perfetto accordo con quello previsto in base ad un certo tipo di specificità;

(2) l'evidenza *chimica* derivava essenzialmente dagli studi effettuati con le sostanze mutagene attive su particelle fagiche trattate in vitro (extracellulari) e si riferiva alle basi del DNA con le quali le sostanze reagivano e ai possibili loro prodotti di reazione.

Questi due tipi di prove sperimentali furono prodotti soltanto per pochi mutageni chimici, ed i risultati pubblicati non sono sufficienti a spiegare il meccanismo proposto come base dell'azione mutagena.

TABELLA I

*Tipi di transizione previsti in base ai risultati di analisi chimiche**

Sostanza mutagena	*Meccanismo principale*	*Transizioni previste*
5 - Bromouracile	Incorporazione nel DNA	GC → AT AT → GC
2 - Amminopurina	Incorporazione nel DNA	GC → AT AT → GC
Idrossilamina	Reazione con la citosina	GC → AT
Acido nitroso	Deaminazione delle basi	GC → AT AT → GC
Agenti alchilanti	Alchilazione della guanidina	GC → AT

* da ORGEL (1965) [12].

Nella Tabella I sono indicate le relazioni esistenti tra le *evidenze chimiche* e le previsioni sulla direzione delle transizioni prodotte da diversi tipi di mutageni: i dati raccolti derivano per la quasi totalità dagli studi effettuati sul batteriofago *T4* e sono basati su evidenze di tipo sia *chimico* che mutazionale.

ceppo selvatico
sequenza nucleotidica mRNA : ACC.UAU.CUG.CUG.UCA.CGC.GCC. GGC...
sequenza aminoacilica : Thr. Tyr. Leu. Leu. Ser. Arg. Ala. Gly...

mutante
sequenza nucleotidica mRNA : ACC.UUC.UGC.UGU.CAC.GGC.GCC.GGC...
sequenza aminoacilica : Thr. *Phe*. *Cys*. *Cys*. *His*. *Gly*. Ala. Gly...

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[1] Si definisce mutazione *in avanti* qualsiasi modificazione nucleotidica indotta in un gene selvatico o dominante di un organismo: in questo caso il gene selvatico o dominante è stato mutato in un gene recessivo; la mutazione *di ritorno* invece rappresenta qualsiasi modificazione nucleotidica nel gene recessivo, in modo che esso risulti riportato nella situazione originaria dominante o selvatica.

Una interpretazione sul tipo di specificità di una data sostanza basata su queste relazioni prevede, per esempio, che le sostanze alchilanti producano principalmente la transizione della coppia di basi GC alla coppia di basi AT, ma risulta che una tale conclusione è valida per il fago *T4*, ma non per altri organismi: Tessman *et al.* (1964) [23] hanno dimostrato che nei fagi *S13* e *ΦX174*, l'etilmetansulfonato (sostanza alchilante)[1] è capace di reagire con i quattro tipi di nucleotidi presenti nel DNA; per l'acido nitroso i risultati indicano una capacità di reazione simile.

Si potrebbe arguire che le differenze nelle risposte alle sostanze mutagene osservate tra il fago *T4* e il fago *S13* possono essere dovute alla diversa struttura del DNA, essendo *T4* un fago con struttura a doppia elica, mentre *S13* ha una struttura a singola elica: infatti, sottoponendo a trattamento con la sostanza alchilante N-metil-N'-nitro-N-nitrosoguanidina (NG) entrambi i tipi di fago (*T4* e *S13*), mentre tutti e quattro i tipi di nucleotidi reagiscono in *S13*, per il *T4* si osserva quasi esclusivamente soltanto la transizione $GC \rightarrow AT$. Ciò nonostante questi risultati non devono far concludere che la diversa risposta mutagena dipenda dalla struttura del DNA nei due tipi di particelle fagiche, in quanto dall'analisi mutagena effettuata con la NG su *Salmonella typhimurium* (Eisenstark *et al.*, 1965) [6], sulla base di evidenze *mutazionali* è stata dimostrata la produzione di entrambi i tipi di transizione: la maggior parte degli *auxotrofi*[2] prodotti con NG erano suscettibili di mutazione di *ritorno* se sottoposti ad un secondo trattamento con la stessa sostanza mutagena. Secondo gli Autori, il diverso comportamento dei due fagi nei confronti della NG suggerisce la possibile esistenza di diverse condizioni molecolari nel punto di replicazione del DNA.

Da questi pochi esempi risulta chiaro come la specificità necessaria per produrre un tipo particolare di alterazione molecolare nel DNA dipenda dall'organismo sottoposto ad analisi; ma, tuttavia, un cambiamento specifico nel DNA dipende anche dal tipo particolare di sistema genetico utilizzato per l'analisi delle sostanze mutagene.

Nel tentativo di interpretare, sulla base di evidenze *mutazionali*, i risultati di un'analisi dei cambiamenti di amminoacidi osservati in una serie di mutanti per la proteina A della triptofanosintetasi[3] di

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[1] L'etilmetansulfonato ($CH_3SO_2OC_2H_5$) è una sostanza fortemente reattiva per la molecola di DNA: è capace di trasferire il suo gruppo etilico (C_2H_5) ad una molecola di una della quattro basi nucleiche (in modo da alterare le proprietà di complementarietà della base lechilata) per cui avverranno con facilità delle sostituzioni di basi nucleiche nel DNA nelle replicazioni cellulari successive al trattamento con questa sostanza.

[2] Si chiamano *auxotrofi* quei mutanti di qualsiasi organismo che hanno subito mutazioni *in avanti* in geni dominanti responsabili della sintesi di particolari sostanze nutritive di base, quali gli amminoacidi, le basi nucleiche, le vitamine: in conseguenza di queste mutazioni i mutanti possono sopravvivere solo se vengono allevati in presenza di quelle particolari sostanze nutritive che essi non sono più capaci di sintetizzare.

[3] L'enzima triptofanosintetasi consiste di due proteine (A e B) la cui sintesi dipende da due geni distinti disposti nel DNA di *Escherichia coli* uno di seguito all'altro; quest'enzima è capace di metabolizzare « in vivo » l'amminoacido L-triptofano a partire da una molecola di indoglicerofosfato ed una di L-serina. Per questa proteina sono stati documentati a livello della sequenza di aminoacidi tutti i tipi possibili di alterazioni genetiche che potevano essere prodotti nel gene responsabile di quella proteina.

Esherichia coli, YANOFSKY (1965) [26] ha avanzato le seguenti riserve:

(1) non si può stabilire se una data coppia di nucleotidi reagisca con un mutageno allo stesso modo, indipendentemente dalla natura chimica dei nucleotidi circostanti;

(2) nel caso di una debole risposta, questa può indicare sia un meccanismo di mutazione poco frequente (nel caso di quel mutageno), sia un meccanismo normale ma con scarsa efficenza operativa in certe posizioni del DNA;

(3) nel caso di diversi possibili cambiamenti di nucleotidi, una forte risposta in una direzione può mascherare una debole risposta in un'altra direzione; questa quindi sarebbe riconosciuta inesistente per una data sostanza mutagena.

Allo scopo di chiarire i possibili meccanismi di azione delle diverse sostanze mutagene, basandosi sul presupposto che tutti gli effetti biologici e genetici dei mutageni chimici debbono dipendere dalle reazioni tra quei composti e gli acidi nucleici (per quanto, con l'AUERBACH, 1967) [1] si può ritenere che un'alterazione chimica nella molecola del DNA è una condizione necessaria, ma non sufficiente, per la produzione di una mutazione rilevabile a livello del fenotipo), sono stati sviluppati numerosi studi basati sulle analisi degli effetti chimici dei mutageni su molecole modello, quali singoli nucleotidi o polinucleotidi.

Queste analisi possono fornire delle evidenze indirette sulle possibili reazioni specifiche che determinano le mutazioni; per esempio:

(1) se un poli C viene trasformato chimicamente in poli U e questa molecola, a reazione avvenuta, perde le sue proprietà di appaiamento con un poli G ed invece si lega con un poli A, si può ritenere questa trasformazione come suscettibile di provocare un evento mutazionale (SINGER e FRAENKEL-CONRAT, 1969) [19][1];

(2) poichè le proprietà di appaiamento di ciascun nucleotide dipendono dalla presenza di certi particolari legami nella molecola, le reazioni che alterano questi legami possono essere ritenute reazioni potenziali con valore mutageno (SINGER e FRAENKEL-CONRAT, 1969) [19].

Da questi studi, tuttavia, è derivata tutta una serie di informazioni anche sull'influenza della conformazione delle molecole di acido nucleico sulla loro reattività verso mutageni chimici.

Un esempio è fornito dal comportamento della NG: è stato visto che questo composto è capace di reagire con la guanina quando questa base si trovi sotto forma di polimero, ma non con la guanosina, l'acido guanilico, GpU o CpG (SINGER e FRAENKEL-CONRAT, 1967) [18]: secondo questi Autori è possibile che la guanina in forma polimerizzata sia particolarmente più suscettibile alla reattività con NG. È stato accertato, inoltre, un diverso comportamento di questo mutageno, come di altri in relazione al tipo di acido nucleico analizzato.

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[1] Sperimentalmente si può sintetizzare una molecola di acido ribonucleico contenente soltanto la base nucleica citosina ripetuta moltissime volte (poliC): questa molecola può formare dei legami idrogeno quando viene messa in presenza di un'altra molecola sintetica contenente soltanto guanina (poliG), in modo da costituire una molecola di acido ribonucleico a doppio filamento. Se la molecola di poliC viene trattata con acido nitroso e poi sottoposta alla fase di costituzione di molecola a doppio filamento, si vede che il poliC trattato con acido nitroso forma legami idrogeno con una molecola di poliA, in quanto l'acido nitroso è capace di deaminare la citosina, trasformandola in uracile, per cui la molecola di poliC è stata trasformata in poliU che ora forma legami idrogeno con poliA.

Nella Tabella II sono indicati i valori relativi di mutagenicità di RNA del TMV (*Tobacco Mosaic Virus*) in due diverse condizioni e sottoposte a reazioni con cinque tipi di mutageni chimici: per entrambe le condizioni, la mutagenicità fu analizzata con o senza ricostruzione della particella virale (RNA e proteine).

Tabella II

*Mutagenicità relativa di mutageni chimici nei confronti di TMV-RNA in diverse condizioni**

Mutageno	*Mutagenicità*	
	RNA in acqua	*RNA nel virus*
Acido nitroso	34	110
Idrosilamina	11	1,3
Metilmetansulfonato	5	4
Etilmetansulfonato	4,5	8
Nitrosoguanidina	2	60

* da Singer e Fraenkel-Conrat (1969) [19].

Questi risultati possono tuttavia dipendere anche dalla diversa reattività chimica delle singole basi nucleiche: ad esempio, si sa che l'acido nitroso è capace di deaminare le basi e che la sua mutagenicità dipende dal rapporto relativo tra eventi mutazionali ed eventi inattivanti. Infatti, allorquando si tratta RNA nella particella virale con acido nitroso la guanina non risulta deaminata, mentre dopo il trattamento di RNA libero il 28% della guanina risulta deaminata: nel 1° caso la mutagenicità relativa è di 110, nel 2° soltanto di 34 (Tabella II).

L'influenza della conformazione del DNA (a singola elica o a doppia elica) sulla reattività ad un dato mutageno chimico è stata analizzata da Tessman (1968) [22] sottoponendo a trattamento con idrossilanina due fagi con DNA a conformazione diverse, *T4* (doppia elica) e *S13* (singola elica). Per il *T4* si ebbe una velocità di inattivazione di $5{,}4 \times 10^{-3}$/min./M, mentre per *S13* si ebbe un alore di $3{,}1 \times 10^{-1}$/min/M: la maggiore reattività dell'*S13*, per quanto esso abbia una quantità di DNA di circa 80 volte inferiore a quella del *T4*, dipende esclusivamente dalla sua conformazione a singola elica. Infatti, *S13* in forma isolata o incapsulata nella particella fagica presenta la stessa reattività e tutte e due le forme furono più reattive della forma replicativa di *S13*, che ha una conformazione a doppia elica.

L'importanza della conformazione molecolare del DNA in relazione alla specificità mutagenica si rileva anche dagli studi effettuati con la nitrosoguanidina (NG) su *Escherichia coli* da CERDÀ-OLMEDO *et al.* (1968) [3] i quali hanno dimostrato che allorquando si trattano cellule sincronizzate, le mutazioni si verificano quasi esclusivamente entro quei geni in cui il DNA si trovi in condizione di replicazione. Poichè il trattamento venne effettuato in assenza di sintesi di DNA, gli Autori escludono possa trattarsi di azione del mutageno sul complesso di replicazione (la DNA polimerasi) ed ammettono invece che la regione in replicazione, contenendo un DNA con conformazione diversa dalla struttura classica di WATSON e CRICK, deve essere particolarmente suscettibile alla NG proprio a causa della sua conformazione molecolare.

La specificità mutagenica, valutata sulla base della mutazione di particolari siti genetici, è stata dimostrata dipendere anche dalla natura genetica del sistema analizzato: nel fago *T4* SPEYER [20] ha descritto alcuni tipi di alterazione che ha ottenuto nel gene strutturale della DNA polimerasi (gene 43) ed ha presentato dati che dimostrano come la risposta ad un dato mutageno dipende dalla presenza di quelle alterazioni genetiche in quel particolare gene.

DRAKE e GREENING (1970) [5] hanno dimostrato che proprio nel caso di acido nitroso e del BUdR (5-bromodeossuridina) la specificità mutagena dipende dal modo in cui le lesioni primarie prodotte dal mutageno sulla molecola del DNA vengono interpretate dall'apparato enzimatico preposto nella replicazione del DNA.

È inoltre noto che molti organismi mantengono l'integrità del proprio materiale genetico mediante una serie di meccanismi enzimatici di riparo capaci di ridurre o eliminare gli effetti dannosi delle alterazioni provocate nel DNA: il meccanismo enzimatico di riparo più conosciuto è quello presente nei batteri, che riduce gli effetti dannosi dell'irraggiamento con luce ultravioletta (UV). Nelle cellule sottoposte ad un trattamento con UV si ha la formazione di dimeri del tipo ciclobutano tra i residui pirimidinici presenti nella molecola del DNA: esistono oggi numerosi dati che stanno ad indicare come la presenza di questi dimeri determini anche una distorsione nella struttura a doppia elica del DNA. I batteri, tuttavia, sono dotati di tre tipi particolari di riparo, capace specificatamente di eliminare quest'alterazione primaria prodotta da UV:

(1) la fotoriattivazione;

(2) l'eliminazione del segmento del DNA contenente il dimero e la successiva sintesi di DNA;

(3) la ricostituzione dell'integrità del DNA mediante processi di ricombinazione. I primi due meccanismi eliminano i dimeri primidinici dal DNA; il terzo è capace di riparare delle lesioni secondarie del DNA, che si sono formate nel primo ciclo di replicazione del DNA dopo il trattamento con luce UV e che sono rappresentate da interruzioni

nella catena polinucleotidica: in questo caso si ha un processo di ricostruzione a caso di molecole polinucleotidiche integre per mezzo di fenomeni di ricombinazione (Howard-Flanders *et al.*, 1968) [8].

Di tutti questi processi il primo riduce la frequenza di mutazione in quanto elimina delle alterazioni (dimeri della timina) che sono mutazioni allo stato potenziale; il 2° ed il 3° producono delle mutazioni sia direttamente, inserendo delle alterazioni molecolari stabili nel DNA, sia indirettamente, provocando delle alterazioni secondarie o delle discontinuità nel filamento del DNA, le quali rappresentano lesioni mutazionali potenziali. In generale si può dire che gli studi di questi processi hanno dimostrato che le mutazioni (almeno nel caso della luce ultravioletta) dipendono non solo dal numero delle alterazioni primarie indotte (ad es., numero di dimeri pirimidinici), ma anche da alterazioni secondarie e dal verificarsi di eventi cellulari successivi al trattamento mutageno: questi eventi sono rappresentati principalmente da processi metabolici di riparo che la cellula mette in atto allo scopo di ridurre il danno derivante dalle alterazioni provocate dal trattamento mutageno.

L'esistenza di meccanismi di riparo nei batteri fu dimostrata a seguito dell'isolamento di mutanti più sensibili del ceppo selvatico all'azione inattivante degli UV, in quanto incapaci di eliminare dal loro DNA i dimeri pirimidinici: fu anche dimostrato chimicamente che nei ceppi *UV-sensibili* non si ha liberazione nel citoplasma di dimeri pirimidinici e che la persistenza di questi prodotti nel DNA era la causa dell'aumento della letalità osservata dopo irraggiamento con UV di questi ceppi sensibili.

Un analogo meccanismo di riparo, specifico però per le lesioni primarie prodotte nel DNA da un mutageno chimico è stato individuato in *Bacillus subtilis* a seguito degli studi sviluppati da Reiter e Strauss (1965) sul composto alchilante metilmetansulfonato (MMS) [14]. Questa sostanza produce delle rotture nel filamento del DNA in conseguenza della metilazione dei residui guanosinici e della successiva idrolisi di questi composti dando così luogo alla formazione di siti apurinici lungo la catena del DNA: dopo il trattamento con MMS nelle cellule di *B. subtilis* è stata osservata una sintesi supplementare di DNA e, contemporaneamente, una conseguente riduzione del numero delle lesioni prodotte dall'alchilazione.

Strauss *et al.* (1966) [21] hanno isolato un mutante di *B.subtilis* sensibile all'azione letale di MMS; studi successivi hanno dimostrato, inoltre, che i meccanismi di riparo specifici per MMS o per luce UV non sono gli stessi, in quanto i mutanti *UV-sensibili* non sono *MMS-sensibili* e viceversa.

Alcuni studi hanno cercato di chiarire il meccanismo di riparo specifico per le sostanze alchilanti: la presenza di siti apurinici e di rotture di un singolo filamento nel DNA possono essere dimostrati sottoponendo ad analisi di sedimentazione in saccarosio alcalino il DNA estratto da cellule trattate con MMS in quanto si ha la rottura dei siti apurinici e la denaturazione del DNA in corrispondenza delle rotture di singoli filamenti: in generale, dopo trattamento con 25mM di MMS (20 minuti) il DNA di *B. subtilis* presenta una variazione nella velocità di sedimentazione in saccarosio alcalino: secondo Reiter *et al.* (1967) [15] queste osservazioni stanno a indicare che le rotture di un singolo filamento di DNA provocate da MMS sono soggette a riparo.

Prakash e Strauss (1970) [13] hanno dimostrato che incubando le cellule trattate con MMS per 40 minuti prima dell'estrazione del DNA e della analisi di sedimentazione si osserva un aumento della velocità di sedimentazione verso valori prossimi a quello del controllo non trattato. Al contrario, un ceppo di *B. subtilis* sensibile a MMS non presentò alcun aumento della velocità di sedimentazione, per cui si deve ammettere che questo ceppo non è capace di riparare rotture di un singolo filamento prodotte dall'azione alchilante del MMS. Lawley e Brookes (1968) [10] hanno dimostrato che sottoponendo a trattamento con mostarda azotata cellule di *Escherichia coli* si osserva una liberazione di materiale nucleotidico dal DNA nella misura di 400 nucleotidi per ogni singola alchilazione prodotta nel DNA, in dipendenza con il tempo di trattamento; nel ceppo sensibile al contrario, non si osserva questo fenomeno[1].

Non è stata ancora accertata l'eventuale dipendenza delle mutazioni dai meccanismi di riparo operanti nelle cellule di microrganismi trattate con mutageni chimici; tuttavia i ceppi sensibili a queste sostanze (e quindi probabilmente incapaci di quei processi di riparo) presentano una frequenza di mutazione molto più elevata di quella osservata negli esperimenti con i ceppi resistenti.

Loprieno e Schupbach (1971) [11] hanno studiato l'effetto della caffeina sulle mutazioni indotte dalla luce UV e dalla nitrosoguanidina (NG) nelle cellule del lievito *Schizosaccharomyces pombe*: da lungo tempo si conosce che la caffeina interferisce con i processi di riparo nei batteri trattati con UV (Witkin, 1958) [24], impedendo la rimozione del segmento di DNA contenente dimeri primidinici (Setlow, 1967) [17]: in *S.pombe* è stato accertato che la caffeina riduce le alterazioni premutazionali indotte da UV e la percentuale di eventi ricombinazionali meiotici, dimostrando così che anche negli organismi eucariotici gli eventi ricombinazionali sono responsabili dei meccanismi di riparo messi in atto dopo trattamento con UV (Howard-Flanders *et al.*, 1968) [8] in accordo con i dati ottenuti da Witkin e Farquharson (1969) [25] *in E.coli.* Dopo trattamento con nitrosoguanidina si osserva, in presenza di caffeina, una diminuzione degli eventi premutazionali di circa 5 volte; ciò indica che i meccanismi di riparo specifici per la nitrosoguanidina in parte sono gli stessi di quelli specifici per le lesioni da UV. In questo lievito sono stati isolati dei mutanti più sensibili all'azione inattivante sia dell'UV che della nitrosoguanidina: in alcuni di questi mutanti la frequenza di mutazione nei geni *ade6* e *ade7* (responsabili della biosintesi dell'adenina) aumenta di circa 100 volte dopo trattamento con nitrosoguanidina (Tabella III).

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[1] In generale quando si verificano alterazioni a livello del DNA per la produzione di dimeri pirimidinici o per rotture nel filamento o per alchilazione, i processi di riparazione successivi comprendono una fase di idrolisi del segmento nucleotidico interessato dalla alterazione con liberazione, quindi nel citoplasma di frammenti oligonucleotidici di diversa lunghezza.

TABELLA III

Frequenza di mutazioni in avanti indotte in ceppi UV-sensibili di S.pombe *dalla luce UV* (6.240 *ergs/mm*^{2}) *e NG* (10^{-3}*M*, 5 *min.*, *pH* 6,5).

Ceppo	*Mutageno*	*Sopravvivenza* %	*Frequenza di Mutazione* ($X10^{-4}$)
wt-972*	UV	81,9	1,32
»	NG	71,6	2,02
uvs 5-158	UV	0,3	45,00
»	NG	0,2	287,80
uvs 10-198	UV	0,3	9,18
»	NG	0,2	203,74

* ceppo selvatico

Questi processi di riparo sono comuni a tutti gli organismi: nell'uomo, ad es., le cellule somatiche cono capaci di eliminare i danni prodotti nel DNA dalla luce ultravioletta mediante la eliminazione del segmento di nucleotidi contenente il dimero della pirimidina; le cellule somatiche di individui affetti da *Xeroderma pigmentosum* non sono capaci di tale funzione, mentre riescono a riparare i danni prodotti da metilmetansulfonato e nitrosoguanidina[1] (CLEAVER, 1970) [4].

In conclusione, si può affermare che alla luce delle evidenze presentate dagli studi sui meccanismi di riparo, le celulle sono capaci di eliminare o di ridurre le lesioni inattivanti: le fasi di questo processo sono comuni a diversi tipi di mutageni. Perciò, la produzione di mutazioni di un certo tipo molecolare è in relazione con il meccanismo di riparo e, si può dire, non mutageno-dipendente, sebbene la quantità degli eventi mutazionali dipenda dal numero di lesioni o alterazioni primarie indotte nel DNA dai mutageni.

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L'A. è grato al prof. F. D'Amato per la lettura critica del manoscritto.

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[1] *Xeroderma pigmentosum* è una malattia umana di natura ereditaria: gli individui contenenti due geni omologhi recessivi (individui omozigoti) presentano un cancro della pelle e disturbi di natura neurologica. Nelle colture « in vitro » di fibroblasti e linfociti e nelle cellule epidermiche « in vivo » si nota un difetto dei meccanismi di riparazione del DNA, in quanto gli individui affetti non sono capaci di eliminare i dimeri pirimidinici prodotti dalla luce ultravioletta, essendo deficienti dell'enzima endonucleasi.

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TRANSLATED BY LUBBOCK - ROME

SPECIFICITY OF CHEMICAL MUTAGENS AT THE DNA LEVEL[1]

In the course of the studies developed for the purpose of identifying both the mechanism of action of the chemical mutagens that is capable of producing stable alterations in the DNA molecule and the nature of these alterations, the findings have been marked by three successive stages, of diverse interest: a first stage, which envisages the existence, between the chemical mutagen and the DNA, of a simple and essentially chemical type of reaction, susceptible of being identified in direct reaction experiments « in vitro »; a second stage, which draws attention to the importance of the conformation of the DNA proper to the organism subjected to mutagenic treatment or to the metabolic stage during which an organism was treated; and a third stage, which is being developed, according to which the consequence of a mutagenic action on the DNA depends on a series of cellular metabolic activities, many of which have not yet been sufficiently explained. Through the findings of these three stages, it may be considered that there now exists the possibility of a clearer definition and characterisation of the type of reaction that intervenes between a mutagen and a living organism, and hence of the possible specificity of action of a mutagenic substance.

The genetic effects of chemical mutagens was demonstrated in 1943 by the English scientists C. AUERBACH and J. M. ROBSON and, independently, also by the German scientist F. OEHLKERS. Since then, there has been an increasing development of the studies on mutagens, together with an increase in the number of new substances capable of this activity.

The employment of mutagens in genetic research has been of great theoretical interest, since it has resulted in a development of knowledge on the structure and formation of genetic material.

The effects of chemical mutagens on the DNA of chromosomes are extremely varied: in some cases they are represented by extensive destruction of the genetic material or by far less drastic alterations such as the replacement of one pair of nucleotides by another. This latter class represents a very interesting type of experimental alteration, since it enables a study to be made of the changes in the genetic information contained in the gene and the consequences relative to the expression of this gene (gene mutation).

Among chemical mutagens, the most important compounds are the alkylating substances, nitrous acid, hydroxylamine, the bases, analogues the nitroso compounds, the acridines, formaldehyde, etc.: for most of these substances, a study has been made of the « in vitro » reactions with DNA, in an attempt to identify the chemical alterations produced, so as to draw conclusions about the possible molecular alterations such as occur « in vivo », in the DNA present in the cells.

By means of these studies an attempt has been made to define the *specificity* of a given mutagenic substance: that is to say, the possibility of inducing a particular type of molecular alteration in the DNA that will give rise to a change in the phenotype of the organism.

The extensive series of experimental studies done on the genetic effects of chemical mutagens has led to the development of mutagenesis theories elaborated by Freese (1959) [7] and later by Brenner *et al.* (1961) [2]. These theories offer interpretations of the possible molecular alterations that take place in the DNA molecule after treatment of a cell with a chemical mutagen.

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[1] **Publication no. 43 of the C.N.R. Laboratorio di Mutagenesi e Differenziamento, Pisa Italy.**

The possible changes of a simpler type, at the level of the DNA molecule, take the form of the replacement of one nucleotide by another (base substitution = BS, transition or transversion)[1] or else an insertion or a deletion of a nucleotide (base addition or deletion = AD): the former type of alteration (BS) produces a missense or a nonsense mutation, according to the particular triplet of nucleotides that is produced in the site where the alteration has occurred (nonsense triplets: UAG, UAA, UGA; missense triplets: the other remaining 61 combinations). At the level of the protein coded by that gene in which the alteration has taken place it will be possible to have the replacement of one amino-acid by another in the peptidic sequence corresponding to the segment of the gene in which the alteration has occurred, or else, in the case of the nonsense mutations, the synthesis of a fragment of protein, the size of which depends on the position of the triplet in the nucleotide sequence of the gene in which the alteration has occurred (Sarabhai *et al.*, 1964) [16][2]. The second type of molecular alteration (AD) produces, with the insertion or deletion of a nucleotide in the genetic sequence, a shifting of the code units, so that, at the level of the corresponding protein, there will be a completely altered amino-acid sequence, starting from the position in which the mutation has occurred[3].

As has been emphasised by Krieg (1963) [9] regarding studies on mutagens, the aim was to obtain two types of experimental evidence, one *mutational* and the other *chemical:*

(1) the *mutational* evidence was essentially based on the analysis of *forward* mutations[4] and *reverse* mutations[4], induced by a particular mutagenic

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[1] A « transition » occurs when, in the DNA molecule, a purine is replaced by a purine, or a pyrimidine by a pyrimidine: e.g. A ⇄ G, T ⇄ C; on the other hand, a « transversion » occurs when, in the DNA molecule, a purine is replaced by a pyrimidine and viceversa: e.g. A ⇄ T, A ⇄ C, G ⇄ T, G ⇄ C.

[2] In 1964, Sarabhai *et al.* [16] identified the number and type of peptides in the coat protein of the T4D phage that are synthesised by the phage «in vivo», when cells of *E.coli* are infected with mutants of the phage containing nonsense mutations in the structural gene of the coat protein: peptide analysis showed that a part of the protein was synthesised, the length of which was greater the further the nonsense mutation (nonsense triplet) was from the origin of the gene. In nonsense mutants for the structural gene of the β-galactoxidase of *E.coli*, Fowler and Zabin (Science *154*, 1027-1029, 1966) found peptide fragments of the protein of a molecular weight that was lower the nearer to the origin was the point where a nonsense triplet had arisen, through the mutation.

[3] The experimental evidence for this mechanism has been supplied by an analysis of the amino-acid sequence in the lysozyme of the T4 bacteriophage (A. Tsugita *et al.*: J. Mol. Biol. (1969) 41, 349-364); the histidinoldehydrogenase of *Salmonella typhimurium* (J. Yourno: J.Mol.Biol. (1970) *48*, 437-442); the tryptophano-synthesase of *Escherichia coli* (W. J. Bramar *et al.*: Proc. N.S.A. (1967) *58*, 1499-1506). In the structural genes of these proteins, there were introduced, at two points along the genetic segment, two alterations, of the « addition » or « deletion » type, of opposite kind, in such a way that, at the two points where the mutation had occurred, the intermediate nucleotide sequence would be altered through the shifting of the triplets: in the protein isolated by these mutants, containing in the structural gene of that protein the addition of a nucleotide at one point and the deletion of a nucleotide at another point in the genetic segment, in the peptide section between the two mutations there was found to be a different amino-acid sequence from that of the wild strain. The following refers to a case analysed in *E.coli* for the tryptophanosynthesase:
wild type
mRNA nucleotide sequence: ACC.UAU.CUG.CUG.UCA.CGC.GCC.GGC...
amino-acylic sequence : Thr. Tyr. Leu. Leu. Ser. Arg. Ala. Gly...
mutant
mRNA nucleotide sequence: ACC.UUC.UGC.UGU.CAC.GGC.GCC.GGC...
amino-acylic sequence : Thr. *Phe. Cys. Cys. His. Gly.* Ala. Gly...

[4] A *forward* mutation indicates any nucleotide modification induced in a wild or dominant gene of an organism: in this case the wild or dominant gene has been mutated into a recessive gene; a *reverse* mutation indicates any nucleotide modification in the recessive gene, such that it is seen to be brought back to the original dominant or wild situation.

substance, for the purpose of establishing whether the mutation model agreed perfectly with that expected on the basis of a certain type of specificity;

(2) the *chemical* evidence derived essentially from studies done with active mutagenic substances on phage particles treated *in vitro* (extracellular) and was referred to the DNA bases with which the substances reacted and to their possible reaction products.

These two types of experimental test were done for only a few chemical mutagens, and the published results are not sufficient to explain the mechanism suggested as a basis of the mutagenic action.

TABLE I

*TYPES OF TRANSITION EXPECTED ON THE BASIS OF THE RESULTS OF CHEMICAL ANALYSES**

Mutagenic substance	*Principal mechanism*	*Transitions expected*
5 - Bromouracil	Incorporation in the DNA	GC → AT AT → GC
2 - Aminopurine	Incorporation in the DNA	GC → AT AT → GC
Hydroxylamine	Reaction with cytosine	GC → AT
Nitrous acid	Deamination of the bases	GC → AT AT → GC
Alkylating agents	Alkylation of guanidine	GC → AT

* from ORGEL (1965) [12].

Table I shows the relations existing between the *chemical evidence* and the predicted directions of the transitions produced by different types of mutagen: practically all the data collected derive from studies done on the *T4* bacteriophage and are based on both *chemical* and *mutational* evidence.

For example, one interpretation regarding the type of specificity of a given substance based on these relations envisages that the alkylating substances mainly give rise to the transition of the base pair GC to the base pair AT, but it is found that this conclusion holds only for the *T4* phage and not for other organisms: TESSMAN *et al.* (1964) [23] have shown that, in the phages *S13* and *ØX*174, the ethyl methanesulfonate (the alkylating substance)[1] is capable of reacting with the four types of nucleotide present in the DNA; for nitrous acid, the results indicate a similar capacity of reaction.

It might be argued that the differences in the responses to the mutagenic substances observed between the *T4* phage and the *S13* phage may be due to the different DNA structure, since *T4* is a phage having a double strand structure, whereas *S13* has a single strand structure: in fact, when both types of phage (*T4* and *S13*) are subjected to treatment with the alkylating substance N-methyl-N'nitro-N-nitroso guanidine (NG), whereas all four types of nucleotide react in *S13*, with *T4* the transition $GC \rightarrow AT$ is almost exclusively seen to occur. Nevertheless, these results should not lead us to conclude that the different mutagenic response depends on the DNA structure in the two types of phage particle, since a mutagenic analysis done with NG on *Salmonella typhimurium* (EISENSTARK *et al.*, 1965) [6] showed, on the basis of *mutational* evidence, the production of both types of transition: most of the *auxotrophs*[1] produced with NG were susceptible to *reverse* mutation if subjected to a second treatment with the same mutagenic substance. According to the authors [23], the different behaviour of the two phages with regard to the NG suggests the possible existence of different molecular conditions at the point of replication of the DNA.

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[1] Ethyl methanesulfonate ($CH_3SO_2OC_2H_5$) is a substance that is intensely reactive for the DNA molecule: it is capable of transferring its ethylic group (C_2H_5) to a molecule of one of the four nucleic bases, in such a way as to alter the complementarity properties of the alkylated base; hence, nucleic base situations in the DNA will readily occur in the cellular replications subsequent to treatment with this substance.

These few examples clearly show how the specificity necessary to produce a particular type of molecular alteration in the DNA depends on the organism subjected to analysis; nevertheless, a specific change in the DNA also depends on the particular type of genetic system used for the analysis of the mutagenic substances.

In an attempt to interpret, on the basis of *mutational* evidence, the results of an analysis of the changes in amino-acids observed in a series of mutants for the A protein of the tryptophanosynthetase[2] of *Escherichia coli*, YANOFSKY (1965) [26] stated the following reservations:

(1) it is not possible to establish whether a given pair of nucleotides reacts with a mutagen in the same way, independently of the chemical nature of the surrounding nucleotides;

(2) in the case of a weak response, this may indicate either an infrequent mutation mechanism (in the case of that mutagen) or a normal mechanism that operates with little efficiency in certain positions of the DNA;

(3) in the case of different possible changes of nucleotides, a strong response in one direction may cover up a weak response in another direction; the latter would therefore be considered non-existent for a given mutagenic substance.

In order to explain the possible mechanisms of action of the different mutagenic substances, on the assumption that all the biological and genetic effects of the chemical mutagens must depend on the reactions between those compounds and the nucleic acids (hence, in agreement with AUERBACH, 1967) [1], it may be considered that a chemical alteration in the DNA molecule is a necessary but not sufficient condition for the production of a mutation detectable in the phenotype), numerous studies have been developed, based on an analysis of the chemical effects of the mutagens on model molecules such as individual nucleotides or polynucleotides.

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[1] *Auxotrophs* are those mutants of any organism that have undergone *forward* mutations in dominant genes responsible for the synthesis of particular basic nutritive substances such as the amino-acids, the nucleic bases and the vitamins. In consequence of these mutations, the mutants can survive only if they are grown in the presence of those particular nutritive substances that they are no longer able to synthesise.

[2] The enzyme tryptophanosynthetase consists of two proteins (A and B), the synthesis of which depends on two distinct genes arranged in the DNA of *Escerichia coli*, one following the other; this enzyme is capable of metabolising « in vivo » the amino-acid L-tryptophane, starting from one molecule of idoglycerophosphate and one of L-serine. For this protein, as regards the sequence of amino-acids, documentation exists for all the possible types of genetic alteration that might be produced in the gene responsible for that protein.

These analyses may provide indirect evidence regarding the possible specific reactions that determine the mutations; for example:

(1) if a poly C is transformed chemically into a poly U, and this molecule, when the reaction has taken place, loses its properties of pairing with a poly G and attaches itself to an poly A, this transformation may be considered as susceptible of giving rise to a mutation (SINGER and FRAENKEL-CONRAT, 1969) [19];[1]

(2) since the pairing properties of each nucleotide depend on the presence of certain particular bonds in the molecule, the reactions that alterate these bonds may be considered potential reactions having a mutagenic value (SINGER and FRAENKEL-CONRAT, 1969) [19].

However, these studies have produced a whole series of information also on the influence of the conformation of the molecules of nucleic acid on their reactivity towards chemical mutagens.

One example is provided by the behaviour of NG: it has been seen that this compound is capable of reacting with guanine when this base appears in the form of a polymer, but not with guanosine, guanylic acid, GpU or CpG (SINGER and FRAENKEL-CONRAT, 1967) [18]; according to these authors, it is possible that guanine in the polymerised form is considerably more susceptible to reactivity with NG. Moreover, it has been shown that this mutagen, like others, behaves differently according to the type of nucleic acid analysed.

Table II shows the values for the RNA mutagenicity of the TMV (*Tobacco Mosaic Virus*) in two different conditions and subjected to reactions with five types of chemical mutagen: for both conditions the mutagenicity was analysed with and without reconstruction of the viral particle (RNA and proteins).

TABLE II

*RELATIVE MUTAGENICITY OF CHEMICAL MUTAGENS IN TMV-RNA COMPARISONS IN DIFFERENT CONDITIONS**

Mutagen	*Mutagenicity*	
	RNA in water	*RNA in virus*
Nitrous acid	34	110
Hydroxylamine	11	1.3
Methyl methanesulfonate	5	4
Ethyl methanesulfonate	4.5	8
Nitrosoguanidine	2	60

* from SINGER and FRAENKEL-CONRAT (1969) [19].

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[1] Experimentally, it is possible to synthesise a molecule of ribonucleic acid containing only the nucleic base cytosine repeated a very great number of times (poly C): this molecule may develop hydrogen bonds when it is introduced into the presence of another synthetic molecule containing only guanine (poly G), such that it constitutes a double-stranded molecule of ribonucleic acid. If the poly C molecule is treated with nitrous acid and then subjected to the phase of constitution of a double-stranded molecule, it is seen that the poly C treated with nitrous acid develops hydrogen bonds with an poly A molecule, since nitrous acid is capable of deaminating cytosine, transforming it into uracil, so that the poly C molecule has been transformed into poly U, which now develops hydrogen bonds with poly A.

However, these results may also depend on the different chemical reactivity of the individual nucleic bases: for example, it is known that nitrous acid is capable of deaminating the bases and that its mutagenicity depends on the relative relationship between mutational events and inactivating events. In fact, when RNA in the viral particle is treated with nitrous acid, the guanine is not deaminated, whereas after the treatment of free RNA 28% of the guanine is found to be deaminated: in the 1st case the relative mutagenicity is 110, in the second case it is only 34 (Table II).

The influence of the conformation of the DNA (single strand or double strand) on the reactivity to a given chemical mutagen has been analysed by TESSMAN (1968) [22] by treating with hydroxylamine two phages with DNA of different conformations, *T4* (double strand) and *S13* (single strand). For the *T4* the inactivation rate was 5.4×10^{-3}/min./M, while for *S13* the value was 3.1×10^{-1}/min./M: the greater reactivity of the S13, though it has a quantity of DNA that is about 80 times less than that of the *T4*, depends exclusively on its single-helix conformation. In fact, *S13* in the isolated or incapsulated form in the phage particle shows the same reactivity and both forms were more reactive than the replicative form of *S13*, which has a double-helix conformation.

The importance of the molecular conformation of the DNA in relation to the mutagenic specificity is also shown by studies done with nitrosoguanidine (NG) on *Escherichia coli* by CERDÀ-OLMEDO *et al.* (1968) [3], which have demonstrated that, when synchronised cells are concerned, the mutations occur almost exclusively in those genes in which the DNA is in a condition of replication. Since the treatment was performed in the absence of DNA synthesis, the authors exclude the possibility that the mutagen may act on the replication complex (the DNA polymerase) and assume that the region in replication, since it contains a DNA having a conformation different from the classical structure shown by WATSON and CRICK, must be particularly susceptible to the NG by very reason of its molecular conformation.

The mutagen specificity, assessed on the basis of the mutation of particular genetic sites, has been shown to depend also on the genetic nature of the system analysed: in the *T4* phage, SPEYER [20] has described certain types of alteration that he has obtained in the structural gene of the DNA polymerase (gene 43) and has reported data showing how the response to a given mutagen depends on the presence of those genetic alterations in that particular gene.

DRAKE and GREENING (1970) [5] have shown that, in fact, in the case of nitrous acid and BUdR (5-bromodeoxurydine), the mutagen specificity depends on the way in which the primary lesions produced by the mutagen on the DNA molecule are interpreted by the enzyme apparatus given preference in the DNA replication.

Moreover, it is well known that many organisms maintain the integrity of their own genetic material by means of a series of enzymatic repair mechanisms capable of reducing or eliminating the harmful effects of the alterations produced in the DNA: the best-known enzymatic repair mechanism is that present in bacteria, which reduces the harmful effects of irradiation with ultraviolet light (UV). In cells subjected to a treatment with UV, dimers of the cyclobutane type are formed between the pyrimidine residues present in the DNA molecule: today, there exist numerous data that indicate how the presence of these dimers also causes a distortion in the double-helix structure of the DNA. However, bacteria are endowed with three particular types of repair, specifically capable of eliminating this primary alteration produced by UV:

(1) photo-reactivation;

(2) the elimination of the DNA segment containing the dimer and the subsequent DNA synthesis;

(3) the reconstitution of the integrity of the DNA by means of recombination processes. The first two mechanisms eliminate the pyrimidine dimers from the DNA; the third is capable of repairing secondary lesions of the DNA, which have formed in the first DNA replication cycle after the treatment with UV light and are represented by interruptions in the polynucleotide chain: in this case there is a processes of random reconstruction of whole polynucleotide molecules actuated by recombination phenomena (Howard-Flanders *et al.*, 1968) [8].

Of these processes, the first reduces the mutation frequency, since it eliminates alterations (dimers of Thymine) that are mutations in a potential state; the 2nd and 3rd produce mutations, either directly, by introducing stable molecular alterations into the DNA, or indirectly, by giving rise to secondary alterations or discontinuities in the DNA strand, which represent potential mutational lesions. In general, it may be said that the studies on these processes have shown that mutations (at least in the case of ultra-violet light) depend not only on the number of primary alterations induced (e.g. the number of pyrimidine dimers) but also on secondary alterations and the occurrence of cellular events subsequent to the mutagen treatment: these events are mainly represented by metabolic repair processes that the cell sets off in order to reduce the damage arising from the alterations produced by the mutagen treatment.

The existence of repair mechanisms in bacteria was shown as a result of the isolation of mutants that were more sensitive than the wild strain to the inactivating action of UV light, since they were unable to eliminate the pyrimidine dimers from their DNA: it was also shown chemically that in *UV-sensitive* strains there is no release of pyrimidine dimers in the cytoplasm and that the persistence of these products in the DNA were the cause of the increase in lethality observed after irradiation of these sensitive strains with UV.

A similar repair mechanism, though specific for the primary lesions produced in the DNA by a chemical mutagen, has been identified in *Bacillus subtilis* as a result of studies done by Reiter and Strauss (1965) on the alkylating compound methyl methanesulfonate (MMS) [14]. This substance produces breakages in the DNA strand as a result of the methylation of the guanosine residues and the subsequent hydrolysis of these compounds, thus giving rise to the formation of apurinic sites along the DNA chain: after treatment with MMS, a supplementary DNA synthesis was observed in the cells of *B.subtilis,* together with a consequent reduction in the number of lesions produced by the alkylation.

Strauss *et al.* (1966) [21] have isolated a mutant of *B.subtilis* sensitive to to the lethal action of MMS; later studies have shown, moreover, that the repair mechanisms specific for MMS and for UV light are not the same, since the *UV-sensitive* mutants are not *MMS-sensitive,* and viceversa.

Certain studies have attempted to explain the repair mechanism specific for alkylating substances: the presence of apurinic sites and of breaks of a single DNA strand can be shown by subjecting the DNA extracted from cells treated with MMS to sedimentation analysis in alkaline saccharose, since there occurs the break of the apurinic sites and the denaturation of the DNA corresponding to the breaks of single strands: in general, after treatment with 25nM of MMS (20 minutes), the DNA of *B.subtilis* shows a variation in the sedimentation rate in alkaline saccharose: according to Reiter *et al.* (1967) [15], these observations indicate that the breaks in a single DNA strand caused by MMS are subject to repair.

Prakash and Strauss (1970) [13] have shown that, by incubating the cells treated with MMS for 40 minutes before the extraction of the DNA and the sedimentation analysis, the sedimentation rate is observed to increase towards values close to that of the non-treated control. On the other hand, a strain of *B.subtilis* sensitive to MMS did not reveal any increase in sedimentation rate, so that it must be assumed that this strain is not capable of repairing breaks of a single strand, produced by the alkylating action of the MMS. Lawley and Brookes (1968) [10] have shown that, when cells of *Escherichia coli* are subjected to treatment with nitrogen mustard, nucleotide material is seen to be released from the DNA to the extent of 400 nucleotides for each individual alkylation produced in the DNA, depending upon the time of treatment; in the sensitive strain, on the other hand, this phenomenon is not observed[1].

It has not yet been ascertained whether the mutations depend on the repair mechanisms operating in the cells of micro-organisms treated with chemical mutagens; however, the strains sensitive to these substances (and hence probably incapable of those repair processes) reveal a much higher mutation frequency than that observed in the experiments with resistant strains.

Loprieno and Schupbach (1971) [11] have studied the effect of caffeine on the mutations induced by UV light and by nitrosoguanidine (NG) in the cells of the yeast *Schizosaccharomyces pombe:* it has long been known that

Table III

FREQUENCY OF FORWARD MUTATIONS INDUCED IN UV-SENSITIVE STRAINS OF S.pombe *BY UV LIGHT* (6.240 *ergs/sq.mm.*) *AND NG* (10^{-3}*M*, *5 min., pH* 6.5).

Strain	*Mutagen*	*Survival* %	*Mutation Frequency* ($\times 10^{-4}$)
wt-972*	UV	81.9	1.32
»	NG	71.6	2.02
uvs5-158	UV	0.3	45.00
»	NG	0.2	287.80
uvs10-198	UV	0.3	9.18
»	NG	0.2	203.74

* wild type strain

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[1] In general, when alterations occur at the DNA level through the production of pyrimidine dimers or breaks in the strand or alkylation, the subsequent repair processes comprise a phase of hydrolysis of the nucleotide segment involved in the alteration, with the consequent release of oligonucleotide fragments of different lengths in the cytoplasm.

caffeine interferes with the repair processes in bacteria treated with UV (WITKIN, 1958) [24], by inhibiting the removal of the DNA segment containing pyrimidine dimers (SETLOW, 1967) [17]: in *S.pombe* it has been shown that caffeine reduces the premutational alterations induced by UV and the percentage of meiotic recombinational events, thus showing that also in eucaryotic organisms the recombinational events are responsible for the repair mechanisms set off after treatment with UV (HOWARD-FLANDERS *et al.*, 1968) [8], which agrees with the data obtained by WITKIN and FARQUHARSON (1969) [25] in *E.coli.* After treatment with nitrosoguanidine, a diminution of about 5 times is observed in the premutational events, in the presence of caffeine; this indicates that the repair mechanisms specific for nitrosoguanidine are partly the same as those specific for the lesions produced by UV. Mutants have been isolated in this yeast that are more sensitive to the inactivating action both of the UV and of the nitrosoguanidine: in some of these mutants the mutation frequency in the *ade6* and *ade7* genes (responsible for the biosynthesis of adenine) increases by about 100 times after treatment with nitrosoguanidine (Table III).

These repair processes are common to all organisms: in man, for example, the somatic cells are capable of eliminating the damage produced in the DNA by the UV light through the elimination of the nucleotide segment containing the pyrimidine dimer; the somatic cells of individuals affected with *Xeroderma pigmentosum* are not capable of this function, whereas they are able to repair the damage produced by methyl methanesulfonate and nitrosoguanidine[1] (CLEAVER, 1970) [4].

In conclusion, it may be stated that, in the light of the evidence provided by studies on repair mechanisms, cells are capable of eliminating or reducing inactivating lesions: the stages in this process are common to different types of mutagen. Therefore the production of mutations of a certain molecular type is related to the repair mechanism and, it may be said, is not mutagen-dependent, although the amount of the mutational events depends on the number of primary alterations or lesions induced in the DNA by the mutagens.

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The author is grateful to Prof. F. D'Amato for his critical reading of the manuscript.

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[1] *Xeroderma pigmentosum* is a human disease of a hereditary nature: individuals possessing two recessive homologous genes (homozygotic individuals) reveal a skin cancer and disorders of a neurological nature. In « in vitro » cultures of fibroblasts and lymphocytes and in the epidermic cells « in vivo », a defect may be noted in the DNA repair mechanisms, since the individuals affected are not capable of eliminating the pyrimidine dimers produced by UV light, perhaps because they are deficient in the enzyme endonuclease.

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EFFECTS OF CHEMICAL STIMULATION AT THE [illegible] LEVEL III

[illegible]

OGGETTO E FONDAMENTI DELLA MATEMATICA

Nel presente articolo, dopo aver accennato alla difficoltà di dare un'unica definizione della matematica, essendo questa una scienza in continua evoluzione caratterizzata dalla tendenza ad una sempre maggiore astrazione, specialmente a partire dal secolo scorso, si sviluppa con vari esempi la definizione di matematica come « scienza delle strutture » e si fornisce in proposito il concetto di qualcuna delle strutture matematiche fondamentali e quello più recente di « categoria ». Passando, quindi, all'esame del problema dei fondamenti della matematica si illustrano le posizioni platoniste e logiciste accennando a quella di Cantor, di Frege, Russell, Wittgenstein e dell'empirismo logico. Si descrive poi il significato del programma formalista di David Hilbert e l'importanza del teorema di incompletezza di Gödel con il conseguente fallimento del formalismo integrale. Accennato ai tentativi dei formalisti post-hilbertiani si passa a presentare un quadro concettuale della corrente intuizionista facente capo a Brouwer e Heyting, sottolineando le sostanziali differenze rispetto alla scuola logicista e formalista e l'originalità della impostazione di alcuni fondamentali concetti matematici. Brevi indicazioni bibliografiche indicano al lettore italiano non specialista i mezzi indispensabili per entrare nel vivo delle questioni presentate.

1. - Che cos'è la matematica. — Una vera consapevolezza del significato e del contenuto della matematica presuppone una chiara conoscenza dell'evoluzione storica di essa[1]. L'orientamento moderno della ricerca matematica è infatti il risultato del profondo travaglio spirituale di tante generazioni di matematici e soprattutto di quelle che hanno operato a partire dal secolo scorso. In ogni caso, la matematica è attività umana che riflette la volontà operativa, « la ragione contemplativa e il desiderio di perfezione estetica. I suoi elementi fondamentali sono la logica e l'intuizione, la generalità e l'individualità. Tradizioni diverse potranno mettere in evidenza aspetti diversi, ma è soltanto la reazione di queste forze antitetiche e la lotta per la loro sintesi che costituiscono la vita, l'utilità e il valore supremo della matematica[2] ».

Per una disciplina come la matematica le cui origini si perdono nella notte dei tempi e che almeno da venticinque secoli si presenta organizzata come scienza, non è facile individuare la direzione costante del suo sviluppo e della sua evoluzione. Eppure una tendenza costante senza dubbio possiamo rilevarla nello sforzo per una sempre maggiore generalizzazione. Tale impegno per astrarre entità matematiche dal concreto, certamente presente presso i primi interessati geometri egiziani, è già diventato cosciente ed organico presso i matematici greci mentre è una delle caratteristiche dominanti della ricerca matematica moderna.

Oggi non possiamo più considerare la matematica come scienza delle quantità e neppure ci basta definirla scienza delle relazioni[1], ma dobbiamo pensare che la matematica sia questo e altro ancora. Come scienza delle relazioni l'ha vista Frege e la scuola logicista, ma Hilbert e i formalisti la considerano come scienza del possibile, mentre gli intuizionisti come Brouwer e Heyting la ritengono la scienza delle « costruzioni » possibili. E forse con ciò non abbiamo ancora esaurito l'oggetto della matematica contemporanea.

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[1] Ettore Carruccio: *Matematica e logica nella storia del pensiero contemporaneo*, Gheroni, Torino, 1958, p. 7.

[2] R. Courant e H. Robbins: *Che cos'è la matematica?*, Einaudi, Torino, 1950, p. 25.

Saremmo tentati di concludere con Bertrand Russell che « la matematica pura è quella scienza in cui non sappiamo di che cosa stiamo parlando o se ciò che stiamo dicendo è vero »[2], ma ci incoraggia la sicurezza con cui Émil Borel afferma che « la matematica è l'unica scienza in cui si conosce sempre esattamente ciò di cui si parla e nella quale si è certi che ciò che si dice è vero »[3]!

2. - LA MATEMATICA COME SCIENZA DELLE STRUTTURE. — Ma quali sono i nuovi connotati della ricerca matematica moderna? La matematica non si sviluppa come una antica costruzione dalle basi molto solide sulla quale le successive generazioni costruiscono piani sempre più elevati, perchè ogni epoca preferisce ricostruire l'edificio secondo un progetto nuovo e sopra fondamenta nuove[4].

La nostra epoca è particolarmente ricca di nuovi oggetti e di nuove architetture che tendono a portare fino alle estreme conseguenze la potenza di generalizzazione e di astrazione della matematica.

Gli oggetti studiati dalla matematica sono sempre entità astratte, anche i numeri naturali o una retta o un triangolo lo sono. Ma, gli oggetti della matematica moderna sono ancora più astratti di questi.

Un primo semplice esempio, illustrativo della tendenza alla generalizzazione che caratterizza il senso della matematica moderna, può essere fornito dalla considerazione dell'evoluzione del concetto tradizionale di funzione, da cui si è passati a quello di funzionale che generalizza quello di funzione e successivamente, nell'Analisi generale, a quello di trasformazione ancor più ampio del precedente[5].

Un altro esempio si ha considerando l'evoluzione del concetto di spazio.

Da quello intuitivo di spazio fisico, che ha il suo modello nella geometria euclidea, si è giunti a quello di spazio astratto, che è un insieme di elementi di natura qualsiasi (numeri, applicazioni, polinomi, ecc.) dotato di struttura basata sulla nozione di intorno (spazi topologici), di distanza (spazi metrici di Hausdorf o spazi *distanciés* di Bouligaud-Fréchet), di norma (spazi normati o di Banach), ecc.

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[1] cfr GIOVANNI VAILATI: *La più recente definizione della matematica.* In Scritti di Vailati; Leipzig-Firenze, 1911, pp. 528-533.

[2] Citato in Nagel e Newman: *La prova di Gödel* (traduzione di Luigi Bianchi), Boringhieri, Torino, 1961, p. 20.

[3] Nell'opera in collaborazione a cura di F. LE LIONNAIS: *Les grands courants de la pensée mathématique*, Cahiers du Sud, Marseille, 1948, p. 24.

[4] A. LENTIN et J. RIVAUD: *Leçons d'Algèbre moderne*, Vuibert, Paris, 1964, p. V.

[5] Cfr M. FIORENTINI: *Dall'analisi matematica classica all'analisi funzionale e all'analisi generale*, in Cultura e Scuola, 12(1964), pp. 214 ss.

Interviene qui il concetto di struttura che ha un ruolo decisivo nella determinazione dell'oggetto della matematica moderna. La ricerca di maggiore generalità e di nuove strutture sempre più generali è, attualmente, uno degli stimoli più potenti dell'indagine matematica.

Oggi, quando si vuol dare una definizione della matematica si parla spesso di essa come scienza delle strutture, in esse, infatti, i matematici ritrovano l'unità di quegli argomenti che la matematica tradizionale considerava come compartimenti distaccati.

La terza delle proposte finali, a proposito dell'insegnamento della matematica, del Colloquio internazionale sulla riforma e il coordinamento degli insegnamenti di matematica e fisica, tenuto a Losanna nel gennaio 1967 dice: « La matematica si evolve sempre più verso una scienza generale delle strutture. Queste le conferiscono una potenza considerevole di applicazione, di informazione e di unificazione. La conoscenza e la padronanza di tali strutture, il loro impiego nella conquista della realtà sono i veri fini dell'insegnamento matematico[1] ».

Pertanto, studiare le strutture astratte non significa allontanarsi dalle cose concrete, nè « in alcun modo fuga in elucubrazioni di scarsa o nulla utilità pratica », anzi così « si è conquistato un mezzo straordinariamente potente, atto a risolvere, con un solo ragionamento formale, parecchi problemi concreti a prima vista diversissimi (potenzialmente infiniti problemi concreti diversi, se infiniti sono i 'modelli' concreti con la medesima struttura formale). Quanto più astratta è la matematica, tanto più essa è semplice e utile[2] ».

3. - Le strutture matematiche. — La nozione di struttura si estende al di là della accezione che a noi qui interessa. Si parla, infatti, anche di strutture logiche, fisiche, biologiche, linguistiche, psicologiche, sociali, ecc., sebbene non sia sempre agevole individuarne i confini.

Per struttura, nel senso più generale, Jean Piaget intende « un sistema di trasformazioni, che comporta delle leggi in quanto sistema (in opposizione alle proprietà degli elementi) e che si conserva o si arricchisce grazie al gioco stesso delle sue trasformazioni, senza che queste conducano fuori delle sue frontiere o facciano appello a elementi esterni. In breve una struttura comprende così questi tre caratteri: totalità, trasformazioni e autoregolazione[3] ».

Il primo carattere essenziale di una struttura è, dunque, quello di essere una totalità; cioè, gli elementi che la costituiscono sono dipendenti dal tutto, non possono essere scelti senza tener conto di tutto il sistema. L'altro elemento caratterizzante una struttura è l'insieme delle trasformazioni che avvengono nel sistema; forse, per poter includere alcune strutture matematiche, potremmo più genericamente parlare di insieme delle relazioni. Infine alla struttura è essenziale che essa sia capace di autoregolazione, cioè congegnata in modo che le leggi che la regolano generino sempre elementi appartenenti alla struttura.

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[1] *Actes du Colloque international sur la réforme et la coordination des enseignements de mathématique et de physique*, in Dialectica, 21(1967), p. 258.

[2] Lucio Lombardo Radice: *Istituzioni di algebra astratta*, Feltrinelli, Milano, IV edizione, 1967, pp. 4-5.

[3] Jean Piaget: *Lo strutturalismo* (traduzione di Andrea Bonomi), Mondadori, Milano, 1968, p. 39.

Una presentazione sistematica della matematica come scienza delle strutture è quella che a partire dal 1939 viene sviluppata negli *Eléments de Mathématiques*, la possente opera collettiva pubblicata sotto lo pseudonimo di *Nicolas Bourbaki*[1] da un gruppo di matematici francesi che continuamente si rinnova. Ai bourbakisti si deve non solo l'accurata precisazione del concetto di struttura matematica, ma anche una nuova visione della architettura della matematica[2].

Le più importanti strutture matematiche possono ricondursi a poche strutture semplici, dette strutture madri, catalogabili, secondo Bourbaki, in tre tipi: strutture algebriche, strutture d'ordine, strutture topologiche. Sovrapponendo e collegando queste si possono ottenere le strutture multiple[3].

In tutti i tipi di strutture matematiche la totalità è costituita da opportuni insiemi astratti (cioè di enti di natura non definita).

Nelle strutture algebriche le trasformazioni sono operazioni (dette pure leggi di composizione), anch'esse di natura non definita.

Nelle strutture d'ordine gli elementi dell'insieme sostegno sono caratterizzati da proprietà relazionali che danno luogo a un ordinamento; questo può considerarsi una generalizzazione di quello che caratterizza la successione dei numeri naturali.

Nelle strutture topologiche le relazioni tra gli elementi dell'insieme sostegno riguardano i concetti di vicinanza, intorno, limite, frontiera, continuità, ecc.

Ma, anche i tre tipi di strutture fondamentali sviluppate dai bourbakisti sono stati unificati nella cosiddetta *teoria generale delle strutture*. Essa tratta dei *modelli di relazioni*, che sono coppie ordinate costituite da un insieme e dalle strutture di relazioni sull'insieme stesso ed eventualmente sull'insieme delle parti. Col concetto di relazione, infatti, è possibile abbracciare sia quello di operazione algebrica, sia quello di ordine, sia quello di topologia.

Mac Lane, Eilenberg, Cartan, Buchsbaum, ecc. in tempi ancor più recenti, hanno presentato un altro aspetto generale della teoria delle strutture, mediante lo studio delle *categorie*, che sono coppie ordinate costituite da un insieme e da tutte le applicazioni (funzioni) che esso comporta. Con lo studio delle categorie l'attenzione si sposta dalle strutture alle operazioni che le hanno generate. Si intuisce facilmente la grande potenza di tali considerazioni.

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[1] Dal nome di un generale francese che, durante la guerra franco-prussiana del 1870, in contrasto con la tradizione militare francese, preferì far sconfinare in Svizzera le sue truppe, piuttosto che impegnarle in una impari battaglia con i prussiani.

[2] Cfr N. BOURBAKI: *L'architecture des mathématiques*, in F. Le Lionnais, op. cit. pp. 35-47.

[3] Cfr AA.VV.: *Strutture algebriche e strutture topologiche*, Feltrinelli, Milano, 1963.

4. - LE STRUTTURE DELL'INSIEME R DEI NUMERI REALI. — Un semplice esempio in cui sono operanti varie strutture matematiche è fornito dall'insieme dei numeri reali. Esso ha una struttura multipla in cui è facile isolare le varie strutture semplici.

Strutture algebriche in R: L'insieme dei numeri reali ha la struttura di gruppo[1] rispetto all'addizione. Infatti è chiuso rispetto ad essa (cioè la somma di due numeri reali è ancora un numero reale), è dotato di elemento neutro (lo zero), inoltre ogni numero reale è dotato di simmetrico (il suo opposto), infine l'addizione gode della proprietà associativa. Siccome, poi, l'addizione gode anche della proprietà commutativa, il gruppo additivo dei numeri reali è abeliano (commutativo).

Ma, l'insieme R, privato dello zero (perchè lo zero non è dotato di reciproco), ha una struttura di gruppo anche rispetto alla moltiplicazione, come si verifica agevolmente; inoltre la moltiplicazione gode della proprietà distributiva rispetto alla addizione; possiamo allora dire che l'insieme dei numeri reali ha una struttura di anello[2].

Strutture d'ordine in R: Si tratta dell'ordinamento naturale totale, per cui due numeri reali qualsiasi sono legati dalla relazione minore o uguale, la quale gode delle tre proprietà riflessiva, antisimmetrica e transitiva.

Strutture topologiche in R: Se ci limitassimo a considerare soltanto le strutture algebriche e quelle di ordine potremmo concludere che l'insieme dei numeri reali, finora considerato, non si differenzia troppo dall'insieme Q dei numeri razionali, perchè questo, come è facile verificare, ha le stesse strutture già considerate nel primo. Dunque, le sole strutture algebriche e d'ordine non bastano a caratterizzare completamente l'insieme dei numeri reali. Ciò avviene mediante considerazioni di carattere topologico. Si vede subito infatti che l'insieme R è continuo, mentre l'insieme Q non lo è[3].

Altre proprietà strutturali possono ricavarsi dalla sovrapposizione, per esempio, delle strutture algebriche e di ordine (e queste saranno, ovviamente, comuni a R e a Q). Così, dati tre elementi a, b e c appar-

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[1] Si chiama *gruppo* ogni insieme G di elementi che soddisfa ai seguenti assiomi:

1) Esiste una *legge di composizione* « T » che ad ogni coppia (a,b) di elementi di G fa corrispondere un elemento c di G (chiusura di G rispetto a T);

2) tale legge di composizione è associativa, cioè: aT (bTc) = (aTb) Tc;

3) esiste un elemento neutro « e » tale che, per ogni a, aTe = eTa = a;

4) ogni elemento a ammette un elemento inverso a^{-1} tale che:

$$aTa^{-1} = a^{-1}Ta = e.$$

[2] Si chiama *anello* ogni insieme A di elementi, che ha una struttura di gruppo rispetto ad una legge di composizione « T », e inoltre è chiuso e associativo rispetto ad una seconda legge di composizione « ⊥ ».

[3] Cioè se la coppia di insiemi (A,B) costituisce una « sezione » di R si verifica che o A ha in R il suo massimo o B il suo minimo. Invece è possibile trovare in Q delle sezioni (A,B) tali che A sia privo di massimo in Q e B di minimo, il che equivale a dire che in Q vi sono dei « salti ». Ciò significa anche che i numeri reali possono mettersi in corrispondenza biunivoca con i punti di una retta orientata r (continuo lineare), mentre invece ogni corrispondenza tra Q e la retta r è soltanto univoca. Infatti mentre Q ha la potenza del numerabile (cioè può porsi in corrispondenza biunivoca con l'insieme dei numeri naturali N), R ha la potenza del continuo.

tenenti a R (o anche solo a Q), tali che siano $a<b$ e $c>0$, vale certamente la nuova relazione $ac<bc$.

Abbiamo quindi mostrato un esempio di struttura multipla ottenuta collegando e sovrapponendo strutture semplici.

5. - La nozione algebrica di gruppo e il Programma di Erlangen.

— Alcune considerazioni sulla struttura di gruppo potranno darci un'idea della importanza dello studio delle strutture algebriche per gli altri rami della matematica e anche al di fuori di questa.

Il gruppo è la prima struttura che sia stata studiata. Fu introdotta da Évariste Galois, nel 1832, per dimostrare che una generica equazione di grado superiore al quarto non può essere risolta mediante radicali[1].

La straordinaria fecondità e utilità costruttiva della nozione di gruppo traspare, ad esempio, nella concezione nuova della geometria presentata da Felix Klein, nel 1872, nel celebre programma di Erlangen, che presenta la *geometria* come *lo studio delle proprietà invarianti rispetto a un determinato gruppo di trasformazioni.*

Un primo elementarissimo studio della geometria può farsi sottoponendo le figure geometriche soltanto a quelle trasformazioni che lasciano invariate tutte le distanze (e quindi gli angoli, le parallele, le rette, ecc.). Si prende cioè, in considerazione soltanto il gruppo delle *congruenze* (movimenti e simmetrie), come si fa ad esempio nello studio delle uguaglianze.

Ma il gruppo delle congruenze può essere considerato come sottogruppo del più ampio gruppo delle *similitudini*, che - lasciando invariati gli angoli (e quindi le parallele, le rette e la forma delle figure) - permette di variare le dimensioni. La geometria elementare studia solo queste trasformazioni, essa, quindi, può definirsi come lo studio delle proprietà delle figure invarianti rispetto al gruppo delle similitudini.

Anche il gruppo delle similitudini può, a sua volta, considerarsi come sottogruppo di un gruppo più ampio, quello delle *affinità*, che permette di modificare anche gli angoli (e quindi la forma), e le distanze ma non le parallele, nè le rette. Nella geometria affine, per esempio, un quadrato può essere trasformato in un qualunque parallelogrammo.

Ma il gruppo delle affinità può essere considerato come sottogruppo del gruppo proiettivo, le cui trasformazioni sono le *proiettività* (proiezioni e sezioni). Esse possono trasformare rette parallele in rette convergenti (si pensi alle prospettive) ma non modificano le rette.

La geometria proiettiva, quindi, può definirsi come lo studio delle proprietà invarianti rispetto al gruppo delle proiettività (o omografie).

Se poi, ammettiamo anche le trasformazioni delle rette in coniche o in curve algebriche di grado superiore al secondo, otteniamo il gruppo delle *trasformazioni birazionali*, di cui quello delle proiettività è sottogruppo. La geometria algebrica è, quindi, lo studio delle proprietà invarianti rispetto al gruppo delle trasformazioni birazionali.

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[1] Cfr *Oeuvres mathématiques d'Evariste Galois*, par G. Verriest, Gauthier-Villars, Paris, 12.e édition, 1951, pp. 33 ss.

Infine, se ampliamo ancora il gruppo delle trasformazioni, considerando le nostre figure come se fossero costituite da materiale elastico, ma non lacerabile, nè duplicabile, otterremo il gruppo degli *omeomorfismi*, studiato dalla topologia, dove si conservano in ogni caso le proprietà di biunivocità e di continuità. La topologia è, dunque, lo studio delle proprietà invarianti rispetto al gruppo delle trasformazioni biunivoche e continue, come avviene, per esempio, nelle carte geografiche o nelle immagini di un oggetto ottenute mediante opportune lenti deformanti.

In tal modo, mediante la nozione di gruppo, le varie geometrie non costituiscono più teorie distaccate, ma sono capitoli incapsulati l'uno nell'altro di un'unica teoria generale.

Quanto alla possibilità di applicare la teoria dei gruppi alle scienze sperimentali, basti pensare che mediante lo studio del gruppo delle simmetrie si è potuto prevedere l'esistenza di particelle elementari poco dopo scoperte sperimentalmente dai fisici.

Possiamo trovare la struttura di gruppo anche nella logica, sia attraverso considerazioni sull'algebra di Boole, sia direttamente nella logica degli enunciati[1].

6. - L'INSEGNAMENTO DELLA MATEMATICA MODERNA.

— Emerge da quanto abbiamo detto l'utilità, per approfondire gli altri rami della matematica, dall'algebra astratta e della topologia.

Ormai non si può più fare della matematica senz'algebra[2], al punto che nella nostra epoca l'algebra più che essere un ramo della matematica « occupa, nell'ambito di questa, il posto che per lungo tempo la matematica ha ricoperto rispetto alla fisica[3] ».

È anche chiaro che algebra e topologia presuppongono la teoria degli insiemi e che a tutto ciò è assai opportuno premettere la logica matematica che fornisce gli elementi indispensabili per una rigorosa trattazione formale.

L'apprendimento matematico ha così un compito essenzialmente formativo ed esercita intrinsecamente una notevole potenza di attrazione sulla mente di studenti anche giovanissimi.

Parlando di questo modo di insegnare la matematica, Aldo Masullo osserva che « lo stimolo all'apprendimento va cercato nelle proprietà stesse degli oggetti logici » e sottolineando l'antipragmatismo di tale metodo dice che così « il valore autonomo dell'apprendere viene sostituito al suo valore strumentale » in quanto « il principio ispiratore della didattica non sta più nel mobilitare progressivamente le funzioni psicologiche attraverso la loro sollecitazione pragmatica per rendere così finalmente possibile l'acquisizione di strutture operative logiche, ma nella nuda presentazione della struttura logica quale modello dell'operazione psicologica. [...] L'apprendimento, allora, non ha più il suo valore nella sua utilità, i suoi processi non sono più strumentali rispetto ad una finalità che li trascende, non ha più dunque un senso pragmatico, ma ha un valore autonomo, è dotato di un senso estetico e ludico, dal momento che i suoi processi, coincidendo con la strutturazione stessa dei significati, non obbediscono ad interessi estrinseci, sono 'disinteressati', non vivono che per il piacere di vivere, come appunto è sempre avvenuto nell'arte e nel gioco[1] ».

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[1] Cfr JEAN PIAGET; *op. cit.*, pp. 63-4.

[2] P. DUBREIL et M. L. DUBREIL-JACOTIN: *Leçons d'algèbre moderne*, Dunod, Paris, 1961, Avant-propos.

[3] CLAUDE CHEVALLEY: *Concetti fondamentali di Algebra*, Feltrinelli, Milano, 1964.

Una moderna visione della matematica, osserva Ferdinand Gonseth, concludendo il già citato Colloquio di Losanna, apre anche la strada ad una nuova metodologia (il metodo assiomatico, la formalizzazione, ecc.) da cui ci possiamo attendere una nuova soluzione del problema dei fondamenti della matematica[2].

Per chiarire l'oggetto della matematica bisogna anche domandarsi qual'è il fondamento su cui essa si poggia.

7. - LE RICERCHE SUI FONDAMENTI DELLA MATEMATICA. — La visione tradizionale di una matematica i cui teoremi sono logicamente dedotti da alcuni *assiomi intuitivamente evidenti*, che per lunghi secoli aveva appagati filosofi e matematici e aveva perfino reso possibile l'opinione comune che, proprio in forza di tale concezione, la matematica è la scienza esatta per antonomasia, non può più essere considerata soddisfacente.

Riflettendo sul senso dell'attività matematica si può, in prima approssimazione, assumere uno di questi due contrapposti atteggiamenti.

Accettare una prospettiva *contenutista* della matematica intendendola come scienza che studia particolari oggetti, che pre-esistono ad essa; oppure una prospettiva *formalista* intendendo la matematica come un discorso intorno a certi enti che sono di volta in volta da essa stessa realizzati[3]. Il primo atteggiamento può dirsi *platonista* il secondo *costruttivista*.

Nel contesto della visione platonista si articola il tentativo di ridurre la matematica alla logica, il cosiddetto *logicismo*.

Trascurando, per brevità, le numerose posizioni intermedie ci proponiamo qui di presentare alcuni cenni sulla prospettiva logicista e, tra quelle costruttiviste, del *formalismo estremo* e dell'*intuizionismo*.

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[1] ALDO MASULLO: *L'apprendimento matematico e gli sviluppi delle scienze psicologiche e sociali*, in Didattica della Matematica.

[2] Dialectica, 21(1967) p. 270.

[3] Cfr ETTORE CASARI: *Questioni di filosofia della matematica*, Feltrinelli, Milano, 1964, pp. 11-2 e passim; CARLO CELLUCCI: *La filosofia della matematica*, Laterza, Bari, 1967, pp. 15-6 e passim.

8. - Platonismo e logicismo. — Secondo la concezione platonista, come abbiamo già detto, la matematica è una scienza che scopre e isola le entità matematiche che ritrova al di fuori di sè. Il suo centro di interesse è, pertanto, negli oggetti esterni. Essa quindi si propone innanzitutto di precisare gli assiomi che individuano le caratteristiche fondamentali degli oggetti che sono alla base della matematica, e, cioè, in ultima analisi, degli insiemi.

Rientrano qui sia la grandiosa teoria intuitiva degli insiemi dovuta soprattutto a Georg Cantor, sia le teorie assiomatiche degli insiemi ideate per superare le antinomie[1] scoperte nella teoria intuitiva, come ad esempio quella di Zermelo-Fraenkel. Con tali teorie si vuole trovare un fondamento all'aritmetica, in modo che - ultimato il lavoro di riduzione delle altre branche della matematica all'aritmetica - si possa considerare fondato l'intero edificio matematico.

La stessa aritmetica, su cui grava il compito di fondare tutta la matematica, deve essere ridotta al più piccolo numero di assiomi.

Quest'opera, che pone anche le premesse al logicismo, è stata sviluppata da Giuseppe Peano.

Egli per fondare l'aritmetica si serve di tre concetti fondamentali (quello di zero, di numero naturale e successivo) implicitamente definiti da cinque assiomi: 1° zero è un numero naturale, 2° il successivo di ogni numero naturale è un numero naturale, 3° due numeri naturali che hanno lo stesso successivo sono uguali, 4° zero non è successivo di nessun numero naturale, 5° se godono di una certa proprietà sia lo zero, sia il successivo di qualunque numero naturale fornito di quella proprietà, allora di essa godono tutti i numeri naturali (principio di induzione matematica, già enunciato da Francesco Maurolico nel 1575).

Questi assiomi, però, non caratterizzano univocamente i numeri naturali, essi sono infatti riferibili a qualunque progressione, mediante opportune interpretazioni degli assiomi su tali modelli.

Russell[2] dà vari esempi di tali modelli; ne riportiamo uno.

Supponiamo che « zero » abbia il suo ordinario significato intuitivo, che « numero naturale » significhi « numero pari » e che con « successivo » s'intenda « successivo numero pari ». Otterremo allora la successione: 0,2,4,6,..., che verifica anch'essa i cinque assiomi di Peano.

Nè, d'altra parte, è possibile superare questa indeterminatezza degli assiomi di Peano aggiungendone degli altri in modo da poter individuare univocamente i numeri naturali; Thoralf Skolem, infatti, nel 1933, ha dimostrato che non è possibile caratterizzare univocamente l'insieme dei numeri naturali con un numero finito o con una infinità numerabile di assiomi. Non è quindi possibile scoprire qualche proprietà interna all'insieme dei numeri naturali, capace di distinguerlo dalle altre successioni[1]; bisogna quindi trovare una definizione caratterizzante l'aritmetica al di fuori di essa e cioè, secondo il programma logicista, nell'ambito della logica.

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[1] Sulle antinomie cfr E. W. Beth: *I fondamenti logici della matematica* (Traduzione e note di Ettore Casari), Feltrinelli, Milano, 1963, pp. 209-238.

[2] B. Russell: *Introduzione alla filosofia matematica*, Longanesi, Milano, 1962, pp. 22 ss.

Di qui il progetto di dare base sicura all'aritmetica riducendola alla logica, anzi considerandola come parte di questa[2].

Tale programma fu ideato e, in un certo senso, realizzato da Gottlob Frege, alla fine del secolo scorso ed esposto nei suoi *Grundgesetze der Arithmetik, begriffsschriftlich abgeleitet*[3]. Nel 1902, però, proprio mentre stava per essere pubblicato il secondo volume dei *Grundgesetze* di Frege, Russell scopriva che la logicizzazione della aritmetica era avvenuta a condizione di usare una logica troppo potente, in quanto includeva un assioma di comprensione che, non essendo sottoposto a opportune restrizioni, finiva col dar luogo all'antinomia della classe totale, detta proprio antinomia di Russell.

Per superare queste difficoltà, Russell presentava una teoria dei tipi logici, che pur sfuggendo alle antinomie logiche era in realtà troppo debole per realizzare il programma di ridurre l'aritmetica alla logica. Successivamente, in collaborazione con Alfred North Whitehead nei *Principia mathematica*[4] propone una nuova soluzione al problema dei fondamenti della matematica basata sulla cosiddetta teoria dei tipi ramificata. In essa, però, nonostante i tentativi degli autori di restare fedeli all'indirizzo platonista, si finisce col presentare un « primo quadro sistematico adeguato ad una impostazione concettualistica della matematica[5] ».

9. - SUCCESSIVI SVILUPPI DEL LOGICISMO. — Alla corrente logicista possono collegarsi altre impostazioni che, però, non nascono da una matrice platonista come quelle di Frege o del primo Russell.

Nel suo *Tractatus logico-philosophicus* Ludwig Wittgenstein dice che « la matematica è un metodo logico » (6.2) o « un metodo della logica » (6.234) e che « le proposizioni della logica sono tautologie » (6.1). « Le proposizioni della logica dicono dunque nulla. (Esse sono le proposizioni analitiche) » (6.11).

Questa concezione, ripresa dal Circolo di Vienna, rese del tutto inutile il tentativo di dare una spegazioni empirista della logica e della matematica. Gli *empiristi logici*, in genere, mostrarono infatti scarso interesse per le ricerche sui fondamenti della matematica, tranne per quanto riguarda i fondamenti del metodo induttivo e del calcolo delle probabilità, studiati specialmente da H. Reichenbach.

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[1] Cfr FRIEDRICH WEISMANN: *Introduzione al pensiero matematico* (traduzione di L. Geymonat), Einaudi, Torino, 1944, p. 148.

[2] « In tutto il procedimento [della matematica] non viene impiegata alcuna entità che non sia definibile nei termini delle costanti logiche fondamentali. [...] Da tutto ciò risulta stabilita la natura puramente logica della matematica ». (B. RUSSELL: *Principi della matematica*, Milano, 1951, p. 875).

[3] Due volumi, Jena, 1893 e 1903.

[4] Tre volumi, Cambridge, 1910 - 1913.

[5] ETTORE CASARI; *op. cit.* p. 141.

Risulta, anche solo da questi cenni, la stretta correlazione che da oltre un secolo lega gli studi matematici e quelli logici. A tale proposito Lucio Lombardo-Radice parla di « impossibilità storica e ideale di separare la ricerca matematica militante dalla riflessione logica che in essa si sviluppa » e dello stretto collegamento tra i progressi della logica e della matematica[1].

I vantaggi di questa simbiosi sono evidenti per entrambe. Proprio sotto l'influenza della logica e con l'ausilio di essa la matematica ha potuto diventare una scienza delle strutture. D'altra parte questa ha messo a disposizione della logica i suoi infallibili strumenti di calcolo nella cosiddetta *algebra della logica*, realizzando così, entro certi limiti, il sogno leibniziano di un *calculus ratiocinator*.

« Per quasi un secolo - scriveva nel 1934 Rudolf Carnap - i matematici e i logici si sono tenacemente sforzati di fare della logica una scienza esatta. In una certa misura i loro sforzi sono stati coronati da successo, in quanto la scienza della logistica ha insegnato a manipolare con precisione simboli e formule, che sono di natura simile a quelli impiegati in matematica[2] ».

10. - IL PROGRAMMA FORMALISTA.

— Tra le posizioni costruttiviste la più anti-platonista è il formalismo estremo, per il quale « l'esperienza matematica va analizzata unicamente in termini degli atti del riconoscimento di una successione finita di espressioni come ottenuta mediante un certo numero finito di regole formali. Tutto ciò che cade al di là delle operazioni elementari di tale riconoscimento viene considerato estraneo alla matematica. Va osservato che qui 'formale' deve essere inteso nel senso di 'meccanico' o 'effettivo' come si usa dire correntemente[3] ».

David Hilbert così illustrava la sua concezione anti-platonista scrivendo a Gottlob Frege: « Se io, come miei punti, penso quali si vogliano sistemi di cose, per esempio, il sistema: amore, legge, spazzacamino, ...; e poi non faccio altro che assumere tutti i miei assiomi come relazioni tra tali cose, allora le mie proposizioni, per esempio, il teorema di Pitagora, valgono anche per queste cose[4] ».

Il programma hilbertiano, formulato intorno al 1920, prevedeva innanzitutto la riduzione delle teorie matematiche agli assiomi; questi dovevano, poi, essere espressi in linguaggio formalizzato e simbolico per essere inseriti in una conveniente logica (quella dei predicati). Si sarebbe ottenuto, così, un unico sistema formale, nel quale - una volta dimostrato il teorema di completezza, cioè che ogni espressione del sistema è soddisfacibile o è refutabile - diventava possibile dimostrare 'meccanicamente' tutte le conseguenze degli assiomi posti alla base.

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[1] LUCIO LOMBARDO-RADICE: *I progressi della logica nel lavoro quotidiano del matematico*, in Società, a. XVII (1961), p. 143.

[2] R. CARNAP: *Sintassi logica del linguaggio* (traduzione di Alberto Pasquinelli), Ubaldini, Roma, 1968, p. 9.

[3] C. CELLUCCI: *op. cit.*, p. 34.

[4] Citato da HERBERT MESCHKOWSKI: *Mutamenti nel pensiero matematico* (traduzione di L. Lombardo-Radice), Boringhieri, Torino, 1963, p. 122.

Naturalmente, di per sè, gli assiomi non sono nè veri, nè falsi, sono stabiliti liberamente, cioè sono semplicemente convenzionali; pertanto, mentre per il platonismo la scelta degli assiomi viene suggerita e giustificata dal confronto di essi con una soddisfacente nozione di insieme, per il formalismo estremo non esiste giustificazione esterna a se stesso, nè quindi la possibilità di controllare per tale via la correttezza di una scelta di un sistema di assiomi, e tanto meno di orientarsi nella costruzione di esso.

Pertanto il formalismo in ultima analisi finisce con lo scegliere il proprio sistema di assiomi in base alle applicazioni di esso alla matematica o all'esperienza fisica. La matematica, dice Haskell B. Curry « è un sistema di proposizioni su un certo argomento, vere nella misura in cui corrispondonò ai fatti[1] ». Qui, dunque, il formalismo mostra la sua incoerenza e un suo grave limite trasformandosi in empirismo.

Ma ancor più compromettente per il programma formalista è stato lo sconcertante teorema di incompletezza di Gödel.

Hilbert aveva ammesso l'esistenza degli enti matematici in base alla possibilità di dimostrare la non contraddizione del sistema prescelto. Ma, nel 1931, Kurt Gödel pubblicava la celebre memoria « über formal unentscheidbare Sätze der Principia mathematica und verwandter Systeme » (Proposizioni formalmente indecidibili dei Principia Mathematica e di sistemi affini)[2]. In essa egli dimostra, tra l'altro, che un sistema formale abbastanza potente per esprimere l'aritmetica, cioè la logica dei predicati del secondo ordine, non può contenere all'interno di se stesso la dimostrazione della propria coerenza.

Con ciò crollava la possibilità dell'attuazione integrale del progetto formalista. Hilbert, infatti, aveva posto alla base del programma nella sua *teoria della dimostrazione* o *metamatematica*, la completezza del sistema e la possibilità di dimostrare la propria coerenza.

11. - SUCCESSIVI SVILUPPI DEL FORMALISMO. — Tentativi di realizzare almeno parzialmente il programma hilbertiano si sono sviluppati successivamente.

Carnap, Tarski, Gentzen ampliando la sfera della metamatematica riescono ad ottenere risultati in qualche modo positivi. Gerhard Gentzen, per esempio, nel 1936, con la introduzione del principio di induzione transfinita (che non essendo finitista non poteva essere accolto dal formalismo estremo) riesce a dimostrare la coerenza di una parte importante dell'aritmetica elementare.

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[1] H. B. CURRY: *Osservazioni sulla definizione e sulla natura della matematica* in C. Cellucci, *op. cit.* p. 153-4.

[2] In Monatshefte für Mathematik und Phisik, 38 (1931), pp. 173-198. Essa è tradotta in italiano in Evandro Agazzi: *Introduzione ai problemi dell'assiomatica*, Vita e Pensiero, Milano, 1961, pp. 203 ss. Cfr anche Nagel-Newmann, *La prova di Gödel*, Boringhieri, Torino, 1961.

Come non è possibile venir fuori da una palude tirandosi per i capelli, così la matematica per essere fondata esige una serie di sistemi sempre più ampi.

Il concetto di *ricorsività* (già creato da Skolem nel 1922), esteso da Herbrand, Gödel e Kleene alla ricorsività generale, quello di *computabilità*, introdotto da Post e Turing, quello di *effettività*, quello di *λ-definibilità* offrono, in proposito, molte possibilità[1].

12. - L'INTUIZIONISMO. — Una presentazione sistematica di questa visione si deve agli olandesi L. E. Brouwer e A. Heyting, ma per la sua critica all'infinito attuale cantoriano L. Kronecker può considerarsi con H. Poincaré precursore dell'intuizionismo, la cui matrice filosofica risale a Kant. Non è facile presentare in poche righe ed evitando il ricorso al linguaggio tecnico le linee essenziali di questa interessante concezione, che ancor oggi è in via di sistemazione.

Sottolineiamo in essa alcuni atteggiamenti fondamentali.

L'intuizione della verità è il solo criterio del vero, perciò l'intuizione matematica immediata rappresenta la parte esatta del pensiero matematico, indipendentemente da ogni verifica sperimentale e dallo stesso linguaggio che serve per comunicare le intuizioni.

Questa intuizione matematica non è automatica, ma ad essa collabora l'attività creatrice dello spirito[2]. Pertanto non è possibile ricercare i fondamenti della matematica prescindendo dalla considerazione dell'attività psicologica del matematico. La matematica, dunque, non è considerata (come nella visione platonista) una indagine su oggetti esterni, ma una introspezione sull'attività che il matematico svolge nel tempo e che si sviluppa per fasi successive, partendo dalla intuizione e poi considerando unicamente quegli enti che sono effettivamente costruibili sulla base di essa.

Perciò non è possibile introdurre l'infinito attuale (che non è costruibile), ma solo l'infinito potenziale e intendere, quindi, per esempio, la successione ordinata dei numeri naturali come proseguibile illimitatamente nel tempo. Le ricerche che prescindono da tali considerazioni di ordine psicologico non possono approdare alla conoscenza della essenza della matematica; e ciò vale non solo per l'indagine sui fondamenti, ma anche per le altre richerche matematiche.

Solo in una seconda fase, la matematica che all'inizio è soltanto intuitiva sarà rivestita del linguaggio in vista della comunicazione con gli altri. E solo in una terza fase interviene la logica, che è analisi del linguaggio.

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[1] Per avere una prima informazione sui vari problemi elencati cfr in *Enciclopedia della Scienza e della Tecnica*, Mondadori, le relative voci trattate da Ettore Casari con indicazioni bibliografiche.

[2] Cfr FERDINAND GONSETH: *Philosophie mathématique*, Hermann, Paris, 1936, p. 62.

Pertanto la matematica è indipendente dalla logica. Anzi sia il linguaggio, sia la logica dipendono dalla matematica. D'altronde si sa che la matematica necessita fin dall'inizio del concetto di infinito, mentre la logica si occupa soltanto di insiemi finiti.

È, quindi, naturale che la logica formale perde la fiducia illimitata che gode presso i logicisti e i formalisti; Brouwer parla di « illusione della libertà logica ».

Da ciò segue il rifiuto intuizionista dell'applicazione illimitata del principio del terzo escluso (la quale suppone che ogni problema matematico sia risolubile) e conseguentemente della dimostrazione per assurdo.

Lo stesso Hilbert, che nel 1900 aveva enunciato il principio della risolubilità di ogni problema matematico, ha dovuto in un secondo tempo rinunciarvi enunciando invece il *problema fondamentale della matematica o problema della decisione.*

In conseguenza, gli intuizionisti sono costruttivisti, cioè accettano solo l'effettiva costruzione per l'introduzione di oggetti matematici e per le dimostrazioni di esistenza.

Scrive, in proposito, Heyting: « Un teorema matematico esprime un fatto puramente empirico, cioè il successo di una costruzione. '2 + 2 = 3 + 1' dev'essere letto come un'abbreviazione dell'asserzione 'Ho effettuato le costruzioni mentali indicate da 2 + 2 e da 3 + 1 e ho trovato che esse conducono allo stesso risultato' »[1].

Coerentemente con il proprio sistema gli intuizionisti respingono ogni assiomatizzazione della matematica, perchè essa è una semplice costruzione verbale e quindi priva di vero contenuto matematico e di capacità creativa. Non accettano nè la teoria intuitiva degli insiemi di Cantor, nè quella assiomatica di Zermelo, del quale - ovviamente - respingono l'assioma delle infinite scelte[2].

Sviluppano una propria teoria degli insiemi che sostituisce al concetto platonista di Cantor di insieme[3], i concetti di *spiegamento* e di *specie*. Per spiegamento intendono un'accurata determinazione dello schema costruttivo attraverso il quale vengono generati gli elementi dell'insieme. Per specie intendono una precisazione del concetto di definizione intesa come isolamento, mediante la formulazione della proprietà caratteristica di quegli enti che godono di tale proprietà[4].

Sulla base di questi concetti gli intuizionisti possono introdurre il cosiddetto « continuo intuizionista » inteso come la specie delle successioni convergenti illimitate di numeri razionali o - per sottolineare la circostanza che il continuo intuizionista non si fonda sul concetto di misura - di elementi di una opportuna successione ordinata[1].

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[1] A. HEYTING: *Intuitionism. An introduction*, North-Holland, Amsterdam. 1956, p. 8.

[2] Cfr. RUSSELL, op. cit., pp. 192 segg.

[3] « Per insieme si intende una collezione M, concepita come un tutto, di oggetti ben determinati e distinti (detti elementi) della nostra intuizione o del nostro pensiero », in *Beiträge zur Begründung der transfiniten Mengenlehre*, Mathematische Annalen, 46(1895), p. 481.

[4] E. CASARI, *op. cit.* pp. 202 ss.

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Le indicazioni date sono sommarie e lacunose. Diamo perciò qui di seguito i titoli di alcune opere che costituiscono una bibliografia in lingua italiana per chi voglia introdursi nella conoscenza del problema.

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[1] Cfr L. E. Brouwer: *Fondamenti storici, principi e metodi dell'intuizionismo*, in C. Cellucci, *op. cit.* pp. 223-231.

TRANSLATION: J. E. HOLMSTROM · FOLKESTONE

THE OBJECTS AND FOUNDATIONS OF MATHEMATICS

Reference is made firstly to the difficulty of giving any single definition of mathematics, because that science is in continual evolution and is characterised by a tendency towards ever greater abstraction. Particularly since the end of the last century, this tendency has given rise to various definitions of mathematics as being a « science of structures », under which description some of the fundamental mathematical structures and the more recent concept of « category » are explained here. Following upon this, the problem raised by the foundations of mathematics is examined and the positions taken up respectively by the platonists and the «logicists» are illustrated, referring to those of Cantor, Frege, Russell, Wittgenstein and to what is called logical empiricism. Further, the significance of the formalist programme associated with David Hilbert is described, as well as the importance of Gödel's theorem of incompleteness which has led to the breakdown of formalism as an integrated system. After mentioning the endeavours of post-Hilbertian formalists a conceptual picture is presented of the intuitionist trend, giving prominence to Brouwer and Heyting while stressing the substantial differences that exist with respect to the 'logicist' and formalist schools as well as the originality of the arguments associated with certain fundamental mathematical concepts. Brief bibliographical references are appended for enabling the non-specialist Italian reader to acquire the essentials needed for entry into the questions outlined.

1. - WHAT MATHEMATICS IS. — A true understanding of what mathematics means and of what it contains presupposes a clear awareness of its historical development[1]. Actually the modern trend in mathematical research is the outcome of profound intellectual labour by many generations of mathematicians, particularly in the last century. In any case mathematics is an activity which reflects the human creative urge towards « contemplative reasoning impelled by desire for aesthetic perfection. Its basic elements are logic and intuition, generality as well as individuality. While different traditions may stress different aspects the vitality, the utility and the supreme value of mathematics are nourished solely by the reactions between these mutually opposed forces and the struggle to bring about their synthesis[2]. »

In a discipline such as mathematics whose origins are shrouded in the mists of time, and which has been presented as an organised science for at least twenty-five centuries, it is no easy matter to discern what goals have constantly been pursued throughout its development and evolution. One such constant tendency can, however, be observed in the quest for ever wider generalisation. The endeavour to abstract mathematical entities from concrete circumstance was undoubtedly active among the earliest of the geometers concerned in Egypt, it had already become organic and present in the consciousness of the Greek mathematicians and it remains one of the paramount characteristics of modern mathematical research.

Today mathematics can no longer be considered the science that deals with quantity, nor can it be sufficiently defined as the science of relationships[3].

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[1] ETTORE CARRUCCIO: *Matematica e logica nella storia del pensiero contemporaneo*, Gheroni, Torino, 1958, p. 7.

[2] R. COURANT e H. ROBBINS: *Che cos'è la matematica?*, Einaudi, Torino, 1950, p. 25.

[3] cfr GIOVANNI VAILATI: *La più recente definizione della matematica*. In Scritti di Vailati; Leipzig-Firenze, 1911, pp. 528-533.

We have to think of it as such but also as being something more. Frege and the logical school saw it as the science of relations but Hilbert and the formalists regard it as the science of what is possible, while intuitionists such as Brouwer and Heyting think of it as the science of possible « constructions ». And perhaps even so we have not yet fully covered the range of today's mathematical thought.

But if we feel tempted to conclude with Bertrand Russell that pure mathematics « may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true »[1] we can be reassured by Emile Borel's affirmation that « mathematics is the only science in which one always knows exactly what one is talking about and can be certain that what is said is true »[2]!

2. - MATHEMATICS AS THE SCIENCE OF « STRUCTURES ». — What, then, are the new concepts involved in modern mathematical research? Mathematics is not something still being added to an old existing structure, on very firm foundations, which successive generations have raised to ever higher levels; rather it would be true to say that each age prefers to reconstruct the building in accordance with a new design and standing on a new foundation[3].

Our own age is especially prolific in its suggestions of new objectives and new kinds of mathematical « architecture », always endeavouring to extend the generalising and abstracting potency of mathematics to the utmost.

The objects of mathematical study are always abstractions. This is true even of natural numbers, straight lines or triangles. But the objects of modern mathematics are even more abstract then those.

A first simple example to illustrate this generalising tendency of modern mathematics can be obtained by considering how the traditional concept of « function » has evolved, leading on to that of « functional » and then, in general analysis, to that of a still more complete transforrmation[4].

Another example is provided by considering how the concept of space has evolved.

From the intuitive idea of physical space modelled upon Euclidean geometry we have gone on to abstract space, which is a whole put together from any kind of elements (numbers, applications, polynomials, etc.) that possess a structure based on the ideas of neighbourhoods (topological spaces), distance (the metrical spaces of Hausdorf or the *Éspaces distanciés* of Bouligaud-Fréchet) or normalization (such as Banach spaces), etc.

Here the concept of structure is involved which plays a decisive role in determining the objective of modern mathematics. At the present time, the quest for greater generality and for ever more general structures is one of the most powerful stimuli for mathematical investigations.

When, nowadays, we want to define mathematics we often refer to it as the science of structures; it is thus, in fact, that mathematicians rediscover a unity underlying the areas of thought which traditional mathematics has regarded as unconnected.

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[1] Citato in Nagel e Newman: *La prova di Gödel* (translation by Luigi Bianchi), Boringhieri, Torino, 1961, p. 20.

[2] In the collective work edited by LE LIONNAIS: *Les grands courants de la pensée mathématique*, Cahiers du Sud, Marseille, 1948, p. 24.

[3] A. LENTIN et J. RIVAUD: *Leçons d'Algèbre moderne*, Vuibert, Paris, 1964, p. V.

[4] Cfr M. FIORENTINI: *Dall'analisi matematica classica all'analisi funzionale e all'analisi generale*, in Cultura e Scuola, 12 (1964), pp. 214 ss. et sea.

At the international symposium on the reform and coordination of mathematical and physical teaching, held at Lausanne in January 1967, the third of the final resolutions regarding the teaching of mathematics was to the effect that « Mathematics increasingly tends to develop in the direction of a general science of structures. Such structures give it a considerable power for application, for information and for unification. Knowledge and mastery of them, and their use in the conquest of reality, are the true purpose of mathematical teaching[1] ».

Investigation of abstract structures does not, however, imply moving away from what is concrete; « in no sense do such studies become dissipated in insubstantial musings with little or no practical ability »; on the contrary « we have been armed, in this way, with a weapon of extraordinary power to solve, by merely formal reasoning, a whole range of concrete problems which at first sight appear to be extremely diverse - potentially, indeed, to solve an infinite number of different concrete problems if an infinite number of concrete « models » exist having the same formal structure. The more abstract the mathematics, the simpler and more useful it becomes[2] ».

3. - MATHEMATICAL STRUCTURES. — The idea of structure is of wider scope than the meaning that concerns us here. One may speak, for instance, of logical, physical, biological, linguistic, psychological, social and yet other kinds of structures and it may not always be possible to define their limits.

In the most general sense a structure is to be understood according to Jean Piaget as being « a system of transformations which is subject to certain laws implicit in its being a system - as distinct from laws dictated by the properties of the elements comprised in the system - and which maintains or enriches itself as a result of the interplay of the transformations it undergoes without such transformations either overpassing its own boundaries or involving elements that are external to itself. Thus, in short, a structure combines three characteristics: totality, transformability and self-regulation[3] ».

The first essential characteristic of structure, then, is totality; that is to say, its constitutive elements are dependent on the system as a whole and they cannot be chosen without having regard to the whole. Another characterizing feature of a structure is the « whole » of the transformations able to take place in it; or perhaps, to allow the inclusion of certain mathematical structures, we may say more generally the whole set of relationships. Finally, it is essential for a structure to be capable of self-regulation, by which is meant that the laws governing it must always be such as act upon the elements that form part of the structure.

A systematic presentation of mathematics as the science of structures has been available since 1939 in *Eléments de Mathématiques*, the impressive collective work brought out under the pseudonym *Nicolas Bourbaky*[4], by a group of French mathematicians continuously replacing one another. The

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[1] *Actes du Colloque international sur le réforme et la coordination des enseignements de mathématique et de physique*, in Dialectica, 21(1967), p. 258.

[2] LUCIO LOMBARDO RADICE: *Istituzioni di albebra astratta*, Feltrinelli, Milano, IV edizione, 1967, pp. 4-5.

[3] JEAN PIAGET: *Lo strutturalismo* (translated by Andrea Bonomi), Mondadori, Milano, 1968, p. 39.

[4] Named after a French general who, during the Franco-Prussian war of 1870, acted contrary to French military tradition by choosing to let his troops be interned in Switzerland rather than engage them in an unequal battle with the Prussians.

« bourbakists » are to be credited not only with the accurate definition of the concept of mathematical structure but also with having opened up a new outlook on the « architecture » of mathematics[1].

The more important mathematical structures can all be reduced to a few simple ones which are called « parent structures » and which, according to Bourbaki, can be divided into three groups: albebraic, sequential and topological. Multiple structures can be obtained by superimposing and combining these three[2].

In all types of mathematical structures the « whole » is made up of appropriate abstract wholes (by which is meant entities of an undefined kind).

In algebraic structures the transformations consist of operations (also called laws of composition) which, likewise, are undefined.

In sequential structures the elements of the supporting whole are characterised by relational properties able to confer ordered sequence. This can be considered as a generalisation of the order that characterises the sequence of natural numbers.

In topological structures the relations between the elements appertain to such concepts as proximity, envelopment, limits, boundary, continuity, etc.

Furthermore, the three fundamental structures developed by the bourbakists have been brought together under what is known as the *general theory of structures*. This theory deals with *models of relation* which are couples consisting of a whole with relational structures both for that whole and for the whole made up of it s parts, if required. The concept of relationship can, in fact, be applied equally well to albegraic, to sequential or to topological operations.

Still more recently MacLane, Eilenberg, Cartan, Buchsbaum, etc. have revealed yet another general aspect of the general theory of structures. This uses studies of *categories*, which are sequential couples each made up of a whole of all the applications (functions) comprised therein. In studying categories, attention is switched from the structures themselves to the operations generated by them. It will readily be perceived that such studies have great possibilities.

4. - STRUCTURES OF THE ASSEMBLY R CONSISTING OF REAL NUMBERS. — A simple example in which a variety of mathematical structures operate is supplied by the whole of the real numbers. This example shows a multiple structure within which the various simple structures can easily be separated.

Algebraic structures in R: the « whole » of the real numbers has a group structure[3] with respect to addition. As such, it is a closed structure (by which is meant that the sum of two real numbers is still a real number); it has a neutral element (zero) and every real number has symmetry (its opposite number); finally, addition has the property of associativeness. Since addition also has

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[1] Cfr. N. BOURBAKI: *L'architecture des mathématiques*, in F. Le Lionnais, op. cit. pp. 35.

[2] Cfr AA.VV.: *Strutture algebriche e strutture topologiche*. Feltrinelli, Milano, 1963.

[3] *Group* is the term applied to any set G composed of elements that satisfies the following axioms:

(1) There exists a *law of composition* « T » whereby every couple of elements (a, b) in G is matched by an element c in G (the closure of G with respect to T);

(2) This law of composition is associative, i.e.: $aT\ (bTc) = (aTb)Tc$

(3) There exists a neutral element « c » such that, for every a, $aTe = eTa = a$;

(4) Every element a allows of an inverse element a^{-1} such that:

$$aTa^{-1} = a^{-1}Ta = e$$

the property of commutability, the additive group of numbers is Abelian (commutative).

But zero has no reciprocal, so the whole R not containing a zero has also a group structure in regard to multiplication, as can easily be seen. Besides this, multiplication has the distributive property as regards addition, so we are enabled to say that whole of real numbers has a ring structure[1].

Ordinal structures in R: Here we are dealing with the whole natural series wherein any two natural numbers are coupled by the relation « less than » or « equal to », which has the three properties of being reflexive, anti-symmetrical and transitive.

Topological structures in R: If we confined ourselves to considering algebraic and sequential structures we might conclude that the whole of real numbers, dealt with up to this point, was not very different from the whole of rational numbers Q, for the latter may easily be seen to have the same structures as those considered above. Therefore algebraic and serial structures are not sufficient by themselves for a complete characterization of the whole of real numbers. That can be achieved by way of topological considerations and it is indeed obvious that whereas the whole R is continuous the whole Q is not[2].

Yet other structural properties can be obtained by superposition, for instance by superimposing algebraic upon sequential structures (the results being obviously common to R and Q). Thus, given three elements a, b and c which appertain to R (or to Q alone), such that $a < b$ and $c > o$, the new relation $ac < bc$ will certainly be true.

We have, therefore, given an example of multiple structure obtained by combining simple structures and superimposing them on one another.

5. - THE ALGEBRAIC NOTION OF GROUPS AND THE ERLANGEN PROGRAMME.

— Some considerations relating to group structure may serve to convey an idea of how important the study of algebraic structures can be for other branches of mathematics, and also beyond those.

The group was the earliest structure to be studied. It was introduced by Évariste Galois in 1832 for the purpose of proving that a generic equation of higher than the fourth degree cannot be solved by means of roots[3].

The extraordinary fruitfulness and the constructive uesfulness of the group idea can be seen, for instance, in the new conception of geometry put forward by Felix Klein in 1872 in the famous Erlangen programme, wherein it is represented as bieng *the study of properties that remain invariable with respect to a given group of transformations.*

A first, very elementary, investigation of geometry can be made by subjecting geometrical figures only to those transformations that that leave all distances invariable (and consequently the angles, parallels, straight lines, etc.). In other words only the group of *congruences* (movements and symmetries) is considered, as is done in the study of equalities.

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[1] *Ring* is the term applied to every set A composed of elements that have a group structure in accordance with the law of composition « T » and which furthermore is closed and is associative with respect to a second law of composition « ⊥ ».

[2] That is to say, if the couple of sets (A, B) constitutes a « section » of R it is found that either A has its maximum or B has its minimum in R. Alternatively it is possible to find, in Q, sections (A, B) such that neither A has a maximum nor B has a minimum in Q, implying that « gaps » occur within Q. This would further imply that the real numbers can be brought into biunivocal correspondence with points on a straight line with orientation r (a linear continuum) whereas on the other hand any correspondence between Q and the line r is solely univocal. In fact, while q has the power of the number (meaning that it can be put in biunivocal correspondence with the set of natural numbers N), R has the power of the continuum.

[3] Cfr *Oeuvres mathématiques d'Evariste Galois*, by G. Verriest, Gauthier-Villars, Paris, 12.th edition, 1951, pp. 33 et seg.

The congruence group can, however, be considered as a sub-group of the more comprehensive *similarities* group in which the angles (and hence parallels, straight lines and shapes of the figures) are kept invariable while allowing the dimensions to be changed. Elementary geometry, in which it is only these transformations that are studied, can therefore be defined as the study of figures that are invariable with respect to the similarities group.

The similarities group in turn can be considered as a sub-group of a still wider group, that of *affinities*, which allows changes to be made also in the angles (and hence the shapes) but not the parallels or straight lines. For instance, in the geometry of affinities a square can be transformed into any parallelogram.

Further, the affinities group can be considered a sub-group of the projective group wherein the transformations consists of *projectivity* (the making of projections and sections). Thereby parallel lines can be changed to converging lines (as in perspective drawing) but their straightness remains unchanged.

Thus projective geometry can be defined as the study of properties that are invariant with respect to projectivity (or homography).

If, going further, we allow straight lines to be transformed into conics or into algebraic curves of higher than the second degree we arrive at the *birational transformations* group, whereof the projectivity group is a sub-group. Thus algebraic geometry is seen as the study of properties that are invariant with respect to birational transformations.

Finally, if we widen the transformation group still more by considering our figures as if they were on a material which was elastic but not able to be torn or folded, we obtain the *homeomorphic* group which is studied in topology. Thus topology is the study of properties that are invariant, with respect to the group of biunivocal continuous transformations, such as occur for example in geographical maps or in the images obtained when objects are viewed through appropriate deforming lenses.

In this way, the idea of groups shows us the different geometries no longer as separate theories but as boxes enclosed within one another under an enveloping general theory.

As regards the possibility of applying group theory to the experimental sciences it is only necessary to recall that it was the study of symmetry groups that made it possible to foresee the existence of elementary particles which were discovered experimentally by the physicists a little later.

Group structure can be recognised also in logic, either by considering Boolean algebra or directly in the logic of propositions[1].

6. - THE TEACHING OF MODERN MATHEMATICS. — The usefulness of abstract algebra and topology as means of penetration into other departments of mathematics is apparent from what has been said above.

Today it is no longer possible to cultivate mathematics without algebra[2]. This is true to such an extent that algebra is not merely a branch of mathematics but « occupies, within the latter, the place that mathematics as a whole has long occupied with respect to physics »[1].

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[1] Cfr JEAN PIAGET; *op. cit.*, pp. 63-4.

[2] P. DUBREIL et M. L. DUBREIL-JACOTIN: *Leçons d'algèbre moderne*, Dunod, Paris, 1961, Preface.

It is clear, likewise, that algebra and topology both pre-suppose the theory of sets, which may with great advantage be prefixed by mathematical logic as a source of those elements that are indispensable for its rigorous formal treatment.

Thus the learning of mathematics has an essential part to play in education besides being in itself intellectually attractive even to very young students.

Discussing how mathematics might be taught along these lines, Aldo Masullo remarked that they would enable « the stimulus to learning to be sought for directly in the properties of the logical objects ». Stressing that this method would be anti-pragmatic he argued that it should cause learning to be valued for its own sake instead of as a step towards some other end, insofar as « the teaching will no longer depend on the principle of progressively animating the psychological functions by calling them into action pragmatically while aiming at the acquisition of logical operative structures as the ultimate object, but will rely on the principle of directly exhibiting the logical structure as a model for the psychological operation... Learning will thus cease to be valued merely for its utility and its processes will cease to be regarded merely as instruments for attaining an end which transcends them. Its direction will cease to be pragmatic. When the processes in use are attuned to the structure of the significant data they handle, becoming 'disinterested' instead of obeying interests external to themselves, the subject will be seen to have enjoyable aesthetic values in its own right and to be something kept alive merely for the pleasure of living - exactly as has always been the case in art and play[2] ».

As Ferdinand Gonseth remarked when concluding the Lausanne Symposium already mentioned, today's vision of mathematics also opens they way to a new methodology - the axiomatic method, formalisation, etc. - by means of which we may hope to arrive at a new solution to the problem of the basis of mathematics[3].

To throw light on the object of mathematics it is necessary also to illuminate the foundation on which it rests.

7. - RESEARCHES INTO THE FUNDAMENTALS OF MATHEMATICS. — It is no longer possible to be satisfied with the traditional view of mathematics as being built up of theorems logically deduced from certain *axioms which are intuitively obvivus* and which have provided support for philosophers and mathematicians over many centuries, a view which has given rise to the common opinion that mathematics is the exact science par excellence.

Reflecting on the aims sought by mathematical activity, it is possible as a first approximation to take up either of the following two contrary positions:

One may look at it from the point of view of its *contents*, as being the science that deals with particular matters in existence before it; or one may look at it from the point of view of its *forms*, as being a commentary upon certain entities created by mathematics itself from time to time[1]. The first mentioned of these two viewpoints may be called *platonic* and the second *constructive.*

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[1] CLAUDE CHEVALLEY; *Concetti fondamentali di Algebra*, Feltrinelli, Milano, 1964.

[2] ALDO MASULLO: *L'apprendimento matematico e gli sviluppi delle scienze psicologiche e sociali*, in Didattica della Matematica.

[3] Dialectica, 21(1967) p. 270.

In the context of the Platonic view, mention was made of endeavours being made to reduce mathematics to logic, under the name of *logicism.*

Disregarding for brevity the many intermediate positions, it is proposed here to offer some references to the logicist point of view and, on the constructionist side, some references to *extreme formalism* and to *intuitionism.*

8. - PLATONISM AND LOGICISM. — According to the platonist conception, as already stated, mathematics is a science which discovers and isolates entities outside of itself. Its centre of interest, therefore, lies among external objects and its main preoccupation consists in giving precision to the axioms that distinguish the basic characteristics of the objects on which it is founded. In the final analysis these objects are wholes.

The relevant theories here are either the great intuitive theory of sets due mainly to Georg Cantor or the axiomatic set theories evolved to overcome the antinomies[2] found in intuitive theory, such as Zermelo-Fraenkel theory. It is sought, by these means, to establish a theoretical foundation for arithmetic upon which the entire mathematical edifice can be based once the work of reducing other branches of mathematics to arithmetic is completed.

Arithmetic itself, upon which the whole of mathematics is to be founded, needs to be reduced to the smallest possible number of axioms.

This work, demanded also by the premisses of logicism, has been developed by Giuseppe Peano.

To serve as a foundation for arithmetic he utilises three basic concepts those of zero, of natural numbers and of successive numbers - which are implicitly defined by five axioms: 1. zero is a natural number; 2. the successor to every natural number is a natural number; 3. any two numbers having the same successor number are equal; 4. zero is not successive to any natural number; 5. if a certain property appertains either to zero or to any natural number resulting from that property, then the same property appertains to all natural numbers (this being the principle of mathematical induction already enunciated by Francesco Maurolico in 1575).

These axioms, however, do not characterise unequivocally all natural numbers; but provided the five axioms are suitably interpreted in application to the models they are referable to any progression.

Russell[3] gives various examples of such models, one of which is as follows: suppose that « zero » has its usual intuitive meaning, that « natural number » means « even number » and that « successive » is taken to mean « successive even number ». This will give us the succession 0, 2, 4, 6... which again verifies Peano's five axioms.

On the other hand it is impossible to overcome the indeterminacy of Peano's axioms by supplementing them with others in such a way as to be able to specify natural numbers unequivocally. In fact Thoralf Skolem proved in

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[1] Cfr ETTORE CASARI: *Questioni di filosofia della matematica,* Feltrinelli, Milano, 1964, pp. 11-2 and passim; CARLO CELLUCCI: *La filosofia della matematica,* Laterza, Bari, 1967, pp. 15-6 and passim.

[2] On the antinomies, see E. W. BETH: *I fondamenti logici della matematica* (Translation and notes by Ettore Casari), Feltrinelli, Milano, 1963, pp. 209-238.

[3] B. RUSSELL: *Introduzione alla filosofia matematica,* Longanesi, Milano, 1962, pp. 22, translation of *Introduction to mathematical philosophy,* Allen & Unwin, London, 1919, 12th imprn. 1967.

1933 the impossibility of unequivocally characteristing the whole of the natural numbers by either a finite or an infinite number of axioms. Hence there is no possibility of discovering some internal property appertaining to the whole of natural numbers enabling that whole to be distinguished from other series[1]. A definition for the characterisation of arithmetic must, therefore, be looked for externally, and according to the logicians' programme this means looking for it in the field of logic.

Hence the proposal to seek a firm foundation for arithmetic by connecting it back to logic, henceforward regarding it as a part of the latter[2].

This programme was first conceived by Gotlob Frege at the end of the last century and in a sense carried out by him in his *Grundgesetze der Arithmetik, begriffsschriftlich abgeleitet*[3]. In 1902, however, just as the second volume of Frege's *Grundgesetze* was about to be published, Russell discovered that the logicalisation of arithmetic had been effected conditionally upon the use of too powerful a logic, since it included an axiom of comprehension without appropriate restrictions and this led to an antinomy of the whole class (which has become known as Russell's antinomy).

To overcome the difficulty Russell advanced a theory of logical types. Although this avoided the logical antinomies it was in fact not strong enough for carrying out the programme of reducing arithmetic to logic. Later, in collaboration with Algred North Whitehead he proposed in *Principia mathematica*[4] a new solution to the problem of the foundations of mathematics, based on what is called the branched theory of types. In this work, however, despite the authors' efforts to remain faithful to Platonic doctrine they end by putting forward a first attempt to establish a systematic conceptualist framework for mathematics[5].

9. - SUCCESSIVE DEVELOPMENTS OF LOGICISM. — Current logicist schemes may be conjoined with proposals on different lines, not derived from Platonic principles as are those of Frege and of Russell at first.

Ludwig Wittgenstein declares in his *Tractatus logico-philosophicus* that « mathematics is a logical method » (6.2) or « a method employed in logic » (6.234) and that « logical propositions are tautologies » (6.1) which « therefore tell us nothing (being in fact analytical propositions) » (6.11).

This conception, taken up by the Vienna group, cut away the ground from attempts to provide an empirical explanation of logic and mathematics. Generally speaking, in fact, the *logical empiricists* have shown little interest in investigating the foundations of mathematics apart from the foundations of the inductive method and of probability calculus, which have been studied especially by H. Reichenbach.

Even these references alone are sufficient to indicate the intimate relations that have existed for over a century between mathematicians and logicians. In this context Lucio Lombardo-Radice refers to « the historical and ideal

[1] Cfr FRIEDRICH WEISMANN: *Introduzione al pensiero matematico* (Translation by L. Geymonat), Einaudi, Torino, 1944, p. 148.

[2] « In no mathematical process is any entity used that cannot be defined in terms of the fundamental logical constants... All this goes to demonstrate the purely logical nature of mathematics ». Here re-translated from the Italian edition (*Principi della matematica*, Milano, 1951, p. 875) of B. Russell: *Principles of mathematics*. Allen & Unwin, London. 2nd edn. 1967.

[3] Two volumes, Jena, 1893 and 1903.

[4] There volumes, Cambridge, 1910 - 1913.

[5] ETTORE CASARI; *op. cit.* p. 141.

impossibility of isolating active mathematical research from reflections of the logical environment in which it grows » and to the close connection between advances in logic and in mathematics[1].

The advantages of this symbiosis are obvious as regards both. Manifestly it is the influence and assistance of logic that have enabled mathematics to become the science of structures. On the other hand mathematics has made available to logic its unfailing instrument known as the *algebra of logic*, thereby realising within certain limits the Leibnitzian dream of a *calculus ratiocinator*.

« For close on a century » - wrote Rudolf Carnap in 1934 - « mathematicians and logicians have been strenuously attempting to màke logic into an exact science. Up to a point their endeavours have been crowned with success insofar as logical science has taught the precise manipulation of symbols and formulae which are similar in kind to those used in mathematics »[2].

10. - THE FORMALISTIC PROGRAMME. — Extreme formalism is among the most anti-platonist of attitudes. According to this view « mathematical experience is analysed solely in terms of recognising a finite series of expressions obtained by means of a certain finite number of formal rules. Whatever falls outside the scope of the elementary operations involved in such recognition is not considered to be mathematical. It should be remarked that here the word 'formal' needs to be understood in the sense of what is commonly called 'mechanical' or 'effective'[3].

David Hilbert, writing to Gottlob Frege, illustrated his anti-platonist conception in the following way: « If I choose to imagine any 'system' of things whatever, such as for instance a system made up of 'love', 'law', 'street-sweeper', etc. and then do nothing else but assume all my axioms to correspond with relations between these things, then all my propositions (for example the theorem of Pythagoras) will be true also of these things[4] ».

The Hilbertian programme which was formulated about 1920 envisaged, first of all, reducing mathematical theories to axioms, which were thereupon to be expressed in formalised and symbolic language so as to permit of their insertion into a suitable logic (that of predicates). In this way a unique formal system was to be obtained. As soon as the theorem of completeness was proved - i.e. that every expression forming part of the system was capable of being satisfied or refuted - it would then become possible to prove « mechanically » all the consequences of the axioms postulated as the base of the system.

Of course the axioms in themselves are neither true nor false; they are nothing other than freely established conventions. But whereas, according to platonism, the choice of axioms is suggested and has to be justified by their being compatible with what is considered to be a satisfactory idea of the whole, according to extreme formalism there can be no external justification, therefore no possibility of thus checking whether a system of axioms is correctly chosen, still less of being guided in its construction.

That being so, formalism is compelled in the last analysis to choose its own system of axioms on the basis of its applications to mathematics or physical experience. Mathematics, says Haskell B. Curry, is a system of propositions in a certain subject field which are true in so far as they correspond with the facts[1]. Here, therefore, formalism displays its lack of coherence and one of its serious defects by turning itself into empiricism.

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[1] LUCIO LOMBARDO-RADICE: *I progressi della logica nel lavoro quotidiano del matematico*, in Società, a. XVII (1961), p. 143.

[2] R. CARNAP: *Sintassi logica del linguaggio* (translation by Alberto Pasquinelli), Ubaldini, Roma, 1968, p. 9.

[3] C. CELLUCCI: *op. cit.*, p. 34.

[4] Cited by HERBERT MESCHKOWSKI: *Mutamenti nel pensiero matematico* (Translation by L. Lombardo-Radice), Boringhieri, Torino, 1963, p. 122.

But the formalist programme has been compromised even more deeply by Gödel's disconcerting « theorem of incompleteness ».

The basis on which Hilbert had accepted the existence of mathematical entities was the possibility of proving that a chosen system was not contradictory. But Kurt Gödel published in 1931 his famous paper « über die formal unentscheidbare Sätze der Principia mathematica und verwandter Systeme » (Formally indeterminable propositions in Principia mathematica and kindred systems)[2] . In this paper he proved, among other things, that a formal system powerful enough to express arithmetic - namely, the logic of second-order predicates - cannot contain within itself a proof of its own coherence.

This put an end to any possibility of carrying through the formalist project as a whole, for Hilbert had actually based that project (forming part of his *theory of proof* or *metamathematics*) on the completeness of the system and its ability to prove its own coherence.

11. - Subsequent developments of formalism. — Endeavours continued after this to realise the Hilbertian programme at least partially.

Carnap, Tarski and Gentzen succeeded in obtaining results that were to some extent positive by widening the scope of metamathematics. For example Gerhard Gentzen was able, in 1936, to demonstrate that a large part of elementary arithmetic was coherent by introducing the principle of transfinite induction (which, not being finitist, was not acceptable to extreme formalism).

It being impossible to lift oneself by one's own bootstrings, the quest for a basis on which mathematics can rest demands an ever widening series of systems.

In this sense many possibilities have been mooted, including the concept of *recoursivity* (already created by Skolem in 1922, later extended into general recoursivity by Herbrand, Gödel and Kleene), that of *computability* introduced by Post and Turing, and those respectively of *effectivity* and *λ-definability*[3].

12. - Intuitionism. — The credit for systematically presenting this point of view is owed to L. E. Brouwer and A. Heyting in the Netherlands, but the philosophical mould in which intuitionism is cast dates back to Kant so that its precursor in the existing form may be considered to have been L. Kronecker for his critique of the Kantian concept of infinity, now present, along with H. Poincaré. This interesting conception is still in process of being worked out even today and its essentials are not easy to explain in a few lines without

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[1] H. B. Curry: *Osservazioni sulla definizione e sulla natura della matematica* in C. Cellucci, *op. cit.* p. 153-4.

[2] In Monatshefte für Mathematik und Phisik, 38 (1931), pp. 173-198. Italian translation in Evandro Agazzi: *Introduzione ai problemi dell'assiomatica*, Vita e Pensiero, Milano, 1961, pp. 203 et seq. See also Nagel-Newmann, *La prova di Gödel*, Boringhieri, Torino, 1961.

[3] For introductory information on the various problems mentioned see, in *Enciclopedia della Scienza e della Tecnica*, Mondadori, under the relevant terms explained, with bibliographical references, by Ettore Casari.

the use of technical language, but some of the fundamental principles it contains may be pointed out here.

Intuition of the truth is held to be the sole criterion as to what is true. Hence mathematical intuition represents what is accurate in mathematical thought, independently of any experimental verification and even of the language employed to communicate the intuition.

That mathematical intuition is not automatic, but is aided by the creative activity of the mind in collaboration with it[1]. Therefore the foundations of mathematics cannot be elucidated without having regard to the mathematician's psychological activity. Hence mathematics is not to be considered (as the platonist sees it) a process of investigating external objects but one of introspection into the activity which the mathematician performs in time and which evolves through successive phases, starting from intuition and thereafter attending solely to such entities as can in fact be constructed on that base.

Accordingly it is not possible to bring in the concept of present infinity (which cannot be « constructed ») but only that of potential infinity; for instance, by understanding the ordinal succession of natural numbers to be capable of continuation indefinitely in time. No enquiries that disregard considerations of a psychological nature such as these can penetrate to the essence of mathematics; and this applies not only to investigation of the fundamentals but also to other mathematical researches.

Mathematics at first is solely intuitive. Not until a second phase is it able to be clad in language with a view to communication and not until a third phase does logic come into play, being analysis of language.

Yet mathematics is independent of logic; or rather it is true that language and logic depend on mathematics. Besides, as everyone knows, mathematics needs the concept of infinity from the start whereas logic is concerned only with finite wholes.

It is natural, therefore, that formal logic should lose among mathematicians the boundless confidence it enjoys among logicians and formalists. Brouwer speaks of logical freedom as « an illusion ».

From this there follows the intuitionists' denial that there are no limits to the application of the principle of the excluded third (which implies that every mathematical problem is capable of solution) and consequently their rejection of proof by *reductio ad absurdum.*

The same Hilbert who in 1900 had announced the principle that every mathematical problem is soluble was obliged later to abandon this contention and to affirm instead that *the fundamental problem of mathematics is that of decision.*

In consequence of this the intuitionists are constructionists; that is to say, they accept nothing but effective construction as permitting the introduction of mathematical objects and as proofs of existence.

On this point Heyting has written as follows: « A mathematical theorem is the expression of a purely empirical fact, namely that a construction has succeeded. Thus « $2+ 2 = 3 + 1$ » is to be read as an abbreviation of the statement 'I have carried out the mental constructions indicated by $2 + 2$ and by $3 + 1$ and I have found that both lead to the same result' »[2].

Coherently with their own system, the intuitionists reject all reduction of mathematics to axioms on the ground that mathematics being a purely verbal construction has no true content and creative power of its own. They accept neither Cantor's intuitive theory of sets nor Zermelo's axiomatic theory from which, of course, they reject the axiom of infinite choices[1]. They develop their own theory of sets in which Cantor's platonic set-theory[2] is replaced by the concepts of *explanation* and *specification*. Here explanation is to be understood as accurate determination of the constructional scheme by means of which the elements of a set are generated, and specification as precise statement of the defining concept in isolation, achieved by formulating the characteristic property common to the entities in question[3].

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[1] Cfr FERDINANDO GONSETH: *Philosophie mathématique.* Hermann, Paris, 1936, p. 62.

[2] A. HEYTING: *Intuitionism. An introduction.* North-Holland, Amsterdam, 1946, p. 8.

On the basis of these concepts the intuitionists are able to introduce what is known as the « intuitive continuum », this being understood to mean the specification of unlimited convergent series of rational numbers or - to emphasize the circumstance that the intuitive continuum is not based on the concept of measurement - the specification of elements belonging to any suitably ordered elements[4].

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As the references given in footnotes under the text are brief and scanty the following Bibliography contains the titles of a few works in Italian which may be useful to readers wishing to acquire an introductory knowledge of the problem.

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[1] Cfr. Russell, op. cit., pp. 192 et seq.

[2] « By a set is meant a collection M, conceived as a whole, comprising clearly defined and specified objects of our intuition and thought (called elements) », in *Beiträge zur Begründung der transfiniten Mengenlehre*, Mathematische Annalen, 46(1895), p. 481.

[3] E. Casari, *op. cit.* pp. 202 ss.

[4] Cfr L. E. Brouwer: *Fondamenti storici, principi e metodi dell'intuizionismo*, in C. Cellucci, *op. cit.* pp. 223-23ff.

SOCIAL BEHAVIOR AND AVIAN SEXUAL CYCLES

The annual photoperiodic (solar) cycle is instrumental in controlling the annual sexual cycle of many birds of mid and high latitudes. Other broad-scale ecological factors often play a similar role near the equator. Nevertheless, such factors cannot account for the precise timing of breeding events observed within *many bird populations. Behavioral interactions among birds have often been implicated in this regard. It appears, from the analysis of the present paper, that « social behavior » is effective at various times and in various ways in facilitating the development, timing, and coordination of reproduction among birds. Social facilitation can operate over a much broader spectrum, temporally and mechanistically, than traditionally thought. Several types of mechanisms are apparent. These are defined and illustrated.*

ENVIRONMENTAL CONTROL OF AVIAN SEXUAL CYCLES. — Reproduction among birds is generally periodic (usually annual) in occurrence. And since birds lack perfect internal chronometers, timing mechanisms depend in part upon environmental information for their precision (Baker and Baker, 1936). At middle and high latitudes the annual cycle in daylength (photoperiod) is important in the constellation of environmental information that regulates the annual reproductive cycle of many birds (Farner, 1966, 1970a, b; Farner and Follett, 1966; Farner and Lewis, 1971; Lofts, Follett and Murton, 1970; Lofts and Murton, 1968). In equatorial and arid situations, on the other hand, ecological factors such as humidity, rainfall, and vegetational changes assume greater importance as primary timers of reproduction (e.g., Crook, 1964; Disney and Marshall, 1956; Immelmann, 1963b; Keast and Marshall, 1954; Lack, 1950; Marshall, 1970; Moreau, 1936, 1950a, b; Voous, 1950).

Seasonal (annual) changes in the tropics are complicated and often unpredictable. Consequently, breeding cycles among the various bird populations tend to be locally diverse (*e.g.*, Immelmann, 1963b, 1967; Miller, 1955, 1963; Moreau, 1966; Schreiber and Ashmole, 1970; Skutch, 1950; Thomson, 1950; Voous, (1950). However, even in those parts of the tropics where the daily photoperiod is constant or nearly so, and in which other aspects of the environment are often regarded as nearly constant and equable throughout the entire year, or in which the observed environmental changes are aperiodic, many (but by no means all) birds retain an annual breeding schedule (see especially Baker, Marshall and Harrison, 1940; Bates, 1908; Harris, 1970; Lack, 1950;

We are indebted to Professors C. W. Lewis, G. H. Orians, and F. Richardson for critiques of the manuscript and to A. B. Orcutt who generously helped prepare and edit the manuscript.

Previously unpublished data on *Zonotrichia* referred to in this communication derive from investigations supported by the National Science Foundation through grants GB-5969X and GB-11905 (Professor D. S. Farner, principal investigator).

Miller, 1955; Moreau, 1950a, b; Schreiber and Ashmole, 1970; Voous, 1950). Sources of environmental information serving as primary annual timers under such circumstances are poorly known, but are probably diverse (see Harris, 1970; Immelmann, 1963a, b, 1966; Marshall, 1959, 1970; Moreau, 1966; Nisbet, 1970; Skutch, 1950); Moreau (1936) suggested that in certain equatorial environments where seasonal changes are of limited magnitude, exogenous control of the breeding calendar may result from the summation of several environmental influences. Light effects (both direct and indirect) cannot at this time be excluded from the battery of information that controls the timing of reproduction at relatively low latitudes (Immelmann, 1963b; Marshall, 1970; Nisbet, 1970). Furthermore, it is evident that in any locality where even a few resident populations exhibit, on the average, a precisely annual (or some harmonic of an annual) reproductive periodicity, the extrapopulational environment is, in fact, annually periodic. In such cases, the so-called « constant-equatorial environment » which is so frequently reported is a fiction resulting from subjective human experience and a lack of detailed long-term quantitative information on the locally significant variables.

Even at higher latitudes, photoperiodic effects assuredly do not fully account for the synchrony observed *within* populations of breeding birds such as that reported for colonies of cliff swallows, *Petrochelidon pyrrhonota* (Emlen, 1952), purple martins, *Progne subis* (Allen and Nice, 1952), house sparrows, *Passer domesticus* (Summers-Smith, 1963), and many others. In such situations of local synchrony other local factors presumably play a role. Furthermore, in species in which « full » testicular development and sperm production can be stimulated solely by photostimulation, vitellogenesis and egg-laying are not usually so induced (Farner and Lewis, 1971; Lofts, Follett and Murton, 1970), thus implicating other factors as being of importance in the physiological preparation that precedes breeding. Cues other than photoperiod must also be important where breeding is erratic as in the red crossbill, *Loxia curvirostra* (Tordoff and Dawson, 1965), and in birds of arid regions as well as other opportunistic breeders (see Immelmann, 1963b; Marshall, 1970; Orians, 1961), and in brood parasites (*i.e.*, birds that lay eggs in the nests of other species) (see McGeen and McGeen, 1968).

While mechanisms differ among species, it is the female who determines the precise time of breeding, largely through mate selection and sensitivity to the microenvironment during the terminal stages of ovarian maturation (see Farner and Lewis, 1971). Perrins (1970) presents arguments that in females of some species the availability of food acts as the final determinant governing the timing of egg-laying. There is widespread support for such a view (*e.g.*, Harris, 1969a, b, c; Lack, 1954, 1966; Schreiber and Ashmole, 1970). However, detailed quantitative data are lacking for all but a few species (Lack, 1954, 1966; Perrins, 1970) and there is little evidence suggesting that food *per se* does in fact have a stimulus function in the timing of reproduction but see Payne, 1969. Nevertheless, it may sometimes act as a factor limiting the later stages of ovarian development. The early stages of ovarian development in white-crowned sparrows, *Zonotrichia leucophrys*, are relatively unaffected by plane of nutrition (Lewis and Gwinner, unpublished data).

Present theory suggests, then, that the timing of any perfectly periodic function of a biotic population is conferred by a stimulus or an entrainment function (« Zeitgeber ») which is external to the population; intrapopulational interactions and other modifying factors may act to mediate and modulate effects of such a primary environmental timer.

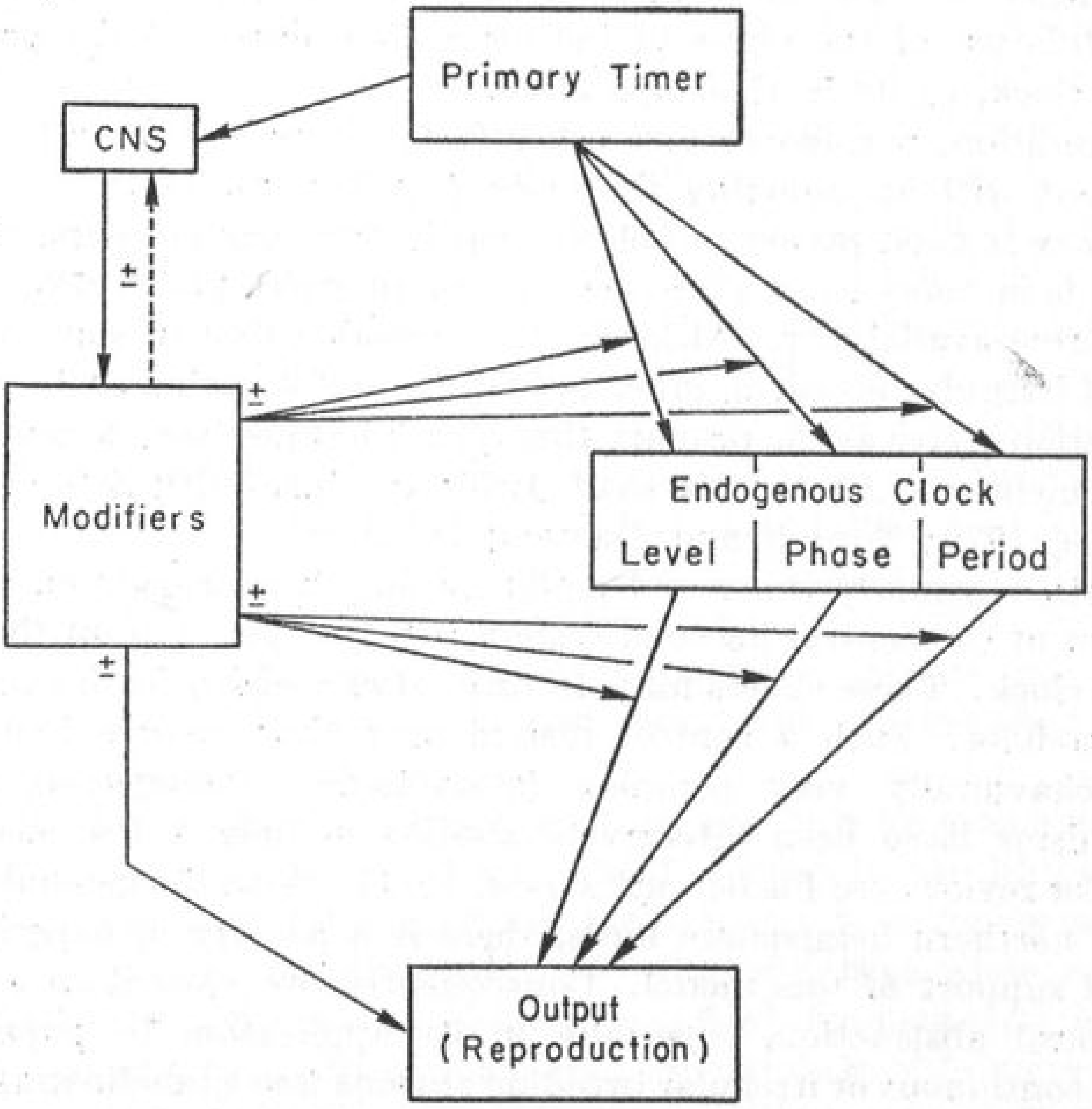

Fig. 1

A hypothetical model of environmental control of reproductive timing in birds. See accompanying text.

A simplified hypothetical model of exogenous control of periodic breeding in birds, which is applicable also to many instances of aperiodic breeding involving environmental entrainment or coercion (see Marshall, 1970), is presented in Figure 1. This model assumes dependence upon a single endogenous clock or oscillator having three main variable parameters: level, phase, and period[1]. Presumably an extrapopulational primary timer could work by affecting one or any combination of these parameters, thus altering the outputs of the clock (*e.g.*, reproductive functions). But in most species additional factors may modify the timing of reproductive cycles. These modifiers may include social behavior, ontogenetic factors, experience, resource availability, temperature, and various geophysical and ecological factors. Furthermore, modifiers can be either stimulatory or inhibitory. Theoretically, the timing of sexual cycles can be influenced by modifiers in any of several ways:

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[1] The level is the arithmetic mean of all instantaneous values of the clock (oscillation) per period. The phase is the instantaneous state of the oscillation within a period as defined by the value of any function and all of its time derivatives. The period is the interval of time between reoccurrences of a definite phase of the oscillation. See also, Aschoff (1965, 1967).

a) alteration of individual perception of the primary timer.
b) alteration of individual responses to the primary timer.
c) in either of these cases (a, b) the outputs may be altered through modulation of the phase of the clock (entrainment,) the period of the clock, or its level of operation.
d) in addition, permissive and other factors may act directly on the output without affecting the clock (see Perrins, 1970).

There is good reason to believe that in irregular breeders, primary control is in many cases exercised via extrapopulational factors related to resource availability. Although it is possible that in some populations of irregular breeders, intrapopulational information such as social stimulation serves as the primary timer, such has not been demonstrated (see Immelmann, 1963b; Marshall, 1970; Perrins, 1970; Schreiber and Ashmole, 1970; Tordoff and Dawson, 1965).

Both a primary timer and modifiers may have effects on « higher centers » of the central nervous system (CNS) other than on the endogenous clock. These effects may, in turn, alter social behavior that acts as a modifier. Such a control system may thus involve temporally and behaviorally very complex interactions. Endogenous (clock) mechanisms have been intensively studied in only a few species of birds (for review, see Farner and Lewis, 1971). With the general exception of northern hemisphere birds, there is a paucity of experimental data in support of this model. Consequently, we view it as a highly conjectural abstraction, especially in its application to populations having continuous or irregular breeding seasons (see Immelmann, 1963a, b, 1966; Lofts and Murton, 1968; Marshall, 1959, 1970; and others).

While the literature of bird behavior and reproduction is extensive, our knowledge of behavioral mechanisms in the timing of reproduction is presently much less advanced and precise than our knowledge of purely physiological mechanisms. Nevertheless, as will be seen, social patterns are known to operate importantly in modifying or coordinating the timing of reproductive activities of birds.

HISTORICAL PERSPECTIVE. — F. Fraser Darling (1938), in an essay entitled « Bird Flocks and the Breeding Cycle », set forth the thesis that « ...the social group and its magnitude, in birds which are gregarious at the breeding season, are themselves exteroceptive factors in the development and synchronization of reproductive condition in the members of individual pairs of birds and throughout the flock ». In view of the great heuristic value of Darling's hypothesis, his work and that which it inspired provides a proper setting for any inquiry into the topic of « sociality » and its « role » in the timing of avian sexual cycles.

In examining this hypothesis, Darling drew largely on data from his own observations of breeding flocks of herring gulls, *Larus argentatus*, and of lesser black-backed gulls, *L. fuscus*, including occasional observations of communal courtship. He concluded that breeding activities, at least in these two gull species, are directly related to colony size. He further inferred that a minimum population is necessary if breeding is to take place, but that at population levels above this « threshold », increasing colony size results in increasing synchronization of breeding activities as well as greater reproductive success.

Though its validity is often taken for granted, Darling's hypothesis as originally stated has not found strong support. Indeed, the work of Haartman (1945) who demonstrated that Darling's original data were statistically non-significant, strongly undermines its empirical base.

In his original essay (1938), Darling made some interesting speculations regarding colonization of the Atlantic fulmar, *Fulmarus glacialis glacialis*, in Britain. Prior to 1878 this petrel was known to nest in the British Isles only on the island of St. Kilda. Here the bird served as a food staple and export of the human population. Beginning about 1878 these birds began to colonize other nearby islands and by 1897 had reached the Scottish mainland. The species then spread rapidly down the eastern coast.

Darling hypothesized that predation pressure by man since 1870 may have decreased, allowing an annual surplus in the bird-crop on St. Kilda, and that some of these birds began to colonize adjacent islands and finally the mainland. He observed that when a colony is newly formed, the site is visited for a few summers, but breeding does not take place. During this period the numbers of fulmars visiting the new station gradually increase until breeding occurs. This observation suggested to Darling that there is a threshold of numbers below which breeding does not take place. Occasional small increases in population that may have occurred while under predation pressure would not be sufficient to supply a large enough initial stock for founding a new breeding colony. Once this pressure relaxes, above-threshold numbers were available and accounted for the phenomenal increase of the bird after 1878.

Fisher and Waterston (1941) brought together and analyzed the available data on the biology of the fulmar. According to these investigators, the extension of the fulmars' breeding range in Britain cannot be attributed to its relation with man on St. Kilda, but is « due to a biological change which is not understood ». They found that fewer eggs or young are produced per breeding pair in small colonies than in the larger ones. In the larger colonies, birds annually arrive a month earlier than in small breeding colonies and may arrive two months or more earlier than in non-breeding colonies. In general, egg-laying takes place earlier in the larger colonies and over a shorter period of time than in the smaller colonies, and members of non-breeding colonies normally depart at a time when birds at breeding colonies are still nesting. Fisher and Waterston claim that the social stimulus of numbers in a colony enables the fulmars to go through their full breeding cycle. Evidence of the relationship, if any, between the changes in their distribution and food supply is lacking.

David Lack (1943) noted that while Fisher and Waterston's results (1941) « ...are consistent with Darling's views, they can be equally well accounted for on the view that new colonies of fulmars are founded by young birds, that the older birds tend to return each year to the colony in which they first nested, and, perhaps, that the older birds breed more often. This alternative explanation is added, but rather as an afterthought, by the authors. Darling's views ... have much in their favor, but it does harm rather than good to a new theory to interpret in terms of it a series of facts which are capable of a simpler alternative explanation ». Fisher (1954) was fully aware that effects due to colony size were not clearly separated from those due to differences in age distribution among the colonies studied.

Recent Studies of Natural Populations. — Among the most exacting studies to date of the « Darling Effect » are those of Coulson and White (1956, 1958, 1960) and of Coulson (1966). These investigators clearly separated the factor of density from that of age distribution in the case of several colonies of the kittiwake, *Rissa tridactyla*. Social « stimulation » was observed to operate in these colonies, but its expression differed from that postulated by Darling.

Coulson and White's early study (1956) gave evidence of marked variations in the timing of three breeding phenomena within the eight kittiwake colonies investigated: 1) the time at which birds returned to the colonies annually, 2) the time at which breeding began, and 3) the « spread of breeding » (duration of a colony's egg-laying period). Their data indicated that all three of these parameters are influenced both by colony size *and* by age structure.

Coulson and White learned that birds visited the site of formation of a new colony during the year preceding initial breeding, and that immigration continued for at least the first four years following the establishment of a colony. Adult kittiwakes exhibit both colony- and nest-site tenacity. Non-breeding birds were present in a large proportion of all colonies, but were present in relatively higher numbers in the younger colonies. Non-breeding individuals returned to colonies later than did breeding birds, the majority of the non-breeders being young birds returning for the first time. Colony age and size were closely correlated. Both return to the colony and time of onset of breeding occurred earliest in older colonies and latest in the younger colonies. Birds of the same age class bred at the same time regardless of colony size, and the spread in onset of breeding within large colonies was *greater* than in small ones. Coulson and White (1956) say the reverse « would be expected if 'social stimulation' were of major importance ».

The authors further conclude (1956) that the age of birds within a colony is more important than « social stimulation » in influencing the time of breeding. Implicit in these conclusions is the assumption (perhaps « borrowed » from Darling) that sociality can only importantly influence reproduction at, or near, the time of breeding. Coulson and White thus failed to recognize *at this time* (1956) a potentially very important factor, namely that differences in age distributions between bird colonies also reflect differences both in breeding and social experience among these colonies. However, in 1960 Coulson and White inferred that the physiological state within a bird that determines the time of breeding already exists in large part before arrival on the breeding grounds. They thought that the role of social stimulation in producing this state probably could not be operative during winter since the colony is not maintained as a distinct flock in the non-breeding season. The authors therefore inferred that effects of social stimulation from the past breeding season(s) can accumulate and affect time of breeding in later years.

Coulson and White's original paper thus gives evidence that while they were greatly stimulated by the « Darling hypothesis », they were also initially conceptually restricted by certain of the assumptions on which it is based. In pursuing the problem, however, the weight of accumulating evidence caused them to assume the view (1960) that the onset, spread, and completion of breeding within the kittiwake colonies can be explained primarily in terms of density. Furthermore, they demonstrated that maximum density, range in density, and the presence of low-density areas (which occurred in all colonies) were all significant influences on the time-course of reproduction. They concluded (1960) that the « social stimulation » that apparently operates in kittiwake colonies differs from that conceived by Darling.

Investigation of reproduction in the kittiwake was further pursued by Coulson (1966) in order to ascertain the effects of age and breeding experience on the breeding biology of these birds. Coulson's major finding was that the reproductive performance, including time of egg-laying, of female kittiwakes can be greatly influenced by a change of mates for at least two breeding seasons following such a change. This conclusion supports Coulson and White's conjecture (1960) that social experiences during one season influence the time of breeding in following years.

Hailman (1964) reports no « Darling Effect » or breeding synchrony at the colony level in the swallow-tailed gull, *Creagrus furcatus*, a non-annual breeder, in the Galapagos Islands. However, there exist within a colony natural groupings in which there is statistically significant local breeding synchrony. Hailman claims that this synchrony « is not attributable to environmental determinants ». He uses this doubtful inference to conclude that synchrony must therefore be a result of the social environment. Indeed, Snow and Snow (1967), while agreeing that social stimulation plays an important role, point out that physical conditions were locally variable and could be correlated with breeding activity in different parts of the colony. Harris (1970) confirmed Hailman's data showing local synchrony and emphasized that the synchronous sub-colonies are often out of phase with each other. Furthermore, peaks of breeding can be associated with a whole colony in spite of differences existing within the colony, but breeding peaks of one colony are usually unrelated to those of colonies on the other islands. Harris speculates, « There can be little doubt that the varied and noisy displays of the gulls bring about this synchrony... », and he suggests that the synchrony aids in reducing predation and cleptoparasitism by frigate-birds.

The breeding synchrony of the gannet, *Sula bassana*, is strongly correlated with colony density (Nelson, 1966). Nelson suggests that the synchronization is facilitated by social behavior within the colony. However, he reaches this conclusion by the same method used by most other investigators of this problem to date, *viz.*, it is the most logical explanation remaining after other possible factors have been rationalized away. There are still no data that directly support Nelson's contention.

One preliminary approach in trying to answer this criticism has been to find and describe one or more behaviors (*e.g.*, communal displays — often precopulatory or copulatory behavior) in synchronous breeders that are performed by many individuals simultaneously. Then when this group of birds begins egg-laying all within a short period of time, the behavior may be suspected of playing some role in adjusting the timing of laying in one pair in relation to that of neighboring birds. For example, copulations in the lesser black-backed gull begin about 30 days before egg-laying and continue until about five days following laying (Brown, 1967). Thus copulation in this species probably has functions in addition to the fundamental one of fertilization. Brown argues that copulation may serve as a physical stimulus to the female, which induces the final stages of ovary growth and follicle development. He further presents some data in support of his vivid field impressions that copulation is a highly synchronized behavior in the colony with many pairs usually performing simultaneously. Thus he concludes that copulation *may* serve as a social stimulus resulting in breeding synchronization of the colony. Also, Brown and Urban (1969) describe a « group display » in the great white pelican, *Pelecanus onocrotalus roseus*, in which small (20 - 200 - bird) groups participate. The display is followed almost immediately by the whole group moving from the display area to the nesting area. Birds in this group will show a high degree of synchrony in time of egg-laying but may differ considerably from other groups of birds within the same large colony. Summers-Smith (1963) suggests that the highly mimetic « communal displays » and copulation in the house sparrow may be an important social factor in achieving high reproductive synchrony in colonies.

In many cyclic breeders such as migrants that breed at middle and high latitudes, there is little opportunity for social factors on the breeding grounds to alter the timing of reproduction. Indeed, the only alternative to breeding « on time » may be non-breeding (Farner and Lewis, 1971). In such cases, the time of breeding may be largely determined prior to arrival on the breeding grounds. Concerning such populations, we infer that observed breeding synchrony is largely conferred by virtue of the fact that all members of a population may arrive on the breeding grounds at very nearly the same time (the sexes may arrive separately, the males in advance of the females) and in very similar physiologic and behavioral states, ready to respond at once to the local ecological complex. In such cases, premigratory and migratory flocking behavior undoubtedly contributes to nesting synchrony (though perhaps sometimes passively through « social attraction ») within populations of both territorial and colonial nesters. Thus social behavior during late winter and spring may be much more important in establishing breeding synchrony in such species than social interactions on the breeding grounds. Furthermore, synchronization of the male and female cycle may, in some species, take place at the behavioral rather than the physiological level. That is to say, the gonadal cycles (especially of males) may be a strong, rather inflexible function of environmental information (*e.g.*, photoperiod) which is quite independent of the social milieu at the onset of breeding (see Farner and Lewis, 1971; Lewis and Gwinner, unpublished data; and see below).

Barry (1962) observed that brant, *Branta bernicla hrota*, breeding at Southampton Island, Northwest Territories, were already paired upon arrival and provided good evidence that copulation takes place prior to arrival on the breeding grounds. Ovaries of newly arrived females are nearing culmination, and if conditions do not allow egg-laying to begin within 7 - 10 days following arrival there is a rapid decline in average clutch size. Also, Ross' goose, *Anser rossii*, lays within five days of arrival at breeding grounds in the Perry River region, Northwest Territories (Ryder, 1967). The females thus ovulate at about the time of arrival on the breeding grounds or very soon thereafter. Furthermore, copulation, vitellogenesis, and the culminative stages of ovarian development must be initiated before arrival, and the bioenergetic support of the early phases of the nesting cycle depend upon stored reserves rather than on local food abundance at the time of arrival. Similar situations seem to obtain for blue and snow geese, *Chen caerulescens* and *C. hyperborea*, breeding at similar latitudes (Barry, 1962; Lemieux, 1959).

Also, the time of laying in many species is known to be in part a function of the age and/or experience of the female, the older hens usually laying significantly earlier than the younger ones (see Coulson and White, 1956; Fisher, 1969; Nelson, 1966; Perrins, 1965, 1970). The question of which effects of differing age distribution among bird populations are largely maturational and which largely experiential has not been addressed and may be of great significance. Until this problem has been explored, it is not possible to recognize a dichotomy between age structure and social structure of a natural population of birds.

THE « FRASER DARLING EFFECT »: REDEFINITION. — Darling's hypothesis captured the minds of a generation of reproductive biologists and ethologists. Nevertheless, the hypothesis as originally conceived (1938) and its extension to birds that are territorial at the breeding season (1952)[1] has received limited support. However, it is evident that social interactions are of considerable importance in the timing and integration of reproduction in birds. Furthermore, the role of social behavior in the timing of reproduction is extremely varied.

Retrospectively, we see that shortcomings of the hypothesis include inattention to the nature and possible roles of physiological, developmental, experiential, and evolutionary functions which may be partly responsible for or limit the timing and sequence of events in the sexual cycle. Furthermore, social information may be expected to operate through mechanisms appropriate to the adaptive strategy of the population. Thus, if the evolved strategy is to reproduce as soon as possible, one can expect social adaptations that facilitate rapid culmination of reproductive development and onset of breeding. Unfortunately, the temporal patterns of reproduction within nesting cycles have not been seriously subjected to functional analysis. Clearly, however, such patterns of reproduction among birds are diverse (see especially Lack, 1950, 1954, 1966; Lofts and Murton, 1968; Schreiber and Ashmole, 1970), suggesting that selective factors and consequent strategies of evolved systems may also be diverse. One may expect social or other intrapopulational (or local) factors to have accelerating, decelerating, synchronizing, or desynchronizing effects within a given nesting cycle.

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[1] In 1952 Darling extended his original hypothesis based on colonial breeders to other species that hold « separate, fairly extensive breeding territories » by postulating that a territory serves primarily to afford a periphery of contact between pairs. This, according to Darling, results in greater opportunity for social stimulation. He further states, « If this hypothesis is correct, then *territorial behavior as a whole is a social phenomenon...* » (Darling's italics).

In the literature of social facilitation, breeding synchrony is a term that is frequently used, but seldom defined (but see Aschoff, 1958; Immelmann, 1963b). A perfectly general definition of synchronization is given by Aschoff, Klotter and Wever (1965): the « state in which two or more oscillations have the same frequency due to mutual or unilateral influences ». With regard to breeding cycles, however, it is important to recognize two types of synchrony. Operationally defined, breeding synchrony can be intrapopulational, in which case it is the maintenance of a constant phase-relationship among individuals within the breeding population; on the other hand, synchrony can be the maintenance of a constant phase-relationship between the individual breeding birds within the population and an extrapopulational (environmental) oscillator such as the annual photoperiodic cycle. In either case, the observed pattern of breeding might be expected to be at least approximately normally distributed with respect to time, assuming the effects of individuals to be additive. We would not, in any event, be able to distinguish the two types of synchrony on the basis of the breeding frequency distribution of the population. If local modifiers are effective, then either « local synchrony » (see Orians, 1961; Hailman, 1964) would be observed or the distribution would be skewed. Local synchrony can also be simply a function of « social attraction » (*op. cit.*). Similarly, skewness can derive from heterogeneity (genetic, ontogenetic, or experiential) within the population. It is thus clear that the elucidation and identification of mechanisms underlying the distribution of breeding functions require rather sophisticated quantitative field and laboratory investigations. Such investigations are costly and often impractical.

The population of yellow-eyed penguins, *Megadyptes antipodes*, studied by Richdale (1957) for a period of 18 years seemingly exhibits environmental, but not intrapopulational synchrony as measured by the initiation of egg-laying. That is to say, the variance in the onset of egg-laying was greater within the colony each year (average standard deviation = 5.02 days) than was the variance in the average annual mean date of lay of the first egg (standard deviation = 2.12 days) (see table 7, Richdale, 1957). Moreover, the success of both early and late nesters was comparable.

« Social stimulation » was conceived by Darling to operate near the time of reproduction as a function of colony size to synchronize events within the colony. However, even if one equates sociality with coloniality, it is evident that social interactions are complex and vary both quantitatively and qualitatively with successive stages of the reproductive cycle (see Lehrman, 1961). Hence, if one dares generalize, social stimulation of reproduction may be expected to be a subtle and complex function of density (and perhaps of colony size).

Darling's concept of a lower threshold of numbers below which successful breeding « cannot » take place is doubtful in most instances (see also Lack, 1954). Fisher (1954) doubted that Darling's data supported the « threshold » hypothesis, and he suggested that the lower breeding efficiency of small colonies derives from the relatively larger proportion of young birds in these colonies (the « young-pioneer » theory). It is true, however, that a single pair of budgerigars, *Melopsittacus undulatus*, (a highly social, colonial species) does not generally breed (Brockway, 1964a; see also Darling, 1938); a lower limiting threshold of two pairs seems to be essential to successful reproduction in this species. Also, Hall (1970) observes that in the black-headed weaver, *Ploceus cucullatus*, there is probably a minimum size colony below which breeding is unlikely to occur. The possibility that very small colonies are likely to be located in ecologically marginal areas has been given little attention (Orians, pers. comm.).

One could also postulate an upper limiting value of density or of numbers at which reproduction among colonial birds ceases to be successful. For example, Collias and Jahn (1959) in a study of social behavior and breeding success in Canada geese, *Branta canadensis*, found that in dense breeding colonies, productivity is reduced. In 1952 at Horicon Marsh, Wisconsin, they observed that about 70 per cent of the potential reproductive increase was lost. This loss was attributed to the failure of birds to pair effectively, to failure of paired females to lay and incubate eggs, and to loss of clutches once laid, all primarily as a function of territorial behavior and dominance relationships. There are respectable data indicating that the social interactions (often termed « conflict ») observed where birds are closely grouped, have a repressive effect on reproduction (see, for example, Flickinger, 1966; Kortlandt, 1948; Lack, 1940, Slater, 1969).

In view of the foregoing considerations, few today would accept Darling's hypothesis as originally proposed. However, restated as follows, we believe that Darling's hypothesis has currency:

> Birds are, themselves, exteroceptive factors to which other birds respond. One expression of the total response pattern is the facilitation[1] of events of the reproductive cycle.

Thus formulated, the hypothesis is less restrictive (though unfortunately more vague). For example, we may entertain the possibility that social stimuli may be effective in one way or another at any time of the year — not just at the breeding season. Furthermore, social stimuli operating during one part of the sexual cycle may conceivably have important effects during later stages of the cycle, and social experiences during one year may have carry-over effects which may influence later cycles. Similarly, social stimuli which are present at one ontogenetic stage of development might influence performance at later stages. Finally, in some birds at least, the transfer of information from one generation to the next directly by social means (tradition) cannot be discounted (Välikangas, 1933; McCabe, 1947).

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[1] Our view of facilitation is, in the present context, necessarily more general than that usually employed by behaviorists (see, for example, Thorpe, 1956; Zajonc, 1965). Herein any effect of numbers upon the timing or specific performance of a behavior is termed «social facilitation».

Clearly, if the hypothesis is to be useful, we must have an operational definition of « facilitation ». We define as facilitating any function that increases reproductive success. While biologically acceptable (see Crook, 1964; Immelmann, 1963b; Lack, 1954; Orians, 1961), this definition leaves us with the task of finding useful and valid criteria of reproductive success. This presents exceedingly difficult problems, not all of which may be discussed here (but see Lack, 1954, 1966; Wynne-Edwards, 1962). Suffice it to say that we may expect that the character and expression of mechanisms which enhance breeding success, together with the parameters of breeding success, will be strong functions of the genetic and phenotypic constitution of any single population of birds, and also of the selection pressures of the particular ecological situation occupied by the population. In other words, « facilitation » might be variously expressed in different species and also within the same species at different times and in different ecological situations.

If we assume that sociality (where it persistently occurs) is adaptive, then alterations in timing, sequence, or specific performance of reproductive events which may be observed as a function of social behavior are properly termed social facilitation. Darling (1938) and Fisher and Waterston (1941) proceeded, however, under the assumption that « synchronization of reproductive condition » within a population is necessarily adaptive; they viewed « social stimulation » simply as a synchronizing function. Recall that this assumption led Coulson and White to conclude initially (1956) that social stimulation was not of major importance in timing the breeding of kittiwakes.

The adaptive importance of synchronization of breeding events within a population may be a very different matter from the coordination of events between two members of a breeding pair. The latter is most essential to reproductive success. Certainly it is true that reproduction in most species of birds is restricted to « favorable seasons »; but within limits that vary with the species, the climate, the latitude, and the length of the « favorable season », desynchronization of breeding may favor breeding success (*e.g.*, because of more favorable distribution of resources).

If one takes the view of social facilitation expressed above, no contradiction is seen in the apparent operation of sociality to synchronize the events of the breeding season within one population (or species) and to desynchronize the events of the cycle in another. We are led, rather, to an exciting question: why does synchronization appear to be adaptive in one situation while the opposite tendency seems to be adaptive in another, perhaps related, situation?

The generalization that social behavior increases reproductive success cannot be assumed without some risk. Unless a social group is regulated at levels below the limit imposed by resource availability, social competition will offset some of the advantages of social organization (see also Lack, 1954, 1966; Perrins, 1970). This situation probably obtains in the previously mentioned observations of Collias and Jahn (1959) indicating reduced reproductive success at high population levels of Canada geese.

The operational criteria of facilitation appear to be of four types:

1) changes in the time of occurrence of an event (or of related events) in the sexual cycle.
2) changes in the time course of one or more events of the cycle.
3) alterations in sequence of events (these include failure of specific events to occur).
4) alterations in specific performance (*e.g.*, clutch size, hatchability, or number of young successfully fledged)[1].

(Such effects presumably may or may not produce or be associated with criteria of types one, two, and three).

Social nesting, especially where highly synchronous, may result in very high prey (*i.e.*, eggs, young, and to a lesser degree the attending parents) densities. This may effect an increase in predation pressure as a function of increased reproductive or survival rates of predators (see Hassell, 1966). Increased prey density as a function of colonial nesting and breeding synchrony may be inadequately relieved by the development of inadequate anti-predator reactions (*e.g.*, Kruuk, 1964). In the face of significant predation pressure, it seems to us that intra-populational breeding synchrony as a function of social stimulation would be unlikely to evolve unless effective anti-predator mechanisms also evolve (see Kruuk, 1964).

It seems reasonable to infer that breeding synchrony can potentially increase effective predation pressure, result in food shortage, increase the risk of epidemic disease, and effectively reduce available space, all of which are density-dependent and may interact complexly. Further conjecture is fruitless at this time; temporal dispersion of nesting birds as a function of resource distribution and predation pressure requires the same careful study and analysis that spatial dispersion is now receiving. Cullen (1960) and Horn (1969) are particularly instructive on this point.

PATTERNS OF SOCIAL FACILITATION. — We have seen that social factors may operate in different ways and at different times to produce effects of varying character which may (with reservation) be termed facilitating. It is therefore useful to classify the patterns of social facilitation, thereby permitting examination of retrospective data, and the design of new experiments with the anticipation of eventually establishing the degree to which avian social systems have exploited the « space » represented by our fundamental hypothesis.

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[1] While we are primarily concerned with the timing of events, considerations of this type are important, especially where they occur in association with criteria of the first three types, since they are likely to offer data relating to the adaptiveness of the social system under consideration. Furthermore, the problem of reproductive timing cannot be dissociated from the broader problems of the regulation of reproductive rates and the control of population size (see especially Lack, 1954, 1966; Wynne-Edwards, 1962).

The classification employed here derives from considerations of earlier sections of this paper and is designed only to serve as a tool in pursuing the present inquiry[1]:

1) *Preparatory mechanisms.* These are effective over some period of time preceding the « event » in question, and may, for example, operate in symphony with other environmental factors to bring the population or pair into some essential physiological or behavioral state. These may be of annual, seasonal, singular (*e.g.*, ontogenetic), or of irregular occurrence.
2) *Induction mechanisms* (or « releasers »). These elicit a specific « event » and will, in general, be effective « immediately » prior to the event. Such mechanisms might operate relatively independently of factors of type « 1) » except insofar as they function effectively only when the bird is in some particular physiological state or has some particular behavioral orientation.
3) *Augmentation mechanisms.* Presumably some mechanisms might act to accelerate, synchronize, or coordinate the performance of some « event » in members which are socially oriented toward each other or mutually engaged in some function. Augmentation mechanisms may complement induction mechanisms, but are not essential to induction.

This classification suffers from certain potential weaknesses which should be recognized. For example, the categories (with respect to realized mechanisms) may not be mutually exclusive. Furthermore, mechanisms having strong interaction terms may be encountered; thus, we may find « induction mechanisms » which function at « intensities » above threshold as augmentation systems. The classification, moreover, offers no particular insight into the specific expression of these mechanisms in natural settings, nor of the factors (other than those specifically considered) which may alter their expression. Nevertheless, if these weaknesses are recognized, the classification affords a valid basis whereby the question of social facilitation may be approached.

PREPARATORY MECHANISMS. — It has become increasingly clear over the past few years that the individual experience of an animal plays an important role in the development of many behavioral patterns (Lehrman, 1956, 1964; Schneirla, 1956; Schneirla and Rosenblatt, 1961). In many vertebrates, the sexual behavior patterns of adults are partly dependent upon the experience which the animals had before they were sexually mature (*e.g.*, Rosenblatt and Aronson, 1958; Valenstein and Young, 1955). Beach (1947), in a now classical review, pointed out that reproductive patterns of various mammals are influenced in both their development and expression by individual experience. Collias (1952), reviewing the development of social behavior in birds, emphasized the probable pervasive influence of their early sociality. The anatomic and physiologic machinery underlying social mechanisms is rather well developed before hatching in precocial species (Gottlieb, 1968), and patterns of socialization in the experience of the young bird both before and after hatching may contribute to the behavioral patterns (sexual and otherwise) of the adult birds. Schein and Hale (1959) demonstrated that early social experience may greatly modify the responsiveness of male domestic turkeys, *Meleagris gallopavo*, to specific sexual stimuli. Collias (*op. cit.*) calls attention to the fact that the results of social development are « individuals and groups that respond adaptively to the habitat, habits, and recognition marks of the species, and are therefore ecologically adjusted ».

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[1] It is important to note that inhibitory as well as stimulatory mechanisms may be involved in all three categories.

Of minor note are recent studies that give direct evidence of social information operating in early ontogenetic stages to influence the breeding phenology of birds. Vince (1964, 1966, 1968a, b) has obtained data that indicate that the synchronization of hatching in the bobwhite quail, *Colinus virginianus*, and the Japanese quail, *Coturnix coturnix japonica*, is influenced by the acceleration of development of retarded embryos through contact with more advanced members of a clutch while the development of more advanced embryos may be retarded through contact with clutchmates (see also Gottlieb, 1968).

Daniel S. Lehrman and colleagues at the Institute of Animal Behavior at Rutgers University are intensively exploring the role of experience in the development of reproductive and parental behavior in the ring dove, *Streptopelia risoria.* Experience has been shown to operate dramatically to facilitate the development of hormone-induced behavior patterns in this bird. Lehrman and Wortis (1960) have demonstrated that incubation behavior is induced much more rapidly following the administration of progesterone in birds with previous breeding experience than in näive birds. These results provide evidence that, in this species, previous experience in a social setting is one of the factors conditioning behavioral responses. Lehrman and Wortis (1967) found that breeding efficiency of experienced pairs of ring doves was superior to that of inexperienced pairs of similar age and physiologic condition. The reproductive performance of all birds was essentially similar following an initial cycle.

Experience in ring doves, and probably many other birds as well, operates in yet another way. If previous breeding experience is facilitating, a rather unsettling question remains: How does a bird breeding for the first time manage to do things appropriately? According to one school of thought (see Lehrman, 1964), it seems that experienced ring doves are initially oriented toward appropriate stimuli, whereas the behavior of näive birds may be directed by « indirect » cues which follow in an orderly or integrated sequence. For example, when a ring dove builds a nest, she becomes attached to the nest-site and remains there much of the time. She is thus gaining « experience » by means of which she becomes oriented to the nest in such a way that she is most likely to lay the egg in the nest. Furthermore, she comes into contact with the egg after it appears even without intending to do so. Sitting on the eggs, she becomes attached to the nest-site even more. When the eggs hatch, she unavoidably comes into contact with the newly hatched squabs. Thus, experience during the early part of the nesting cycle influences the timing and sequence of events later in the same cycle. An alternative point of view (see Eibl-Eibesfeldt, 1970) regarding the ring dove's performance during the initial cycle relies more heavily on instinctive responses of the bird. According to this view, stimuli associated with the nest and young will « release » in the bird appropriate behaviors that are genetically programmed. In any event, participation in one entire cycle does render a pair of ring doves more efficient in subsequent cycles (Lehrman and Wortis, 1967).

From the foregoing discussion, we infer that the behavioral orientation of a bird and consequent influence on details of the reproductive cycle may derive in part from previous experience of a specific behavior, from experiences gained during ontogeny, from direct experiences gained during the course of one cycle, and from traditional information. Social factors may also modify the physiological state of a bird, thus influencing the threshold of response to appropriate stimuli and perhaps the intensity of response as well. Data favoring this possibility are mostly indirect. Nevertheless, it is known for a number of birds that the rate of gonadal development elicited by photic or other exteroceptice stimuli is enhanced by certain types of individual interactions.

Photostimulation alone will not in general produce full ovarian development even in bird species in which the spermatogenic response is strong. It may also be true that the wide variety of ecological factors that time the cycle near the equator similarly stimulate the male to full reproductive activity while having a much weaker effect on the female (*e.g.*, Keast and Marshall, 1954). In general, the factors that act to produce complete development of the ovary in such birds and to trigger ovulation are not known. Polikarpova (1940) found, however, that the maturation of the ovary of the house sparrow is partly stimulated by the presence of an « active » male, but the presence of the nest is essential to « complete » maturation and ovulation. Similarly, Hinde and Warren (1959) found that in the canary, *Serinus canarius*, nest-building activities and the presence of a completed nest are essential to egg-laying. The work of Lehrman on the induction of ovulation in the ring dove has yielded similar, but more quantitative results.

Generally, it should be advantageous if the final stages of maturation of the ovary are stimulated by appropriate factors operating at or near the site where the first egg will be deposited. Thus it would seem that while a breeding cycle might already have been initiated, selection would heavily favor mechanisms that would either stop or continue the cycle depending on local conditions before the female makes the great expenditure of energy (Drent, 1967; El-Wailly, 1966; Kendeigh, 1963; Perrins, 1970) involved in the later stages of ovarian maturation and in incubation.

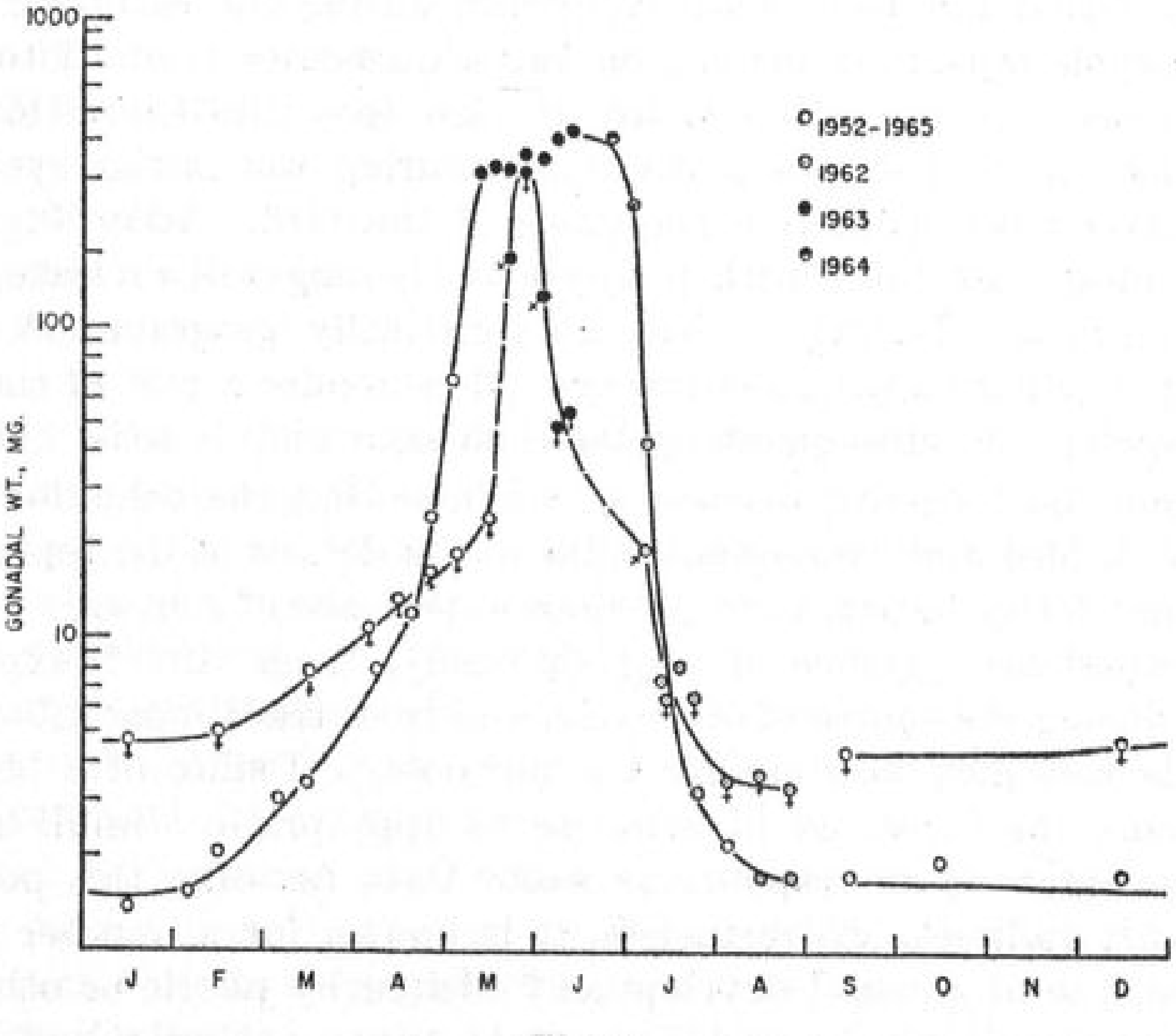

Fig. 2

Annual testicular and ovarian cycles of adult Zonotrichia leucophrys gambelii (from King, Follett, Farner, and Morton, 1966).

Natural gonadal cycles of the white-crowned sparrow, *Z. l. gambelli*, illustrate a fact that indirectly supports this inference (Figure 2). The physiological cycle of the male, as reflected by the gonadal weight, leads that of the female during the progressive phase of the cycle. The male is physiologically capable of fertilizing the female over a rather broad period of time. The female, on the other hand, undergoes the final stages of gonadal growth and development during the course of a few days immediately prior to laying. The curve is biphasic during the progressive phase of the female cycle, suggesting the possibility of new stimuli operating at about the time of arrival on the breeding grounds. Similar patterns of gonadal maturation in which the male reaches « full » gonadal size or activity before the female are common, if not general, among birds (Benoit, 1956; van Tienhoven, 1961). The male also often leads the female behaviorally. For example, as in the present case (Figure 2), he may arrive at the breeding area early and establish territory before the arrival of the female. Allen and Nice (1952) noted that male purple martins are initially more active in nest building than are the females. Similar examples are common.

Ovarian growth and maturation among diverse species of birds is biphasic (see Farner and Lewis, 1971, for review); there is an initial period of slow growth of the ovarian follicles which may begin several weeks to several months prior to the prospective breeding season, followed by a period of rapid growth during which yolk is deposited. The rapid growth phase occupies the last 4 - 11 days before ovulation, and is apparently under the influence of stimuli associated with nesting and the nest site (for review, see especially Farner and Lewis, 1971; Lehrman, 1961). In the case of the migrant Puget Sound white-crowned sparrow, *Z. l. pugetensis,* photoperiodic stimulation alone results in growth of the female reproductive organs to a stage of development

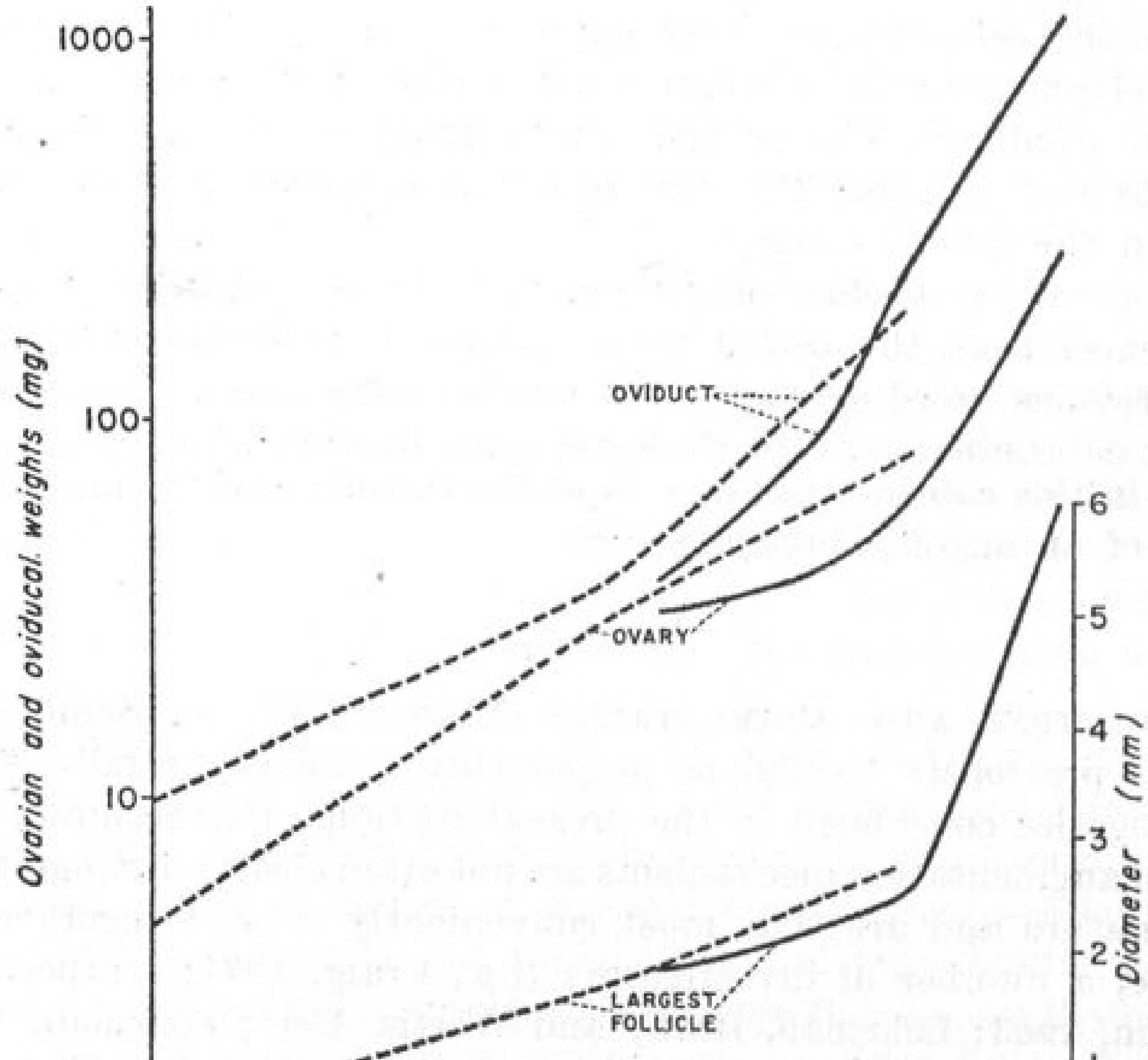

Fig. 3

Growth of ovaries and oviducts in adult Zonotrichia leucophrys pugetensis in response to photostimulation, 18 hours light: 6 hours dark per day (dashed lines), and in feral adults beginning the day of arrival (21 April, 1969) on the breeding grounds, Camano Island, Washington, until the peak of the first ovulatory cycle (solid lines) (Lewis, unpublished data).

comparable to that observed several days after arrival on the breeding grounds (see Figure 3). Within two weeks following arrival (at about the time of nest-building) there is a greatly accelerated growth of the ovary culminating in ovulation. Payne (1969) has reported similar findings on the photoperiodic tricolored and red-winged blackbirds, *Agelaius tricolor* and *A. phoeniceus,* from which he concludes that « ...the complete range of follicular development in early spring up to the time of nesting can be accounted for by the photoperiodic response. Apparently the later stages of ovarian development during nesting are a result of other conditions including presence of a singing male and availability of suitable nest sites ». Data further supporting the view that the terminal stages of ovarian maturation are timed by factors at the nesting site are widely scattered throughout the ornithological literature. For example, it is a matter of common knowledge among field ornithologists that courtship singing and posturing of male birds stimulates sexual receptivity in the female (Howard, 1920; Marshall, 1936).

Lloyd (1965) found that in addition to photic stimuli, the presence of the male, nest boxes, and nest materials were important in influencing the development of the incubation patch of the female starling, *Sturnus vulgaris*. Similarly, Warren and Hinde (1961) found that stimuli from the male may influence the start of defeatherization of the incubation patch in the female canary.

Preparatory mechanisms, then, may have a variety of sources. Experiences from the period before sexual maturity as well as those from previous breeding cycles and non-breeding seasons are often important determinants in the timing of reproduction. Equally important factors in this category are certain of the stimuli received in the initial phases of an ongoing breeding cycle.

INDUCTION AND AUGMENTATION MECHANISMS. — Some of the patterns previously treated as preparatory mechanisms fall also into the categories considered in the present section. Furthermore, induction and augmentation mechanisms are not often clearly distinguished in the literature and are thus most conveniently treated together. For example, a number of investigators (*e.g.*, Craig, 1911; Harper, 1904; Johnston, 1963; Lehrman, Brody and Wortis, 1961; Lehrman, Wortis and Brody, 1961; Matthews, 1939) have demonstrated that the presence of the male stimulates egg-laying in certain female columbids (pigeons and doves). But only Lehrman and his co-workers have shown that the male's presence is essential. Adult female ring doves, when housed in isolation from males, do not lay eggs. Further studies by Lehrman's research group reveal how well induction (*i.e.*, stimuli from the mate) and augmentation (*e.g.*, presence of nesting material) mechanisms interact in the development and timing of egg-laying and incubation in these birds. Some recent experiments (Lott and Brody, 1966; Lott, Scholz and Lehrman, 1967) have shown that sounds from the colony milieu combine with the effects of the mate in advancing ovarian development in female ring doves.

Studying pairs of domesticated budgerigars under conditions of housing which promote reproduction, Brockway (1964b) learned that:

1) The « Loud Warble » of other males appears to stimulate males to warble more than they would otherwise. This effect is also associated with continued sperm production in males and oviposition in females.
2) Being able to see and to hear other pairs is associated with significantly heavier testes and *vasa deferentia.*
3) Large amounts of warbling from other pairs seems to inhibit female receptivity to male precopulatory behavior. However, it also seems to stimulate more extensive precopulatory display of males, possibly offsetting decreased female receptivity.
4) The opportunity to see, but not hear, others seems to stimulate reproductive activities in both members of a pair relative to neither being able to see or hear others. Auditory stimuli alone seem to be more stimulatory in this regard than visual stimuli alone.
5) Visual stimuli seem to elicit nest box activities in the female. However, the opportunity of hearing and seeing other females is not as effective in unpaired females as also being permitted to hear male vocalizations.

Ficken, van Tienhoven, Ficken, and Sibley (1960) have also studied the effects of visual and vocal stimuli on breeding in the budgerigar. Their results similarly indicate that vocalizations of other pairs enhance gonadal development of both sexes. Of particular interest, however, is the fact that the presence of mirrors inhibits the rate of ovarian but probably not of testicular development. The authors have no explanation for this effect.

It would seem from these and previously cited works of Polikarpova (1940), Hinde and Warren (1959), and Warren and Hinde (1961) that the mate and at times other adult birds play a direct role in stimulation of the final stages of gonadal maturation (especially in the female) and in induction and augmentation of reproductive behaviors at a time when they are adaptively appropriate (see also Slater, 1969). Other cues which may operate significantly at about the time of breeding include the presence of nesting material, a nest site, participation in nest building, and the presence of a completed nest.[1]

Perhaps because of their close temporal relationship with physiological and behavioral events of breeding, it is often relatively easy to establish experimentally simple systems of correlation and contingency linking induction and augmentation mechanisms with actual reproduction (*e.g.*, the previously discussed studies of Lehrman and of Brockway). On the other hand, preparatory mechanisms that may be far removed in time from reproductive events under observation, are not as simple from an experimental viewpoint. Consequently (with the singular exception of photoperiodic mechanisms), we know much more about induction and augmentation than we do about preparatory mechanisms. However, preparatory functions must be understood if valid conclusions are to be drawn concerning the mechanisms and importance of social facilitation in the timing of avian breeding cycles.

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[1] A permisive environment (not well-defined for most species) and a high plane of nutrition (see Perrins, 1970) are also essential to the terminal stages of reproductive development, especially of egg-formation.

CONCLUDING REMARKS. — The precise timing of avian sexual cycles derives from the interaction of numerous factors. Among these, social interactions serve in various ways and at various times in the fine-tuning of reproductive timing. In this regard, social behavior was traditionally thought to act in gregarious avian breeders to synchronize their reproductive cycles by mutual stimulation at or near the time of breeding. This view in its original form is not consonant with current knowledge. It fails to explain most of the observed patterns of reproduction within natural populations of birds and does not recognize the influence of social and other factors which may operate at times that are remote from the breeding season.

Most physiological investigations of the timing of avian reproduction have been experimental. Furthermore, because of the precision of photoperiodic responses of many male birds of higher latitudes, the most intensive of such investigations have dealt with the photoperiodic control of reproduction. The effects of other primary environmental timers and of modifiers (see Figure 1 and accompanying text), have for the most part not been subject to such powerful quantitative experimental investigation. Most behavioral and ecological investigations (with certain notable exceptions) have been descriptive rather than experimental and have focused on the role of modifiers. Consequently, a coherent picture of the environmental control of avian reproduction has yet to evolve. The literature as it now stands expresses very diverse points of view with regard to the relative significance and roles of various environmental information in the timing of avian reproductive cycles (compare, for example, Farner and Lewis, 1971; Immelmann, 1963b; Lofts and Murton, 1968; Marshall, 1970; Nalbandov, 1970; Perrins, 1970; Schreiber and Ashmole, 1970). The present state of the literature has led Nalbandov (1970) to comment regarding the disposition of us all to overgeneralize and oversimplify complex functions: « ...it seems obvious that optimal reproductive efficiency is possible only when there is a harmonious and properly synchronized interplay which taken together provides the sum total of what we generally call the feedback system, which, by definition, functions only if none of the links is missing ».

In the present paper, we have generated a hypothetical control scheme and a hypothesis regarding the potential role of social behavior in the timing of avian reproduction. Our thinking, however, is not wedded to either of these. Rather, we hope they may serve to stimulate discussion and to generate new data. We have not seriously dealt with the question of how to approach (operationally) the problems of social facilitation, but rather, have posed some of the more elementary and hopefully significant questions which may be asked.

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PERSUASIVE DEFINITIONS

Persuasive definition and persuasive argumentation are important tools used to shape people's emotive attitudes. Two types of persuasive definitions are distinguished: those intended to change the extension, and those intended to change the emotive charge of the definiendum. Various examples of these language persuasive media are analysed. Thought is also given to some methodological problems of their meaning and use.

Linguistic expressions tend to evoke emotive responses. Under the influence of various factors emotive associations connected with a given expression can be transferred to another one, which was emotively indifferent before, and can thus increase the set of emotively active expressions. This is only one of the many kinds of changes which linguistic expressions can undergo as far as emotive associations are concerned. Another kind consists in the vanishing of such associations; still another, in the driving out of positive associations by the negative ones, or vice versa. It may also happen that emotive associations of a certain kind, e.g. moral ones, are replaced by associations of a different kind, e.g. aesthetic ones. The present paper is intended to describe the typology of such changes and to analyse certain methods of causing them. Changes in emotive attitudes can be evoked by various means: linguistic as well as extra-linguistic. In what follows I shall be concerned solely with those which resort to language signs. Analysis will deal mainly with definitions of various kinds.

Persuasive definitions have been used for centuries to change emotive attitudes. However, it was only recently that their structure and function were given theoretical consideration. The first formulation of the concept of persuasive definition is due to C. L. Stevenson [1]. Although I shall make use of Stevenson's formulations, the description of persuasive definitions given in this paper is my own responsibility. Stevenson really restricts his analyses to persuasive definitions which are here characterized as those the aim of which is to change the extension of the definiendum. In my discussion I shall cover an additional variety which aims to change the emotive charge of the definiendum. Moreover, I shall take into consideration not only full definitions in the form of equivalences, but also partial definitions [2]. The latter often lay down only the necessary or only the sufficient condition for the applicability of the concepts they define. The analysis will also cover a certain kind of utterances - to be called here persuasive argumentations - which, while not being definitions, perform similar functions.

PERSUASIVE DEFINITIONS INTENDED TO CHANGE THE EXTENSION OF THE DEFINIENDUM. — In this variation, the definiendum is an emotively active term, used in everyday parlance, where its meaning, or at least its extension, has been established. The definition is to specify a new extension for the term, and to transfer emotive associations connected with its former extension to the new one. The definiens is here an expression constructed exclusively of emotively indifferent terms or terms loaded with the same charge - positive or negative - as that connected with the definiendum. As will be seen below, it is not so in the case of another variation of persuasive definitions.

Here are some examples:

During an interview Gandhi was once asked whether he appreciated music and if he would want it to be given to the world. In his reply he said: If we mean by music union, concord, mutual help, it may be said that in no department of life can we dispense with it.

In another example, also attributed to Gandhi, victory is defined as consisting in that the defeated does not hate the victor.

Here is a definition of intelligence as formulated by the well known Indian philosopher Krishnamurti:

« ... intelligence is the highest form of sensitivity. To be sensitive to everything, to the birds, to squalor, to poverty, to the beauty of a tree, to the beauty of a face, to the sunset, to colours, to reflections, to the movement of a leaf, to a bird on the wing, to the smile of a child, to tears, to laughter, to pain, agony, anguish, the misery of a human being - to be totally sensitive to all that means to be intelligent » [3].

These examples contain complete definitions, i.e. definitions specifying necessary and, at the same time, sufficient criteria for the applicability of their definienda. They illustrate particularly flagrant change, for purposes of persuasion, of the usual meanings of words. The next example exhibits a partial definition of an artist:

Only he is an artist who improves other people morally through works of art.

This definition specifies a necessary criterion for the applicability of the term artist. It also deviates from everyday usage.

PERSUASIVE DEFINITIONS INTENDED TO CHANGE THE EMOTIVE CHARGE OF THE DEFINIENDUM. — The task of peruasive definition now under discussion is to bring about a change in the emotive associations connected with the term it defines. As with the previous category, here too, the definiendum is taken from everyday language, in which its meaning or at least its extension has been established. It need not, however, be an emotively active term. Moreover, the definiens does not now have to consist only of emotively indifferent terms or terms loaded with the same kind of charge as that connected with the definiendum. On the contrary, the present variation admits of various relations between the emotive charge of the definiendum and that of the definiens. Those relations are shown in the table below, which possibly does not exhaust all the combinations we meet with in everyday parlance. It certainly is not exhaustive with regard to all theoretically possible combinations.

Definiendum	Definiens
1. emotively indifferent	emotively active
2. emotively active	emotively indifferent
3. has a positive charge	has a negative charge
4. has a negative charge	has a positive charge
5. has a charge of one kind	has a charge of a different kind
6. has an incoherent charge	has a coherent charge

The task of persuasive definition is here to replace the emotive charge of the definiendum by that of the definiens. This change may, though need not, be accompanied by a change in the existing extension of the definiendum. The problem arises as to how to make sure that the change in emotive associations takes place in accordance with the intention of the author. If the emotive charge of the definiens drives out that of the definiendum, the change is in agreement with the intention of the author. It is, however, possible that contrary to the author's intention the converse change occurs: the charge of the definiendum drives out that of the definiens. Finally, neither of the two changes may take place, the only effect of the circulation of the new definition being that the addressees become confused, and even develop neuroses. In the latter case we would face a phenomenon similar to that observed in certain experiments on animals. An animal is subjected to two easily differentiable stimuli. The animal's correct responses are rewarded, while the incorrect ones are punished. The stimuli are gradually modified so as to reduce the difference between them. Consequently, the animals's responses become increasingly confused and finally it develops a neurosis. It is not excluded that in some cases persuasive definition may be deliberately intended to cause such a confusion. Obviously, the effective application of persuasive definition depends on a number of extra-linguistic factors. It is therefore important to know these factors, and to control them.

Here are some illustrations:

Civilization, in the real sense of the word, consists not in the multiplication, but in the deliberate and voluntary restriction of wants. This alone promotes real happiness and contentment, and increases the capacity for service.

This example admits of two interpretations, according to two different senses, or rather classes of senses, attached to the term civilization in colloquial language. In the first one, positive charge of an ethical type is given to the previously neutral term. In the second one, the emotive charge associated with the concept of civilization in everyday parlance is replaced by a charge of the ethical type. In both interpretations the change in emotive charge is accompanied by a distinct change in the extension of the definiendum.

« Let us now try to outline the pen portrait of a contemporary patriotic Pole, as stipulated by our young people. ... The perfect patriotic Pole is a man who, above all, is courageous, strong, and fit, and endowed with a number of moral virtues among which pride of place goes to honesty; he is ready to sacrifice himself for his country and to defend it against its enemies; he strives to improve existing social conditions and is an honest and selfless worker, endowed with a number of intellectual qualities among which the most important are knowledge and vocational qualifications, profoundly dedicated to the cause of the economic development of his country » [4].

This passage contains a definition of the term patriotic with which positive emotive associations are connected in everyday usage. Here, however, it is defined by such a set of properties that those associations are strengthened significantly. At the same time the extension of the term is narrowed.

Consider now an example in which the change consists in replacing an incoherent charge by a coherent one. At one time political writers, among them Lenin, used the term patriotism in a derogatory sense. At that time one of the definitional characteristics of the term internationalism was the lack of a home country, while the term itself had a positive charge. Following subsequent social and political changes the derogatory associations connected with the term patriotism came to be replaced by positive ones, and hence the property of lacking a home country became negative. These changes resulted in an incoherence in the emotive charge of the term internationalism and its derivatives. To eliminate that incoherence it was necessary to redefine it in such a way that all its emotively active definitional characteristics should have a charge of the same pole. Since the positive charge of the term internationalism was to be retained, it became necessary to remove the property of lacking a home country from the definitional properties of the term. It is worth noting that the above example illustrates the more general trend to eliminate emotive ambiguity and incoherence from movements, ideologies etc.

In the following example a negative charge is imparted to a term that in its usual meaning is emotively indifferent:

Logistics is meaningless and subjectless logic, dissociated from reality, experience, practice, and objective truth [5].

Persuasive Argumentation. — Persuasive definition is often a part of a broader context to be called persuasive argumentation. By persuasive argumentation is meant here a sequence of expressions which satisfies the following conditions. 1. The sequence contains one (or possibly more than one) term taken from everyday language; it will be referred to as the term of argumentation. 2. The sequence is intended to bring about a change in the emotive associations so far connected with that term. 3. The change is to follow the pattern presented in the previously given table.

There are important differences between persuasive definition and persuasive argumentation. In the former persuasion always takes on a definite form, namely that of a definition. It is true that in everyday parlance the form of definition is not determined univocally, yet the freedom of choice has certain limits, even though they are not strictly determined. In the latter, on the other hand, linguistic expressions of any form whatsoever can be used. Moreover, in both cases the change in emotive charge is based on different principles. In persuasive definition it comes by way of definitional stipulation; in persuasive argumentation it is the context which effects the change.

The following passage contains an example of persuasive argumentation. It comes from a collection of Nazi documents. In the passage some points of political programme are expounded.

First, we must free our nation from the hopeless confusion caused by internationalism, and must do so by bringing the nation up, consciously and systematically, in the spirit of fanatical nationalism. ... Secondly, we must free our nation from the nonsense of parliamentarianism, by training it to combat the madness of democracy and to accept authority and the principle of leadership as indispensable. Thirdly, we must free our nation from the pitiable belief in the help of others, that is, from belief in reconciliation of nations, world peace, international organizations, and international solidarity, thus destroying those ideas [6].

The above persuasive argumentation contains a number of terms: internationalism, fanatical nationalism, parliamentarianism, reconciliation of nations, world peace, international solidarity. Each of these terms has some emotive charge, usually of considerable strength. The aim of the argumentation is to replace each charge by a charge of the opposite pole, i.e. positive by negative and vice versa. The final statement depicts characteristically the persuasive intent of the speach. By « destroying » the concepts, it purports to eliminate their influence upon people's attitudes and upon resulting social consequences.

SOME METHODOLOGICAL PROBLEMS. — All persuasive operations discussed here may take on the intentional or the non-intentional variant. They are understood in the intentional sense when their aims have been deliberately intended by their authors. If the aims have been achieved independently of that, the operations will be classified as persuasive in the non-intentional sense. These two meanings are not mutually exclusive: if the author wanted to achieve the persuasive effect, and has indeed achieved it, both the intentional and the non-intentional criteria are satisfied.

Both varieties of persuasive definitions deviate from the existing linguistic usage, since they change either the extension or the emotive meaning associated so far with their definienda. Therefore, they are definitions of the non-analytical character. This remark requires certain additional explanations in view of three different ways in which the term analytical definition may be understood. A definition is analytical in the intentional sense if its author strives to render the existing colloquial meaning of the term it defines. An analytical definition in the non-intentional sense is an adequate reconstruction of meaning the definiendum had so far in everyday language, regardless of whether such was the intention of its author. A definition is analytical both intentionally and non-intentionally if its author set himself the task of grasping the existing meaning of the definiendum and has in fact attained that goal. In the present paper the concept of analytical definition is used mainly in the third sense. It is obvious that persuasive definition, whether in the intentional or the non-intentional sense, cannot be analytical in the third sense. But other relations also hold: persuasive definition in the intentional sense cannot be analytical in the intentional sense; likewise, persuasive definition in the non-intentional sense cannot be analytical in the non-intentional sense.

In view of the non-analytical nature of persuasive definition the question arises whether in at least some situations the regard for the effectiveness of such definition does not require that its author ought to divert the addressee's attention from the fact that the extension of the definiendum, as delimited by the definition, differs from its extension accepted so far; or that he at least ought to abstain from drawing the addressee's attention to that fact. For should the latter notice that difference, he might reject the definition entirely, or he would not transfer on the new extension his emotive associations previously connected with the definiendum. On the other hand, the same regard for effectiveness requires that the addressee should link his emotive associations with that extension of the definiendum which has been established by the definition. The problem then arises whether it is at all possible that the addressee clearly grasps the new extension and at the same time does not notice that it differs from the former one. Now, such a situation is possible. It may occur when, because of certain circumstances, e.g. suitable manoeuvres of the persuader, the addressee does not reflect on the relation between the former and the new extension of the definiendum, or when the addressee does not realize clearly what the former extension was.

Persuasive definitions are often given a form which can be reduced to the following: The true A is B, The real A is B, etc. Here are few examples:

True happiness consists in a state of perfection.

The really wise man is he who acts magnanimously.

Real beauty means not outward beauty, but a righteous heart.

What is the function of the modifiers: true, real, etc.? First, it must be noted that they occur mainly in a discussion with an opponent's opinion. Sometimes the criticized opinion is stated explicitly together with the suggested definition, as is the case in the third example. In other cases the opinion can be deduced from the context. Very often, however, neither is the case, and the task of the definition is to evoke an unfavourable attitude toward some opinion which is left unspecified. The tension inherent in that attitude can subsequently be channelized according to persuader's wishes.

Persuasive definition using the modifier true or a similar one may be intended to make the addressee think that this is the true analytical definition, that people understand the definiendum as it is stated in the definition. Persuasion conceived in this way would imply a misunderstanding or the intention to mislead the addressee, since persuasive definition always deviates from the usual meaning of the term it defines.

A different interpretation is also possible. The use of the modifier true may be intended to make the addressee believe that a thorough, and not merely superficial, understanding of the definiendum, an understanding that grasps « the essence of the thing », is precisely such as established by the definition. But of what is that thorough understanding to consist? That issue is not analysed in the contexts in which persuasive definitions occur.

The role of the modifier true may also consist in developing an attitude favourable to the definition containing that modifier and possibly also to make the addressee reject the competitive definition advanced by those whose opinion is criticized.

An analogous persuasive role is played by the modifiers false apparent, etc., added to a definition of a term. Sometimes, placing the term in quotation marks serves the same purpose. In such cases the function of the modifier is to develop an attitude unfavourable to the definition preceded by the modifier.

A hypothesis is put forward to the effect that people more readily accept definitions having the external form of real definitions than those having the form of nominal ones. This applies in particular to those nominal definitions in which the definitional copula has the form of we call, we term, etc. Should the hypothesis be right, out of the two definitions of freedom:

Freedom is self-imposition of restraints.

Self-imposition of restraints we call freedom the former would be more readily accepted.

How could this phenomenon be explained? Certainly, the very form of a definition can play an essential role. In nominal definition it suggests a certain arbitrariness: a thing termed in one way might also be termed in some other way. Real definition, on the other hand, may savour of a categorical statement of fact. However, a critical person may resist this suggestion. The terms real definition and nominal definition have several different meanings; and only with some of them is real definition a statement of empirical fact, while nominal definition only reflects terminological conventions. The external form of a definition does not of itself determine its character.

It is sometimes objected that the persuasive operations here discussed raise doubts of a moral nature. Is that objection justified? The answer must be in the negative. From the descriptions given above it does not follow that these persuasive operations have by themselves any morally objectionable property. They are certain instruments of action, and as such, are morally indifferent. The evaluation of goals to which these instruments are applied, and of the ways in which they are used, is another matter. They may arouse both approval and censure.

Persuasive operations are methods of shaping attitudes. Is the use of such methods admissible in science, and, if so, to what extent? The object of research is usually found in classification and description of phenomena, and also in the explanation and prediction of such. Hence it seems that this leaves no room for any persuasive operations. On the other hand, it is certainly true that science is one of the most powerful instruments for shaping attitudes, including emotive ones. Attention must, however, be drawn to the fact that although changes in emotive attitudes do in fact take place under the influence of science, yet they come about in a different way. Science effects changes in attitudes by providing information about various phenomena and regularities of Nature which are sources of emotive attitudes. Persuasive verbal operations, on the other hand, make use of the emotive associations connected with some terms and transfer them to other terms or to other extensions of the same terms. Would then the conclusion be that science leaves no room for persuasive verbal operations? It seems that in mathematics and the exact sciences the answer is to be in the affirmative. It would need a seperate paper to consider whether these operations are admissible in the humanities and the social sciences.

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CRONACHE - REPORTS

Il pensiero di F. Enriques in un discorso di B. Segre

Il contributo dato da F. Enriques allo sviluppo non soltanto degli studi di geometria, ma in un senso ben più ampio del pensiero filosofico e scientifico, viene ricordato quest'anno - nel centenario della nascita dell'illustre studioso che fu tra i fondatori della nostra Rivista - in una serie di Convegni promossi dall'Accademia Nazionale dei Lincei. Il primo di essi - svoltosi a Milano e dedicato agli aspetti più specificamente geometrici del pensiero del compianto Maestro - si è aperto con un discorso introduttivo del Presidente dell'Accademia dei Lincei, Prof. Beniamino Segre. Di questa lucida esposizione che esamina l'opera di F. Enriques in un'ampia prospettiva storica, sicuri di far cosa grata ai lettori di « Scientia » pubblichiamo qui il testo, rammaricandoci che esigenze di spazio ci impongano di omettere la ricca appendice bibliografica.

I. - Un secolo or sono - e precisamente il 5 gennaio 1871 - nasceva a Livorno Federigo Enriques, una delle personalità più spiccate, originali e sapientemente eclettiche che possa vantare l'Italia in tale periodo, la cui influenza in tanti rami del sapere scientifico e filosofico è da presumersi che - anzichè affievolirsi - andrà ancora aumentando col tempo. Onde celebrare quella ricorrenza, l'Accademia Nazionale dei Lincei - di cui l'Enriques fu Socio per un quarantennio, dal 1906 fino alla morte, avvenuta il 14 giugno 1946 - ha patrocinato ed indetto per quest'anno una serie di manifestazioni attinenti a tali rami.

Abbiamo anzitutto il Convegno internazionale di Geometria promosso - sotto gli auspici dell'Accademia - dall'Istituto di Matematica dell'Università degli Studi di Milano, il quale al nome appunto dell'Enriques si intitola. Seguiranno altri tre Convegni internazionali dedicati alla Storia ed alla Didattica delle matematiche ed alla Filosofia della Scienza, che si terranno rispettivamente a Pisa, Bologna e Roma nei giorni 7, 8-9 ed 11-12 del prossimo ottobre.

Non fa certamente d'uopo rilevare le ragioni per la scelta di dette città: Milano, ove ha sede una fiorente scuola geometrica ispirata all'Enriques, tramite l'opera qui perseguita da Oscar Chisini, che di quel grande fu valente discepolo, collaboratore ed amico; Pisa, della cui Scuola Normale Superiore l'Enriques fu promettentissimo allievo; Bologna e Roma, presso le cui Università egli svolse con singolare perizia e dedizione e per vari decenni la propria attività di Maestro. E nemmeno occorre rammentare quale appassionato fervore l'Enriques sapesse suscitare attorno a sé in campi così disparati, ch'egli riusciva ad inquadrare in una veduta filosofica unitaria ed a dominare con una versatilità e profondità di cultura portentose, in ciò agevolato dal suo raffinato senso estetico ed umanistico, dalla sua rapida e penetrante intuizione, dalla sua fede illuminata nel razionalismo umano, non disgiunta tuttavia da diffidenze od avversioni verso ogni troppo elaborato tecnicismo.

Queste inclinazioni e doti copiose ed eccezionali, ch'egli ebbe già a manifestare giovanissimo, rendono non facile un'esauriente valutazione retrospettiva dell'opera sua; in vita esse lo indussero ad un'eccelsa attività, multiforme, sistematica ed indefessa pur sotto l'ingannevole apparenza di una certa dispersione o pigrizia, e non valsero a lasciar esente quell'opera da qualche menda o lacuna. Ma il suo apporto ha da essere misurato con un metro inconsueto, dovendo venir messo in relazione colla sua visione dinamica del pensiero scientifico e filosofico e colle sue concezioni sul valore propulsivo dell'errore.

Data la natura del presente Convegno, non entrerò qui in molti particolari sugli aspetti più propriamente storici, pedagogici e filosofici del pensiero e dell'opera di Federigo Enriques, ai quali verranno d'altronde dedicati i tre già menzionati Convegni di Pisa, Bologna e Roma. Relativamente a quelli, mi limiterò a rinviare ad un discorso di L. Campedelli ed alla *Introduzione* di L. Lombardo-Radice all'Antologia di scritti filosofici dell'Enriques, osservando soltanto come molti di tali scritti e di quelli didattici presentino oggi ancora un loro acuto mordente, sì da farci rammaricare che da essi in passato non sia stato tratto sul terreno pratico tutto quanto sarebbe stato possibile e desiderabile, auspicando infine che il vivificante pensiero dell'Enriques venga tenuto in debito conto nella ristrutturazione in atto nelle nostre Università.

Secondo tale pensiero, la scienza dev'essere considerata nel suo divenire, la realtà che ci circonda viene concepita in termini matematici quale un invariante della corrispondenza fra volizioni e sensazioni, l'insegnamento ha da essere impartito dall'alto come un sacerdozio ma con un'intima rispondenza fra docente e discente, mentre la ricerca scientifica è da intendere quale attività intellettuale creatrice squisitamente aristocratica, in cui prima dei metodi interessano i risultati. Da siffatte impostazioni filosofiche, non si può come già ho detto prescindere nel vagliare l'opera matematica dell'Enriques.

Questa - ancorchè in qualche periodo non sia stata ed oggi ancora non sia troppo di moda - ha valso a fare indubbiamente di lui uno dei maggiori geometri dei nostri tempi ed è ricchissima, oltrechè di risultati, di spunti ed idee geniali varie delle quali attendono tuttora di venire pienamente utilizzate. Per tali motivi, l'Accademia Nazionale dei Lincei ha ritenuto opportuno di raccogliere il fiore delle sue ricerche geometriche in tre grossi volumi, la cui multiforme ricchezza può già desumersi con una rapida scorsa all'Indice analitico posto alle pp. 403-423 del terzo di essi; questi lasciano fuori naturalmente il suo fondamentale volume postumo sulle superficie algebriche ed i molti trattati, fra i quali è superfluo ricordare l'opera magistrale in quattro volumi scritta in collaborazione col Chisini e quella non meno classica riguardante la geometria proiettiva.

II. - La produzione geometrica dell'Enriques è stata tanto vasta e complessa da non permettermi - nel breve lasso di tempo a mia disposizione - che di dare su essa se non rapidi scorci, salvo a rinviare ad altre analisi per ulteriori ragguagli. Un'altra circostanza da tener presente è il numero assai ragguardevole dei lavori di polso da lui scritti in collaborazione con diversi autori, il che viene a porre il problema - alle volte non facile - della parte da lui avuta nella relativa stesura; problema che rientra in quelli non meno significativi e delicati inerenti all'azione indotta dall'Enriques in Italia ed all'estero nello sviluppo della geometria dall'ultimo decennio del secolo scorso fino ai giorni nostri, ed all'influenza che potrà ancora esercitare in futuro l'opera sua.

Soltanto trent'anni prima di tale decennio gli studi geometrici erano alquanto negletti in Italia, specie a raffronto delle rigogliose scuole geometriche tedesca e francese. Un'autonoma scuola geometrica italiana era indi sorta subitamente quasi per magia a partire dal 1860 per merito di Luigi Cremona, al quale si debbono fra l'altro le trasformazioni che ne portano il nome e lo studio di varie superficie algebriche rappresentabili birazionalmente su di un piano; questa scuola si era poi raffermata, ampliata e rinnovata ad opera di Giuseppe Veronese, di Eugenio Bertini e, soprattutto, di Corrado Segre, con importanti sistematici sviluppi di geometria proiettiva iperspaziale, che avevano incidentalmente offerto agli ultimi due geometri gli strumenti per un'originale elaborazione della geometria su di una curva algebrica intesa al modo di Riemann e nell'indirizzo algebrico invariantivo di Brill e Noether. Ciò suggeriva di intraprendere ricerche analoghe sulle superficie, già soltanto adombrate dal Noether, per le quali però - mentre i mezzi a disposizione erano piuttosto rudimentali - nuove difficoltà si intravvedevano e dovevano di fatto palesarsi assai ardue.

Tali difficoltà vennero in una loro parte cospicua superate, ed in modo assai brillante, mercé gli sforzi congiunti di Castelnuovo ed Enriques. Questi hanno infatti il vanto di avere introdotto ed approfondito per le superficie algebriche la distinzione fra genere aritmetico e genere geometrico, e quindi fra superficie regolari ed irregolari, utilizzato con vero virtuosismo la considerazione dei successivi aggiunti di un sistema lineare, resa più nitida e più largamente operativa la concezione invariantiva dei problemi distinguendo chiaramente l'invarianza assoluta da quella relativa, risolta la spinosa questione delle così dette curve eccezionali (già considerate nebulosamente dal Noether), mostrando ch'esse risultano in numero infinito nel caso e nel caso soltanto delle superficie razionali o riferibili a rigate, che sono poi anche quelle su cui il processo dell'aggiunzione viene ad estinguersi dopo un numero finito di passi.

Va rilevato che il Castelnuovo, più anziano di sei anni dell'Enriques (di cui sposò una sorella), e giunto alla cattedra di Roma a 26 anni (mentre l'Enriques doveva poi salire a quella di Bologna a 25 anni soltanto), ebbe agli inizi una funzione determinante nell'orientare l'Enriques verso le suddette questioni. Ma con portentosa rapidità quest'ultimo si pose in grado di trattare di esse col futuro cognato su di un piede di perfetta uguaglianza: nacque così un'intesa fra le più rare, che doveva dare frutti mirabili e copiosi manifestandosi con una serie di lavori algebrico-geometrici basilari in collaborazione, usciti a ritmo serrato negli anni 1895, 1896, 1900 e 1901, seguiti poi da una notevole ricerca di carattere trascendente del 1906 in cui si dimostra la costanza dell'irregolarità di una superficie sufficientemente generale immersa in una varietà a tre o più dimensioni.

Non è facile dire quale sia stato separatamente il contributo dei due autori ai primi lavori, nei quali trovansi fuse le loro spiccate caratteristiche di ampiezza di vedute ed eleganza raffinata, e di entusiasmo e geniale fantasia geometrica. Siamo però in grado di indicare come essi procedevano nella fitta oscurità che allora avvolgeva i problemi di cui si occupavano, in base al seguente passo del discorso tenuto dal Castelnuovo al Congresso internazionale del 1928, con riferimento a quel primo gruppo di lavori.

Ecco come si esprime al riguardo il Castelnuovo: « Avevamo costruito, in senso astratto s'intende, un gran numero di modelli di superficie del nostro spazio o di spazi superiori; e questi modelli avevamo distribuito, per dir così, in due vetrine. Una conteneva le superficie regolari per le quali tutto procedeva come nel migliore dei mondi possibili; l'analogia permetteva di trasportare ad esse le proprietà più salienti delle curve piane. Ma quando cercavamo di verificare queste proprietà sulle superficie dell'altra vetrina, le irregolari, cominciavano i guai, e si presentavano eccezioni di ogni specie. Alla fine lo studio assiduo dei nostri modelli ci aveva condotto a divinare alcune proprietà che dovevano sussistere, con modificazioni opportune, per le superficie di ambedue le vetrine; mettevamo poi a cimento queste proprietà colla costruzione di nuovi modelli. Se resistevano alla prova, ne cercavamo, ultima fase, la giustificazione logica. Col detto procedimento, che assomiglia a quello tenuto nelle scienze sperimentali, siamo riusciti a stabilire alcuni caratteri distintivi tra le due famiglie di superficie. Basterà qui citarne uno solo: mentre sopra una superficie regolare ogni sistema continuo di curve algebriche è contenuto in un sistema lineare di curve dello stesso ordine, ciò non avviene per le superficie irregolari, le quali posseggono sempre sistemi continui non appartenenti a sistemi lineari ».

Il problema di invertire quest'ultima proprietà deve allora indubbiamente essersi posto ai due autori. Di esso - a quanto ebbe poi a riferirmi confidenzialmente Francesco Severi - parlò l'Enriques al Severi nel 1904 durante una visita che questi fece al Maestro (di lui più anziano di otto anni, e di cui era stato Assistente) al momento di salire - egli pure venticinquenne - alla sua prima cattedra a Parma.

Il Severi espresse lì per lì il parere che, per dirimere la questione, dovesse farsi uso della nozione di serie caratteristica di un sistema continuo di curve, nozione ch'egli aveva da poco introdotta come (tutt'altro che immediata) estensione di quella analoga ma assai più elementare per i sistemi lineari. L'Enriques, compresa immediatamente la forza operativa di quel nuovo concetto, pervenne in brev'ora al suo scopo col dimostrare la completezza della serie caratteristica di un sistema continuo completo. La dimostrazione, pubblicata dall'Enriques nel dicembre 1904, si basa su di un'idea brillantissima (ripresa ed un po' semplificata dal Severi l'anno appresso), che portò fra l'altro l'Enriques ad enunciare e stabilire un importante « principio di degenerazione »; essa è però lacunosa in un punto non appena il genere geometrico della superficie si supponga positivo, secondo quanto rilevò poi il Severi nel 1921. Il risultato tuttavia sussiste sotto opportune limitazioni, come ebbe a provare il Poincaré nel 1910 con procedimento trascendente; ma restava il non facile problema di pervenire ad esso per via algebrico-geometrica. Tale questione risulta di notevole rilievo da diversi punti di vista ed è tuttavia ancora aperta, nonostante che attorno ad essa si siano poi ripetutamente affaticati diversi autori ed in modo particolare lo stesso Enriques.

Va osservato che per una siffatta questione l'uso di speciali modelli non riesce di alcun vantaggio. Esso si impone invece in altri ordini di idee: la possibilità di *costruirne* agevolmente di adeguati costituisce un'indiscussa superiorità dell'indirizzo algebrico-geometrico italiano, ed ha dato modo all'Enriques di conseguire altri spettacolari successi con la scoperta di famiglie molto interessanti ed impreviste di superficie (quale, ad esempio, la superficie del sesto ordine passante doppiamente per gli spigoli di un tetraedro, a cui è stato dato il suo nome) e con la classificazione delle superficie algebriche dal punto di vista birazionale, in ispecie di quelle dei primi generi.

È questo un problema fondamentale che sorge e si impone fin dagli inizi della teoria in modo del tutto spontaneo, ma che risulta di difficoltà e complessità assai notevoli. Di esso egli si occupò a lungo con sforzi tenaci, pervenendo a risultati acutissimi con mezzi alle volte relativamente modesti, ma soprattutto mediante penetranti idee originali e sempre fervida fantasia creatrice. Il superiore distacco con cui volutamente talora sorvolava su quelli ch'egli chiamava « dubbi critici », se da un lato gli agevolò la raccolta di risultati importanti, fece sì d'altro canto che le sue indagini sulla classificazione delle superficie non ebbero dapprima in campo internazionale tutta la risonanza che loro sarebbe spettata. Ampi e qualificati consensi sono però ad esse venuti di recente, dopo che gli stessi risultati furono ritrovati da I. R. Sciafarevich e dalla propria scuola con completo rigore e generalità, e con l'esplicito riconoscimento del valore determinante delle idee dell'Enriques.

Al riguardo va ancora rammentato che i primi ardui passi in tale via erano stati compiuti dal Castelnuovo nel 1893 e nel 1896, col dimostrare la razionalità di ogni involuzione piana e stabilire che

$p_a = P_2 = 0$ sono condizioni (non soltanto ovviamente necessarie, ma anche) sufficienti per la razionalità di una superficie algebrica. Questioni analoghe a queste si pongono per le varietà algebriche di dimensione ≥ 3 ed offrono complessità rapidamente crescenti, tanto che abbastanza poco si conosce a tutt'oggi relativamente ad esse. È quindi assai meritorio per l'Enriques di avere colto i primi risultati anche in siffatte difficili estensioni, col dare nel 1912 un esempio concreto di involuzione non razionale dello spazio ed assegnare una condizione sufficiente per l'unirazionalità di una varietà a tre dimensioni.

A lui sono anche dovuti la fondamentale scoperta dei plurigeneri e vari altri risultati non meno importanti, di cui passerò a dire fra breve trattando della sua influenza sullo sviluppo della geometria.

III. - Tale influenza fu veramente assai grande e si esercitò anzitutto al più alto livello sui matematici della sua generazione. Già abbiamo visto com'essa avesse agito in modo decisivo sulla produzione matematica del Castelnuovo ed anche in misura non lieve su quella del Severi: molti dei risultati di questi tre autori trovansi indi ripetutamente citati ed utilizzati nel t. II del celebre trattato di Picard e Simart, che è del 1906, alla fine del quale - per dirla collo stesso Picard - « MM. Castelnuovo et Enriques ont bien voulu nous donner une Note extrêmement intéressante qui complète notre étude et donne sur toutes ces questions l'état actuel de la Science ».

Va poi particolarmente rammentata la poderosa e magistrale memoria sulle superficie iperellittiche, scritta dall'Enriques in collaborazione col Severi, alla quale nel 1907 venne assegnato il Premio Bordin dell'Accademia delle Scienze di Parigi e di cui è stato asserito ch'essa è « una delle più belle memorie di tutta la letteratura matematica italiana del nostro secolo ». Si può inoltre dire che tale lavoro abbia fatto da capostipite a tutta una serie di ricerche compiute poi da numerosi autori in Italia ed all'estero, colle quali è stata creata la teoria delle varietà abeliane e quasiabeliane e delle varietà di gruppo.

Presentano altresì interesse storico considerevole le relazioni, le disparità e le analogie fra l'Enriques ed un nostro matematico di notevole rilievo anche se di non altrettanto gigantesca statura, Gino Fano, di cui pure ricorre quest'anno il centenario, dandosi anzi la circostanza curiosa di essere questi nato a Mantova esattamente nello stesso giorno dell'Enriques.

Il Fano, allievo diretto di Corrado Segre, Guido Castelnuovo e Felix Klein, risentì pure assai l'influsso dell'Enriques, col quale anche collaborò in talune ricerche. Uno dei primi lavori di quest'ultimo è quello del 1893 sulle superficie con infinite trasformazioni proiettive in sé. Dello stesso argomento si occupò poco appresso il Fano, il quale compì poi altresì indagini analoghe riguardanti le varietà algebriche a tre o più dimensioni. All'Enriques si debbono inoltre le prime ricerche sui gruppi continui di trasformazioni cremoniane nel piano, tosto proseguite dal Fano e quindi estese dai due autori allo spazio in un'elaborata memoria congiunta, nella quale la riduzione a tipi dei gruppi continui di trasformazioni cremoniane viene effettuata mediante trasformazioni birazionali.

Sia l'Enriques che il Fano si interessarono proficuamente dei fondamenti della geometria proiettiva, sul quale argomento pubblicarono anche una *Corrispondenza* fra loro intercorsa. All'Enriques si debbono fra l'altro la prima esauriente dimostrazione del così detto teorema fondamentale di Staudt, tanto semplice da potere venir riportata nel suo testo universitario di geometria proiettiva già citato, il completamento di una lacuna riscontrata nella celebre memoria di Riemann sui principi della geometria, l'articolo dell'enciclopedia tedesca su tali principi, ed un originale e significativo collegamento del problema logico al problema psicologico nello studio di essi. Al Fano risale il merito di avere per primo utilizzato le geometrie finite nello studio assiomatico della geometria proiettiva iperspaziale, con l'osservazione della necessità di aggiungere ai postulati dell'appartenenza quello che afferma ogni retta dover contenere più di due punti (mentre non basterebbe supporre l'esistenza di una retta contenente almeno tre punti), nonchè la constatazione ed il significato della possibilità di quadrangoli piani completi aventi i tre punti diagonali allineati. Il Fano può quindi a ragione considerarsi come il fondatore di un nuovo capitolo di matematica, quello detto appunto delle geometrie finite, il quale in tempi recenti ha avuto inaspettati rigogliosi sviluppi ed è tutt'ora ben lungi dal potersi dire esaurito.

Il Fano scrisse inoltre i due articoli dell'enciclopedia tedesca riguardanti la contrapposizione fra gli indirizzi sintetico ed analitico in geometria ed i gruppi continui, nonchè quello dell'enciclopedia di matematiche elementari sulle geometrie non euclidee e non archimedee. Relativamente alla geometria non euclidea egli pubblicò altresì una bella monografia, che in parte si riattacca a conferenze tenute dall'Enriques ed all'opera ben nota redatta da R. Bonola su tale argomento sotto l'ispirazione di quest'ultimo.

È anche da sottolineare il fatto che, nell'importante lavoro già citato del 1912, l'Enriques riesce ad ottenere un'involuzione spaziale irrazionale poggiando in modo essenziale su di una precedente ricerca del Fano, riguardante alcune varietà algebriche a tre dimensioni aventi tutti i generi nulli. Alla fine di quel lavoro, l'Enriques aggiunge di avere desunto dal suo risultato la convinzione della irrazionalità dell'ipersuperficie cubica generale di S_4, sulla scorta di un procedimento di continuità ch'egli ivi brevemente delinea; ed è curioso che molti anni dopo, a me che ignaro allora di questi precedenti gli prospettavo il medesimo procedimento quale piano di una mia ricerca, l'Enriques parve dimentico della sua congettura tanto da sconsigliarmi nettamente quella via di attacco.

Studi approfonditi delle ipersuperficie cubiche di S_4 dal punto di vista proiettivo erano stati effettuati da Corrado Segre, Castelnuovo, Enriques e Fano; fra essi mi limiterò a ricordare quale esempio l'elaborata memoria del Fano, in cui fra l'altro vengon rettificati precedenti risultati dell'Enriques sugli spazi pluritangenti della $V_3{}^3$ generale di S_4. Tuttavia il problema della razionalità od irrazionalità della $V_3{}^3$ generale di S_4, incidentalmente già menzionato da Corrado Segre fin dal 1887, non può dirsi a tutt'oggi soddisfacentemente risolto. Ad esso il Fano ebbe a dedicare lunghi sforzi perseveranti, avvicinandosi assai in fine alla soluzione ed ottenendo per via numerosi risultati di notevole rilievo.

Mi consta intanto che il Fano, come quasi certamente dal canto suo avrà fatto l'Enriques, tentò dapprima reiteratamente la strada delineata da quest'ultimo, in ciò forse confortato dal fatto che - come non è difficile stabilire - un procedimento analogo a quello dell'Enriques vale a dimostrare che sulla superficie cubica generale di S_3 non esiste nessun sistema omaloidico di curve che siano intersezioni complete. Riscontrata però l'inutilità di siffatti tentativi, su cui quindi nulla egli ebbe a pubblicare, il Fano modificò il problema facendolo rientrare in quello più ampio e fondamentale della razionalità o meno delle diverse V_3 algebriche aventi tutti i generi e plurigeneri uguali allo zero. Fra queste V_3 si hanno ad esempio le $V_3{}^n$ di S_4 con retta (n—2)-pla, già più volte incontrate dall'Enriques; e v'è pure tutta una serie di classi assai notevoli, delle quali il Fano riuscì non soltanto a dimostrare l'irrazionalità, ma anche a stabilire un loro diverso distacco dalla razionalità, attraverso al complesso studio di sistemi lineare di superficie con tutti i generi uguali all'unità esistenti su esse. Tale studio venne da lui svolto a più riprese durante un periodo di circa quarant'anni, con delle fasi di carattere sperimentale analoghe a quelle descritte dal Castelnuovo per il suo lavoro di ricerca in collaborazione coll'Enriques, e culmina con la dimostrazione della presunta irrazionalità della $V_3{}^3$ di S_4 sotto un'ipotesi di lavoro concernente i punti e le linee base dei suddetti sistemi lineari. Si tratta di un poderoso complesso di lavori per cui il nome del Fano verrà a lungo ricordato.

Le questioni di uni- e birazionalità sulle superficie e varietà algebriche trattate da Castelnuovo, Enriques e Fano possono farsi rientrare in un quadro più ampio, in cui trovano posto ricerche successive di M. Baldassarri, U. Morin, L. Roth che, pure arrecando pregevoli contributi ulteriori, mostrano quanto resti ancora da fare in quel campo. Ai tre valenti matematici testé nominati, tutti purtroppo prematuramente scomparsi, vada con affettuoso ricordo il nostro cocente rimpianto.

Un altro interessante argomento di ricerca perseguito dal Fano, a seguito di antichi lavori dell'Autonne ed in cui si è ancora ben lungi dall'aver detto l'ultima parola, è quello delle trasformazioni birazionali di contatto; desidero quindi additarlo ai giovani per future ricerche.

Come l'Enriques, il Fano si occupò con passione dei problemi dell'insegnamento a tutti i livelli e trasfuse la propria personalità nelle sue lezioni universitarie. Ciascuno dei due scrisse fra l'altro un trattato di Geometria descrittiva. Il volume del Fano su questo argomento supera forse quello, esso pure eccellente, dell'Enriques: e ciò in quanto la materia si attagliava bene al carattere meticoloso e preciso del Fano, sotto certi aspetti persino troppo rigido e militaresco, ed ai futuri ingegneri a cui si rivolgeva il suo corso; mentre invece l'Enriques confidava volontieri quell'insegnamento al proprio Assistente, preferendo per conto suo campi in cui poteva abbandonarsi al proprio estro geniale ed incidere più profondamente sull'ascoltatore, come quelli offerti dalla geometria proiettiva, dalle matematiche complementari (di cui tenne la prima cattedra in Italia), dalla geometria superiore.

IV. - Inenarrabile fu l'influenza esercitata sui giovani in Italia ed all'estero dall'Enriques nei vari campi di cui ebbe ad interessarsi ed in cui si dimostrò un superbo animatore: e ciò non soltanto attraverso a splendide lezioni e conferenze ed ai suoi scritti e trattati magistrali (questi ultimi tradotti anche quasi tutti in lingue straniere), ma altresì mediante le sue eccezionali capacità organizzative, la fervida passione che poneva in ogni sua intrapresa, la signorile affabilità del tratto e le peculiarità socratiche ed umane delle sue relazioni coi numerosi discepoli, che sapeva autorevolmente scegliere anche fuori della scuola e dirigere con metodo peripatetico, infondendo loro la fiducia nelle proprie forze e riconoscendo acutamente per tempo quali fossero le speciali attitudini di ciascuno di essi.

L'ascendente ch'egli ebbe nell'insegnamento secondario della matematica e nello sviluppo della cultura scientifica e filosofica si dimostrò invero grandissimo e si esercitò fra l'altro per il tramite di opere divenute presto famose, quali le successive edizioni e traduzioni degli «Elementi di geometria» (in collaborazione con Ugo Amaldi), le « Questioni riguardanti la geometria (e poi anche la matematica) elementare » (con la cooperazione di U. Amaldi, E. Baroni, R. Bonola, B. Calò, G. Castelnuovo, A. Conti, E. Daniele, A. Giacomini, B. Guarducci, G. Vitali, a cui poi si aggiunsero G. Vailati ed O. Chisini, D. Gigli, A. Padoa, U. Scarpis, E. Bompiani, V. Notari, A. Sabbatini), ed il potenziamento del « Periodico di Matematiche », nonché la fondazione e l'affermazione in campo internazionale della rivista « Scientia ». Ma ancora maggiore fu l'orma da lui lasciata nell'ambito della ricerca scientifica e dell'insegnamento universitario, ad opera anche dei numerosi discepoli diretti ed indiretti che raggiunsero la cattedra universitaria in Italia, in Europa e nelle due Americhe e dei loro innumerevoli allievi, vari dei quali già hanno conseguito una durevole notorietà, e la cui attività scientifica e didattica è stata ed è tutt'ora strettamente connessa con quella svolta a suo tempo dall'Enriques.

Fra i suddetti discepoli diretti farò soltanto i nomi di Pol Burniat, Luigi Campedelli, Oscar Chisini, Fabio Conforto, Alfredo Franchetta, Harald Geppert, Lucien Godeaux, Paul Libois, Giuseppe Pompilj, Oscar Zariski, bastevoli tuttavia per lasciar comprendere come un'analisi approfondita dell'impronta direttamente od indirettamente arrecata alla geometria dall'Enriques verrebbe ad abbracciare buona parte della storia di questa disciplina negli ultimi cinquant'anni. Per ragioni di tempo, sarò quindi costretto a limitarmi a rapidi tocchi sui tre italiani già purtroppo scomparsi fra quelli testé nominati, relativi cioè a Conforto, Pompilj e Chisini.

V. - Fabio Conforto, triestino, morto a soli 44 anni a Roma nel 1954, può dirsi sia stato un matematico di razza, in tutto degno della scuola geometrica romana cui appartenne, del quale oggi ancora amaramente risentiamo la prematura perdita. Dopo di avere frequentato per due anni il Politecnico di Milano, egli passò agli studi di matematica dietro consiglio del Chisini, che aveva potuto valutarne le doti eccezionali ed a cui forse anche si deve il suggerimento di completare tali studi nell'Università di Roma, presso la quale il Conforto poi sempre rimase.

Le sue ricerche principalmente si svolsero nell'indirizzo del Severi e concernono le funzioni automorfe, abeliane e quasi abeliane, un suo volume sulle funzioni abeliane modulari essendo stato qualificato una delle opere di maggior pregio pubblicate in qualsiasi ramo dell'alta matematica in tempi recenti.

Ma egli - specie agli inizi - subì anche in guisa determinante l'influenza dell'Enriques, alle cui vedute espresse in vari corsi da quegli seguiti si ispira un altro suo bellissimo volume uscito nel 1939, quasi in concomitanza con l'assegnazione della cattedra su cui succedette a Gaetano Scorza. Questo volume, dal titolo *Le superficie razionali,* tocca argomenti disparati, a cui durante più di un secolo hanno contribuito numerosi ricercatori di varie Nazioni, unificandoli in un'elevata concezione - risalente all'Enriques - che fa dei relativi sviluppi quasi un'introduzione ed un banco di prova per una teoria generale delle superficie algebriche. La ricca multiforme e suggestiva materia vien ivi rielaborata e presentata per la prima volta in una trattazione sistematica - che ben si può dire faccia oggi testo sull'argomento - ammirevole sia per la quantità e qualità dei risultati che per la ricchezza ed il rigore con cui essi sono conseguiti, attraverso talora a delicate discussioni e con l'uso di metodi ispirati a recenti sviluppi della geometria algebrica. Ed è precisamente in vista di tale esposizione d'assieme, che il Conforto - sotto la guida dell'Enriques - aveva in precedenza condotto varie ricerche, riguardanti fra l'altro i fasci di Halphen, i piani doppi razionali, le superficie ellittiche.

In detto volume viene anche esposto un'interessante studio sulle superficie a sezioni piane iperellittiche dovuto a Giuseppe Pompilj, che - assieme al Franchetta (con cui curò l'opera postuma dell'Enriques precedentemente menzionata) - fu l'ultimo allievo di Federigo Enriques, ed al quale debbonsi anche ricerche di rilievo concernenti le trasformazioni cremoniane di un piano in sè dotate di una curva di punti uniti, i piani tripli o multipli, le varietà abeliane. Il Pompilj venne poi indotto dall'allontanamento dell'Enriques dalla cattedra per motivi razziali e dagli eventi bellici a passare dalle indagini geometriche a quelle statistiche e probabilistiche; anche in queste, tuttavia, egli ebbe modo di conservare quell'ampiezza ed originalità di vedute nel trattare argomenti elevati, quel prospettare le questioni particolari nel quadro dei problemi di più larga portata, che gli derivavano dagli insegnamenti del Maestro.

Tali doti si ritrovano amplificate e congiunte ad altre notevolissime qualità personali nel Chisini, ch'ebbe a subire minori traversie e che dell'influsso dell'Enriques potè godere più a lungo ed in tempi migliori, in connessione anche con la preparazione del loro comme trattato in quattro volumi, già citato, che gli autori scherzosamente denominavano il « librone ».

È anzitutto superfluo rammentar in questa sede, che lo vide sereno ed alacre per oltre un quarantennio, la squisita bontà e gentilezza d'animo, l'integrità e la modestia, l'attaccamento verso la scuola ed i discepoli ch'erano attributi inconfondibili della personalità di Oscar Chisini. Molte fra le sue ricerche sono state compiute in preparazione della stesura del « librone », e trovansi ivi riportate: fra esse ricorderò quella sulla risolubilità per radicali delle equazioni contenenti un parametro e quelle sulla hessiana, sui fasci di cubiche di ugual modulo, sulla composizione delle singolarità delle curve piane, sull'equazione tangenziale di una superficie dotata di singolarità, sulla decomponibilità di ogni trasformazione quadratica fra piani in fattori quadratici, sulla forma di curve algebriche reali, sulle trasformazioni di prima specie dei gruppi di p punti di una curva di genere p. I più importanti fra i risultati da lui conseguiti sono stati ripubblicati da Colleghi, discepoli ed amici in un'apposita Selezione: spiccano fra essi, oltre ad alcuni fra quelli suaccennati, le elaborate ricerche concernenti la risoluzione delle singolarità di una superficie mediante trasformazioni birazionali dello spazio e quelle relative ai piani multipli, con l'originale introduzione ed utilizzazione della teoria a lui dovuta delle trecce algebriche.

Le condizioni affinchè una curva piana algebrica risulti di diramazione per un piano multiplo vennero assegnate nel 1924 dall'Enriques, però sotto una forma che male si presta all'effettiva costruzione di tali curve. I non facili problemi che così restavano aperti vennero poco appresso affrontati da O. Zariski ed altri, e poi ripresi dal Chisini con metodi propri ispirati dal desiderio di rendere, per così dire, visibili le complesse questioni gruppali che ne seguivano.

Quest'aspirazione al concreto, che è uno degli aspetti caratteristici della multiforme personalità dell'Enriques, si ritrova ancor più accentuata nel Chisini, che non di rado si limitava a svolgere le sue dimostrazioni in casi semplici, talora soltanto numerici, onde evitare le complicazioni formali e chiarire meglio i concetti. Il Chisini aveva poi anche derivato dalle sue consuetudini col Maestro un interessamento fattivo per l'insegnamento secondario, una non comune abilità nel ricostruire in modo semplice teorie matematiche elevate accomunata a brillanti doti espositive, ed un forte intuito geometrico poggiante solidamente sul principio di continuità, e che in lui si esplicava con spiccata originalità soprattutto nell'indirizzo topologico.

Si può infine ricordare che, similmente a quanto avrebbe poi fatto lo stesso Chisini per il Conforto, fu l'Enriques ad indurre il Chisini a passare dagli studi di ingegneria da questi intrapresi nel 1908 presso l'Università di Bologna a quelli di matematica; il che ci induce a riflettere sul potenziale nocumento che la netta separazione poscia instaurata nelle nostre Università fra quei due ordini di studi, ha arrecato e continua ad arrecare nei riguardi del fiorire delle matematiche in Italia.

VI. - Nonostante l'evoluzione od involuzione dei tempi e le suddette molteplici premature scomparse, il pensiero scientifico e filosofico dell'Enriques presenta oggi ancora una sua inattesa freschezza, tanto da potersi dire sotto vari aspetti attuale e probabilmente passibile di ulteriori rilevanti sviluppi. Il germe di questi dovrà ritrovarsi in quel « pensare geometricamente » che nell'Enriques costituiva una seconda tonificante natura, modellato com'era sui grandi dell'antichità, da Talete, Pitagora e gli Eleati, a Socrate, Democrito, Platone, fino ad Euclide, Archimede, Apollonio.

Qualche indizio di ripresa in quel verso si ha già sul piano dell'insegnamento e, per ciò che concerne i più giovani, dai pregevoli contributi recenti della nipote dell'Enriques, Emma Castelnuovo, e della scuola pedagogica belga; ma molto ancora rimane da compiere. È anche prevedibile un imminente ritorno della ricerca matematica agli indirizzi della geometria algebrica promossi e caldeggiati dall'Enriques, in ispecie ora dopo che varie delle speranze riposte nella moderna scuola francese di Alexander Grothendieck non si sono compiutamente avverate. Ho in precedenza accennato come quel ritorno abbia già in parte avuto luogo nell'Unione Sovietica, e sono stati recentemente rilevati sintomi di un'evoluzione in senso analogo negli Stati Uniti d'America.

Anche indipendentemente da ciò, non va infine sottaciuto che l'opera geometrica dell'Enriques può palesarsi come una miniera in parte ancora da esplorare, quando la si consideri dal punto di vista dinamico generale precedentemente adombrato, dall'Enriques medesimo espresso dicendo che « non esiste una verità che si manifesti a noi come qualcosa di compiuto e di esatto esente da errori, ma esistono soltanto verità parziali indissolubilmente connesse all'errore, che costi-

tuiscono gradi di approssimazione, e così teorie suscettibili ognora di essere integrate in altre più ampie e precise, senza che si possa mai segnare un termine al loro perfezionamento ».

Un esempio di siffatti perfezionamenti successivi è stato da me recentemente indicato, in connessione al problema - particolare, ma sotto vari aspetti assai suggestivo - della ricerca di tutte quelle speciali curve canoniche di genere $p \geq 5$ che non risultano definibili quali interferenze delle quadriche che le contengono. Il primo passo al riguardo è stato fatto dall'Enriques in una brevissima Nota del 1919, sviluppata poi alle pp. 97-108 del vol. III del « volumone »; ma passi ulteriori al riguardo dovrebbero potersi ancor compiere, ed in parte già erano stati effettuati in un interessantissimo lavoro di K. Petri [Math. Ann., 88 (1923), 242-289] in cui tuttavia non trovasi nessun riferimento al succitato lavoro dell'Enriques. Inoltre, come già ho accennato, vari perfezionamenti ed integrazioni dovrebbero venire arrecati, ed in molte direzioni, alle questioni di razionalità trattate dall'Enriques, dal Fano e dai loro continuatori.

Sarò lieto se il presente Convegno, e gli altri indetti dall'Accademia Nazionale dei Lincei in onore dell'Enriques, varranno a stimolare rigogliosi sviluppi ulteriori nel senso dianzi prospettato.

The Ideas of F. Enriques in a Discourse Delivered by B. Segre

The contribution made by F. Enriques to the development not only of studies in geometry but also, in a far broader sense, of philosophical and scientific thought, is recalled this year - on the occasion of the centenary of the birth of the eminent scholar, who was one of the founders of this Review - in a series of Meetings promoted by the Accademia dei Lincei. The first of them - held in Milan and devoted to the more specifically geometrical aspects of the ideas of the late Master - opened with an introductory discourse delivered by the President of the Accademia dei Lincei, Prof. Beniamo Segre. We publish here the text of this clear exposition, in which the work of F. Enriques is examined in a broad historical perspective, certain that it will be welcomed by the readers of « Scientia », though we regret that lack of space compels us to omit the extensive bibliographical appendix.

I. — A century ago - to be precise, January 5th, 1871 - saw the birth, in Leghorn, of Federigo Enriques, one of most striking, original and learnedly eclectic personalities of whom Italy can boast in that period, whose influence in so many branches of scientific and philosophical knowledge will - it may be assumed - not fade but become ever greater with time. For the purpose of celebrating this centenary, the Accademia Nazionale dei Lincei - of which Enriques was a Member for forty years, from 1906 until his death on June 14th 1946 - has sponsored and organised, for this year, a series of events relating to these branches.

First of all, we have the international Meeting of Geometry, promoted, under the auspices of the Academy, by the Institute of Mathematics of Milan University, which is in fact called after Enriques. This will be followed by three other international Meetings, devoted to the History and Teaching of

Mathematics and to the Philosophy of Science, which will be held at Pisa, Bologna and Rome respectively on October 7, 8-9 and 11-12.

It certainly is not necessary to point out the reasons for the choice of these towns: Milan, where there is a flourishing school of geometry inspired by Enriques, through the work pursued here by Oscar Chisini, who was a disciple, collaborator and friend of the great man; Pisa, where Enriques was a highly promising pupil at the Scuola Normale Superiore; Bologna and Rome, where, for several decades, with singular skill and devotion, he taught at the Universities. Nor is it necessary to recall the intense keenness that Enriques managed to arouse around him in such different fields, which he succeeded in embracing in a unitary philosophical vision and dominating with portentous versatility and depth of culture, being helped in this by his refined aesthetic and humanistic sense, his quick and penetrating intuition and his enlightened faith in human rationalism, though this was not unaccompanied by diffidence or aversion towards any over-elaborate technicalism.

These copious and exceptional inclinations and gifts, which he already showed as a very young man, make it no easy task to give an exhaustive retrospective appraisal of his work; in his life they led him to engage in a lofty, multiform, systematic and indefatigable activity, albeit under the deceptive guise of a certain dispersion or laziness, and they did not avail to exempt that work from certain flaws or lacunae. But his contribution has to be gauged with no common yardstick, since it has to be related to his dynamic view of scientific and philosophical thought and to his conception of the propulsive effect of error.

In view of the nature of the present Meeting, I will not enter into much detail here regarding the more properly historical, pedagogical and philosophical aspects of the ideas and work of Federigo Enriques, which will be covered by the three Meetings at Pisa, Bologna and Rome, mentioned above. With regard to these, I will merely refer to a discourse by L. Campedelli and the *Introduction* by L. Lombardo-Radice to the Anthology of Enriques' philosophical writings and point out how many of these writings and didactic contribution have a keen bite even today, such that we may regret that, in the past, everything that could and should have been drawn from them was not put into practice; moreover, it is to be hoped that Enriques' enlivening ideas may be duly taken into account in the present process of restructurisation that is now going on in our Universities.

According to these ideas, science must be considered in what it is becoming, the reality that surrounds us is to be conceived in mathematical terms as an invariant of the correspondence between volitions and sensations, teaching must be imparted from above like a ministry but with a close association between teacher and pupil, while scientific research is to be understood as an elegantly aristocratic and creative intellectual activity in which the results, rather than the methods, are the chief concern. As I have already said, these philosophical concepts cannot be ignored in assessing Enriques' mathematical work.

This work - even if in certain periods it has not been particularly 'fashionable', and is still not so today - has undoubtedly helped to make him one of the greatest geometricians of our time; quite apart from the results, it abounds in incentives and ideas of genius, several of which have still to be fully utilised. For these reasons, the Accademia Nazionale dei Lincei has though fit to collect the best of his geometrical studies in three large volumes, the multiform richness of which may be appreciated at once from a quick glance at the analytical Index on pp. 403-423 of the third volume. These naturally do not include his fundamental posthumous volume on algebraic surfaces and his many treatises, among which it is unnecessary to mention the masterly work in four volumes written in collaboration with Chisini and the no less classic work on projective geometry.

II. — Enriques' geometrical work was so vast and complex that it is only possible for me - in the short time at my disposal - to make brief references to it or to refer to other analyses for further information. Another fact to be borne in mind is the very considerable number of skilful works written in collaboration with various authors, whence arises the problem - at times, not an easy one - of determining the extent of his share in them. This is also one of the problems, no less significant and tricky, involved in Enriques' activities in Italy and abroad in the development of geometry from the last decade of the last century up to the present day, and in the influence that his work may still have in the future.

Only thirty years before that decade, studies on geometry were somewhat neglected in Italy, especially compared with the flourishing German and French schools of geometry. An autonomous Italian school of geometry then suddenly arose as if by magic in 1860, through the efforts of Luigi Cremona, to whom are due, among other things, the transformations that bear his name and the study of various algebraic surfaces representable birationally on one plane. This school was later consolidated, enlarged and renovated by Giuseppe Veronese, Eugenio Bertini and, in particular, Corrado Segre, with important systematic developments in hyperspatial projective geometry, which had incidentally offered the last two geometricians the instruments for an original elaboration of geometry on an algebraic curve understood in the manner of Riemann and in Brill and Noether's invariant algebraic approach. This suggested undertaking similar studies on surfaces, which Noether had previously merely outlined, though for these studies, the means available being somewhat rudimentary, fresh difficulties were foreseen, which in fact turned out to be very considerable.

These difficulties were very largely overcome, and in a highly brilliant manner, thanks to the united efforts of Castelnuovo and Enriques. In fact, these two can boast of having: introduced and studied, for algebraic surfaces, the distinction between arithmetical genus and geometrical genus and hence between regular and irregular surfaces; utilised with real virtuosity the consideration of the successive adjoints of a linear system; made the invariant conception of the problems clearer and more widely operative, by making a clear distinction between absolute and relative invariance; and solved the thorny question of the so-called exceptional curves (already considered in a nebulous manner by Noether), by showing that they are of infinite number in the case, and only in the case, of rational surfaces or those referable to ruled surfaces, which are also those on which the process of adjunction terminates after a finite number of steps.

It must be pointed out that Castelnuovo, six years older than Enriques (whose sister he married), who was given a professorship at Rome University at the age of 26 (while Enriques was later to be given that of Bologna University at only 25), played a determining part from the start in directing Enriques towards the above-mentioned questions. But with portentous speed the latter became able to deal with them on a footing of perfect equality with his future brother-in-law. Thus sprang up an understanding of the rarest kind, which was to yield copious and admirable results, manifesting itself in a series of basic algebraico-geometrical works in collaboration, which appeared in rapid succession in the years 1895, 1896, 1900 and 1901 and were followed in 1906 by a considerable study of a transcendental nature, demonstrating the constancy of the irregularity of a sufficiently general surface immersed in a variety of three or more dimensions.

It is not easy to say what was the separate contribution of the two authors to their first works, in which are merged their marked characteristics of breadth of vision, refined elegance, enthusiasm and geometrical imaginative genius. However, we are able to indicate how they proceeded, in the dense obscurity that then enshrouded the problems with which they were dealing, on the basis of the following passage from the discourse delivered by Castelnuovo at the international Congress of 1928, with reference to that first group of works.

This is how Castelnuovo expresses himself on the subject: « We had constructed - in an abstract sense of course - a great number of surface models in our space or in higher spaces; and we had distributed these models in two 'windows', so to speak. One contained the regular surfaces, for which everything proceeded in the best possible manner; the analogy enabled the most salient properties of plane curves to be obtained for them. But when we tried to verify these properties on the surfaces of the other window - the irregular surfaces - our troubles began and exceptions of all kinds cropped up. In the end, a thorough study of our models had led us to guess at certain properties that must subsist, with suitable modifications, for the surfaces of both windows; we then put these properties to the test by constructing new models. If they stood up to the test, we sought, as a final stage, their logical justification. With this procedure, which resembles that followed in the experimental sciences, we succeeded in establishing certain distinctive characteristics between the two families of surfaces. It will suffice here to cite one of them: whereas on a regular surface every continuous system of algebraical curves is contained in a linear system of curves of the same order, this is not the case with irregular surfaces, which always possess continuous systems not belonging to linear systems ».

The problem of inverting this last property must then undoubtedly have presented itself to the two authors. According to a confidential account given to me much later by Francesco Severi, Enriques spoke to Severi about this in 1904 during a visit that the latter paid to the Master (who was eight years older than him and whose Assistant he had been) on the occasion of his taking up his first Chair at Parma - also at the age of twenty-five.

Severi expressed, then and there, the opinion that, in order to settle the question, use should be made of the notion of the characteristic series of a continuous system of curves: a notion that he had recently introduced as the extension - anything but immediate - of the analogous but far more elementary notion for linear systems. Enriques, having immediately understood the operative force of that new concept, at once obtained the desired result with a demonstration of the completeness of the characteristic series of a complete continuous system. This demonstration, published by Enriques in December 1904, is based on a brilliant idea (taken up and slightly simplified by Severi the following year), which led Enriques to state and establish an important « principle of degeneration »; however, it presents lacunae at one point as soon as the geometrical genus of the surface is assumed to be positive, in accordance with what Severi later showed in 1921. Nevertheless, with suitable limitations, the result still holds, as Poincaré proved in 1910 with a transcendent procedure; but there remains the fairly difficult problem of reaching this result by algebraico-geometrical methods. This question is found to be of considerable importance from various points of view, and it still remains open, although different authors, and particularly Enriques himself, have since troubled over it.

It may be observed that for such a question the use of special models proves to be of no advantage. On the other hand, it is successful in other orders of ideas: the possibility of easily *constructing* adequate models constitutes an undisputed superiority of the Italian algebraico-geometrical approach and it offered Enriques a way to achieve other spectacular successes with the discovery of very interesting and unforeseen families of surface (such as the sixth-order surface passing doubly through the edges of a tetrahedron, which has been named after him) and with the classification of algebraic surfaces from the birational point of view, in species of those of the first genera.

This is a fundamental problem that arose and imposed itself entirely spontaneously from the very beginnings of the theory, but it presents very considerable difficulty and complexity. He occupied himself with it tenaciously for a long time, achieving the most subtle results with means that were at times relatively modest, but chiefly through penetrating original ideas and an ever fervid creative imagination. The superior detachment with which he still intentionally skimmed over what he used to call « critical doubts », though it made it easier for him to obtain important results, had the effect that his studies on the classification of surfaces did not at first, in the international field, arouse all the echo that would have been due to them. However, full and qualified recognition has been accorded to them recently, after these very results had been rediscovered by I. R. Sciafarevich and his school with complete rigour and generality and with the explicit acknowledgement of the determining value of Enriques' ideas.

In this respect, it must be recalled that the first arduous steps along this road had been taken by Castelnuovo in 1893 and in 1896, when he showed the rationality of every plane involution and established that $p_a = P_2 = 0$ are (not only obviously necessary but also) sufficient conditions for the rationality of an algebraic surface. Questions similar to these arise for algebraic varieties of dimension ≥ 3 and offer rapidly increasing complexities, such that even today fairly little is known about them. It is therefore highly meritorious, on the part of Enriques, to have achieved the initial results even in such difficult extensions, by giving in 1912 a concrete example of non-rational involution in an ordinary space and assigning a sufficient condition for the unirationality of a three-dimensional variety.

To him are also due the fundamental discovery of the pluri-genera and various other no less important results, to which I will turn shortly, in discussing his influence on the development of geometry.

III. — This influence was really very great and it exercised its chief effect at the highest level upon the mathematicians of his generation. We have already seen how it had acted decisively upon Castelnuovo's mathematical production and also in no small measure upon that of Severi; many of the results of these three authors are repeatedly cited and used in Vol. II of the famous treatise by Picard and Simart of 1906, at the end of which Picard states: « MM. Castelnuovo et Enriques ont bien voulu nous donner une Note extrêmement intéressante qui complète notre étude et donne sur toutes ces questions l'état actuel de la Science ».

Particular mention must be made of the weighty and masterly work on hyperelliptic surfaces, written by Enriques in collaboration with Severi, which in 1907 was awarded the Bordin Prize of the Academy of Sciences in Paris and of which it has been asserted that it is « one of the finest works of all the Italian mathematical literature of our century ». Furthermore, it may be said that from this work has sprung a whole series of studies later carried out by numerous authors in Italy and abroad, from which have arisen the theories of Abelian and quasi-Abelian varieties and of group varieties.

Of considerable historical interest are the relationships, disparities and analogies between Enriques and one of our mathematicians of notable importance, even if he is not of such gigantic stature: Gino Fano, whose centenary also falls this year; in fact, curiously enough, he was born in Mantua on exactly the same day as Enriques.

Also Fano, a pupil of Corrado Segre, Guido Castelnuovo and Felix Klein, was very much influenced by Enriques, with whom he collaborated in certain studies. One of Enriques' early works, dating from 1893, concerns surfaces admitting an infinity of projective transformations in themselves. Shortly afterwards, this very same subject was taken up by Fano, who then also carried out similar studies on algebraic varieties of three or more dimensions. To Enriques are also due the first studies on continuous groups of Cremonian transformations in a plane, a subject that was immediately pursued by Fano and then extended by the two authors to those in an ordinary space, in an elaborate joint paper in which the reduction to types of the continuous groups of Cremonian transformations is performed by means of birational transformations.

Both Enriques and Fano occupied themselves profitably with the foundations of projective geometry, on which subject they also published a *Correspondence* between them. Among other things, to Enriques are due: the first exhaustive demonstration of the so-called fundamental theorem of Staudt, so simple that it was possible to include it in his above-mentioned university text on projective geometry; the filling of a gap encountered in Riemann's celebrated work on the principles of geometry; the article in the German encyclopedia on these principles; and an original and significant association of the logical problem with the psychological problem in the study of the same. To Fano is due the merit of having first used finite geometries in the axiomatic study of hyperspatial projective geometry, with the observation of the necessity of adding to the incidence postulates that which states that every straight line must contain more than two points (while it would not suffice to suppose the existence of a straight line containing at least three points), as well as the observation and significance of the possibility of complete plane quadrangles having the three diagonal points on a line. Fano may therefore, with reason, be considered the founder of a new chapter of mathematics - that dedicated to the study of finite geometries - which has recently had unexpected and flourishing developments and is still very far from being exhausted.

In addition, Fano wrote the two articles in the German encyclopedia on the counter-position between the synthetical and analytical approaches in geometry and on continuous groups, as well as the article in the encyclopedia of elementary mathematics on non-Euclidean and non-Archimedean geometries. With regard to non-Euclidean geometry he also published a fine monograph, which is partly related to lectures given by Enriques and to the well-known work produced by R. Bonola on this subject under the inspiration of the latter.

Emphasis must also be placed on the fact that, in the important work of 1912 mentioned above, Enriques succeeds in obtaining an irrational spatial involution by depending essentially upon a previous work by Fano on certain three-dimensional algebraic varieties having all their genera equal to zero. At the end of that work, Enriques adds that he has derived from his result the conviction of the irrationality of the general cubic hypersurface of S_4, on the lines of a continuity procedure that he briefly outlines there. And it is curious that, many years later, when I, being unaware of these precedents at that time, proposed to him the same procedure in planning a research work of mine, Enriques seemed to have forgotten his conjecture and even flatly dissuaded me from that way of approach.

Thorough studies on the cubic hypersurfaces of S_4 from the projective point of view had been carried out by Corrado Segre, Castelnuovo, Enriques and Fano. Among these, I confine myself to mentioning, as an example, the elaborate work by Fano, in which he rectifies Enriques' previous results on the pluritangential spaces of the general V_3^3 of S_4. Nevertheless, the problem of the rationality or irrationality of the general V_3^3 of S_4, already mentioned incidentally by Corrado Segre as early as 1887, cannot be said to be satisfactorily solved even today. Fano was to devote long and persistent efforts to this problem, finally coming very close to a solution and obtaining, in the process, numerous results of considerable importance.

It has come to my knowledge that Fano, as Enriques too will almost certainly have done, first tried repeatedly the path outlined by the latter, perhaps supported in this by the fact that - as is not hard to establish - a procedure similar to that followed by Enriques goes to show that, on the general cubic surface of S_3, there exists no homaloidal system of curves that are complete intersections. However, having come up against the uselessness of these attempts - on which he therefore published nothing - Fano modified the problem by including it in the broader and more fundamental problem of the rationality or irrationality of the different algebraic V_3's having all genera and plurigenera equal to zero. These V_3's include, for example, the V_3^n's of S_4 with an (n-2)-ple line, already encountered several times by Enriques; and there is also a whole series of very notable classes, of which Fano managed not only to demonstrate the irrationality but also to establish their different detachment from rationality, through a complex study of linear systems of surfaces, with all the genera equal to unity, existing on them. He kept returning to this study over a period of about forty years, with phases of an experimental nature similar to those described by Castelnuovo for his research work in collaboration with Enriques, culminating with the demonstration of the presumed irrationality of the V_3^3 of S_4 according to a working hypothesis concerning the points and base curves of the above-mentioned linear systems. The whole constitutes a weighty complex of works, for which Fano's name will be long remembered.

The questions of uni- and bi-rationality on algebraic surfaces and varieties, considered by Castelnuovo, Enriques and Fano, may be included in a broader picture, comprising subsequent studies by M. Baldassarri, U. Morin and L. Roth, who, though they make further valuable contributions, show how much still remains to be done in that field. May we remember with affection and intense regret these three talented mathematicians who, unfortunately, all died prematurely.

Another interesting subject of research pursued by Fano - following older works by Autonne - on which the last word is still very far from having been said, is that of birational contact transformations. I therefore wish to draw the attention of young mathematicians to it for future research.

Like Enriques, Fano was passionately concerned with the problems of teaching at all levels and he transfused his personality into his university lectures. Each of the two wrote a work on Descriptive Geometry. Fano's volume on the subject is perhaps superior to that of Enriques - excellent though it is - in that the subject was well suited to Fano's meticulous and precise character, which was in certain ways even too stiff and military-like, and also on account of the engineering students attending his course. Enriques, on the other hand, willingly entrusted this teaching on that subject to his Assistant, preferring to devote himself to fields in which he could abandon himself to his own particular original inspiration and have a more profound effect upon his listener, such as those offered by Projective Geometry, Complementary Mathematics (in which he held the first Chair in Italy) and Higher Geometry.

IV. — Enriques' influence upon the young in Italy and abroad was indescribable in the various fields in which he was concerned, where he showed himself to be a superb leading spirit: and this was not only through his splendid lectures and discourses and his masterly writings and treatises (almost all of the latter being translated into foreign languages) but also through his exceptional organisational abilities, the fervid passion that he brought to everything he undertook, his gentlemanly affability and the Socratic and human characteristics of his relationships with his numerous disciples, whom he knew how to choose with authority also outside the school and direct with peripatetic method, infusing them with faith in his own capabilities and recognising shrewdly and instantly the special aptitude that each of them possessed.

The influence that he had in the secondary-level teaching of mathematics and in the development of scientific and philosophical culture proved to be really enormous and was exercised largely through works that soon became famous, such as the successive editions and translations of the « Elementi di Geometria » (in collaboration with Ugo Amaldi), the « Questions in Elementary Geometry » (and later also in Elementary Mathematics) (with the co-operation of U. Amaldi, E. Baroni, R. Bonola, B. Calò, G. Castelnuovo, A. Conti, E. Daniele, A. Giacomini, A. Guarducci, G. Vitali, and later G. Vailati and O. Chisini, D. Gigli, A. Padoa, U. Scarpis, E. Bompiani, V. Notari and A. Sabbatini), and the development of the « Periodico di Matematiche », as well as the foundation of the review « Scientia » and the fostering of the success of the latter in the international field. But even greater was the mark that he left in the sphere of scientific research and university teaching, also through his numerous direct and indirect disciples that obtained university Chairs in Italy, Europe and the two Americas, and their innumerable pupils, some of whom have already achieved lasting fame, and whose scientific and teaching activity has been, and still is, closely connected with that carried on by Enriques in his time.

Among his direct disciples I will only mention the names of Pol Burniat, Luigi Campedelli, Oscar Chisini, Fabio Conforto, Alfredo Franchetta, Harald Geppert, Lucien Godeaux, Paul Libois, Giuseppe Pompilj and Oscar Zariski, as these names are sufficient to make one realise how a thorough analysis of the mark that Enriques, directly and indirectly, left on geometry would embrace a large part of the history of this discipline during the last fifty years. For reasons of time, I shall therefore be compelled to confine myself to rapid glances at the three Italians, unfortunately now deceased, among those that I have just mentioned: Conforto, Pompilj and Chisini.

V. — Fabio Conforto, a Triestino, who died in Rome in 1954 at the age of only 44, may be said to have been a first-rate mathematician, in every way worthy of the Roman school of geometry to which he belonged, whose premature loss we still feel bitterly today. After attending the Milan Polytechnic for two years, he turned to mathematical studies on the advice of Chisini, who had been able to appraise his exceptional gifts and was perhaps also responsible for the suggestion of completing those studies at Rome University, where Conforto remained until his death.

His studies were mainly carried out along Severi's lines and concern automorphic, Abelian and quasi-Abelian functions, a volume of his on modular Abelian functions being described as one of the major works published in any branch of higher mathematics in recent times.

But, especially at the beginning, he also felt - to a determining extent - the influence of Enriques, by whose views, expressed in various courses that he followed, he was inspired to produce another very fine volume in 1939, almost at the same time as he was assigned the Chair, in succession to Gaetano Scorza. This volume, entitled « Le superficie razionali », touches upon various different subjects, to which numerous research workers of various countries have contributed for over a century, unifying them in an exalted conception - going back to Enriques - which makes the relative developments almost an introduction and a test bed for a general theory of algebraic surfaces. The rich, multiform and stimulating material is re-elaborated and presented here for the first time in a systematic treatment - which may well be said to be authoritative in the subject today - admirable both for the quantity and quality of the results and for their richness and the rigour with which they have been obtained, sometimes through refined arguments and with the use of methods inspired by recent developments in algebraic geometry. And it was precisely in view of this overall exposition that Conforto - under Enriques' guidance - had previously carried out various studies, on Halphen pencils, rational double planes and elliptical surfaces.

In this volume there is also an account of an interesting study on surfaces with hyperelliptic plane sections, carried out by Giuseppe Pompilj, who - together with Franchetta (with whom he edited the above-mentioned posthumous work by Enriques) - was the last pupil of Federigo Enriques, and to whom are also due important studies on Abelian varieties, triple or multiple planes and the Cremonian transformations of a plane in itself, having a curve of fixed points. On account of the events of the war and Enriques' removal from the Chair for racial reasons, Pompilj was then obliged to turn from studies in geometry to those in statistics and probability. But also in these studies he showed himself able to preserve that broadness and originality of vision in dealing with exalted subjects, that formulation of particular questions within the framework of problems of wider scope, which he had inherited from the Master's teaching.

These gifts are found in greater degree, and added to other very remarkable personal qualities, in Chisini, who was less troubled by misfortune and who had the possibility of enjoying Enriques' influence over a longer period and in better times, also in connection with the preparation of their joint treatise in four volumes, mentioned above, which the authors jokingly used to refer to as the « librone ».

First and foremost, it is needless to recall in this institution - which enjoyed his quiet but active presence for over forty years - the rare goodness and kindness, the integrity and modesty, the devotion towards school and disciple alike, that were the unmistakable attributes of the personality of Oscar Chisini. Many of his studies were carried out in preparation for the writing of the « librone », in which they are described in detail: among them I will mention his study on the solubility, by radicals, of equations containing one parameter, and those on the Hessian equation, on pencils of cubics of equal modulus, on the composition of the singularities of plane curves, on the tangential equation of a surface endowed with singularities, on the decomposability of every quadratic transformation between planes into quadratic factors, on the form of real algebraic curves, and on the transformations of the first kind of sets of p points on a curve of genus p. The more important of his results have been republished by colleagues, disciples and friends in a special Selection: outstanding among these, in addition to those mentioned above, are the elaborate studies on the resolution of the singularities of a surface by means of birational transformations of the ambient space, and those on multiple planes with the original introduction and employment of his own theory of algebraic « trecce ».

The conditions for an algebraic plane curve to be the branch curve of a multiple plane were stated in 1924 by Enriques, though in a form that is ill-suited to the actual construction of these curves. The hard problems that thus remained open were very soon afterwards tackled by O. Zariski and others and later taken up by Chisini with methods of his own, inspired by the desire to render visible, so to speak, the complex group questions that arose from them.

This aspiration to the concrete - which is one of the characteristic aspects of Enriques' many-sided personality - is found in an even more accentuated form in Chisini, who not infrequently confined himself to carrying out his demonstrations in simple cases, at times merely numerical, so as to avoid formal complications and the better to explain the concepts. Moreover, from his habits with the Master, Chisini had derived an active interest in secondary-level teaching, an uncommon ability in reconstructing higher mathematical theories in a simple way, coupled with brilliant expository gifts, and a pronounced geometrical intuition, resting firmly upon the principle of continuity, that manifested itself in him with marked originality, especially in the topological approach.

Lastly, it may be mentioned that - just as Chisini himself was to do for Conforto - it was Enriques that induced Chisini to turn from engineering studies, which he had begun in 1908 at Bologna University, to mathematical studies; which leads us to reflect upon the potential harm that has been - and still is -

presented to the development of mathematics in Italy by the sharp division that was later to be established in our Universities between those two kinds of studies.

VI. — In spite of the evolution or involution of the actual epoch and the great number of untimely deaths, as mentioned above, Enriques' scientific and philosophical ideas still possess an unexpected freshness of their own today, such that, from various aspects, they may be considered to be of current validity and likely to be susceptible of further important developments. The germ of these ideas is to be found in the concept of « thinking geometrically », which in Enriques constituted an invigorating second nature, modelled as it was on the great men of antiquity, from Thales, Pythagoras and the Eleatists to Socrates, Democritus and Plato, up to Euclid, Archimedes and Apollonius.

There is already some indication of a resumption in that sense on the level of teaching and also, as regards the younger students, from the recent valuable contributions made by Enriques' niece, Emma Castelnuovo, and the Belgian school of teaching; but much still remains to be done. It is even foreseeable that mathematical research may soon return to the lines of algebraic geometry promoted and fostered by Enriques, especially at this present time, when certain of the hopes placed in the modern French school of Alexander Grothendieck have failed to achieve complete fulfilment. I mentioned previously how such a return has already partly taken place in the Soviet Union, and there have recently been symptoms of an evolution in a similar sense in the United States of America.

Also independently of this, it is not to be concealed that Enriques' geometrical work may turn out to be a mine that is still partly to be explored, when it is considered from the general dynamic point of view, hinted at before, which Enriques himself expressed in saying that « there does not exists a truth that reveals itself to us as something complete and exact, free from errors; there exist only partial truths, indissolubly connected with error, which constitute degrees of approximation, and hence theories always susceptible of being integrated into other broader and more precise theories, without it ever being possible to assign a term to the process of their perfection ».

I myself have recently indicated an example of such successive processes of perfection, in connection with the problem - particular, but highly stimulating from certain aspects - of the search for all those special canonic curves of genus $p \geq 5$ that cannot be defined as interferences of the quadrics that contain them. The first step in this respect was taken by Enriques in a very brief Note published in 1919, which was later developed on pp. 97-108 of Vol. III of the « librone »; but it should have been possible to take further steps in this direction, and they had already been partly taken in a highly interesting work by K. Petri [Math. Ann., 88 (1923), 242-289], in which, however, there is no reference to the above-mentioned work by Enriques. Furthermore, as I have already said, the questions of rationality considered by Enriques, Fano and their followers should be perfected and integrated in various ways and in many directions.

I shall be very happy if the present Meeting, and the others promoted by the National Academy of the Lincei in honour of Enriques, have the effect of stimulating further fruitful developments in the many-sided directions that I have here outlined.

La scienza e la cultura

L'esistenza di uno stato di conflitto fra la cultura e la scienza è un dato strettamente connesso allo sviluppo della società, oppure dipende da un modo errato di intendere i due termini del rapporto? Che tale conflitto si sia manifestato, spesso in modo drammatico, nel corso dello svolgimento storico, non pare dubitabile: basterà pensare all'artificio al quale, per sfuggire all'imperio della cultura cattolica, dovettero ricorrere i pensatori che distinsero la verità di fede dalla verità di ragione. Resta da stabilire se questa tensione sia necessaria, o se - frutto di equivoco - possa invece essere eliminata da una coraggiosa e franca opera di chiarificazione.

Un lucido riesame che valga a rimuovere gli equivoci andatisi coagulando nel corso di secoli intorno al rapporto fra cultura e scienza è stato promosso dall'Unesco, che ha indetto un convegno di studiosi sul tema: « Cultura e scienza: la diversità delle culture di fronte all'universalità della scienza e della tecnologia ». È un riesame che si impone con tanta più urgenza nel momento in cui l'esplosione delle scienze e lo sviluppo rapidissimo delle tecnologie che ne è derivato hanno posto in crisi molti valori ritenuti fin qui assodati: le stesse condizioni di vita e di pensiero hanno subito trasformazioni radicali, al punto che sono rimessi in discussione nel loro complesso i principi che guidarono fin qui l'esistenza.

L'ampiezza del fenomeno, tuttavia, non può far dimenticare che gli stessi conflitti che tormentano la nostra epoca si sono manifestati nei diversi stadi dell'evoluzione dell'umanità ed in tutte le civiltà. È, in qualche modo, la storia delle grandi scoperte, degli ostacoli che esse hanno incontrato, del modo in cui esse si sono finalmente imposte ed hanno determinato il corso dello sviluppo della società.

Il convegno promosso dall'Unesco ha perciò come cardini i temi della genesi e delle vicende della tensione fra scienza e cultura dal Medio Evo alla nostra epoca; della sociologia di questa tensione, così come essa oggi si manifesta; dei rimedi che possono essere adottati per attenuare le disarmonie le cui conseguenze sono talvolta drammatiche, così nei paesi nei quali la ricerca scientifica ha dato i maggiori frutti, come in quelli che hanno beneficiato indirettamente di sviluppi maturati altrove. I venti studiosi invitati a partecipare al dibattito dovranno, in sostanza, proporre la loro risposta alle domande che si vanno ponendo con crescente urgenza: come immaginare il rapporto futuro fra scienza e cultura? La nostra civiltà si va orientando verso una

integrazione della scienza e della tecnologia in seno ad una cultura rinnovata ed in sviluppo, oppure il conflitto si andrà inasprendo, con tutti i rischi che ciò comporta per il futuro dell'umanità?

Ci sembra comunque auspicabile che nessuna delle autorevoli personalità impegnate nel dibattito dimentichi per un solo istante che una cultura che non chieda alla scienza il rigore del metodo e la certezza di ciascuna nozione, sarebbe puro gioco, sterile svago; così come una scienza che non fosse essa stessa cultura rischierebbe di ripetere la cieca marcia sotterranea della talpa.

Science and Culture

Is the existence of a state of conflict between culture and science a fact closely connected with the development of society or does it spring from a wrong understanding of the two terms of the relationship? It would seem undeniable that, in the course of history, this conflict has revealed itself, often in a dramatic manner. It will be sufficient to think of the artifice that had to be resorted to, in order to escape the authority of Catholic culture, by the thinkers that distinguished truth of faith from truth of reason. It remains to be established whether this tension is necessary or whether, as the result of misunderstanding, it can be eliminated by a frank and courageous work of clarification.

A clear reappraisal capable of removing the misunderstandings that have been accumulating during the course of centuries around the relationship between culture and science has been promoted by Unesco, which has called a meeting of experts on the theme: « Culture and science: the diversity of the cultures with respect to the universality of science and technology ». This reappraisal is all the more urgent at this moment when the explosion of the sciences and the very rapid development of the technologies arising from them have undermined many values that had previously become consolidated: the very conditions of life and thought have undergone radical transformations, to the point that even the whole complex of principles that have so far governed existence have once more become subjects of discussion.

However, from the magnitude of the phenomenon, it cannot be forgotten that the same conflicts that torment our present age have revealed themselves in the various stages of the evolution of mankind and in all civilizations. In some ways, it is the history of the great discoveries, of the obstacles that they encountered and of the way in which they have finally become established and have determined the course of the development of society.

The meeting promoted by Unesco therefore hinges upon the themes of the origin and history of the tension between science and culture, from the Middle Ages up to the present day; of the sociology of this tension, as it appears today; and of the remedies that can be adopted to attenuate the disharmonies that at times have dramatic consequences, both in the countries in which scientific research has produced the greatest results and in those that have benefited indirectly from developments that have matured elsewhere. In substance, the twenty experts invited to take part in the debate will have to propose an answer to questions that are presenting themselves with increasing urgency: or in other words, how can the future relationship between science and culture be imagined? Is our civilisation heading towards an integration of science and technology within a renewed and developing culture, or will the conflict become more bitter, with all the risks that it involves for the future of mankind?

In any case, it is to be hoped that none of the authoritative personalities concerned in the debate may forget for a single instant that a culture that does not ask of science a strictness of method and certitude in its every notion would be a mere joke, a sterile diversion. In just the same way, a science that was not in itself culture would risk imitating the blind underground groping of the mole.

Il ruolo della scienza nello sviluppo della società

Il nuovo interesse che si va manifestando nei confronti degli aspetti qualitativi dello sviluppo economico e sociale, insieme ad una più ampia concezione del benessere, renderà necessaria una più completa cooperazione internazionale sul piano della politica della ricerca scientifica, nella misura in cui essa può determinare l'evolversi delle condizioni di vita di una società. È questa la conclusione fondamentale di un rapporto pubblicato recentemente dall'OCDE sotto il titolo: « Scienza, sviluppo e società: una nuova prospettiva ». Il documento è il risultato di uno studio compiuto da un gruppo di esperti di diversa nazionalità, presieduto dal prof. Harvey Brooks, decano dell'Università di Harvard.

Secondo il rapporto, le attuali tendenze che si traducono in un parziale declino delle preoccupazioni legate alla difesa, al prestigio nazionale ed agli aspetti puramente quantitativi dello sviluppo economico, sono destinate a consolidarsi nel corso del presente decennio. Al tempo stesso, sebbene molti vecchi bisogni della società debbano prevedibilmente mantenere una grande importanza, il rapporto afferma che i singoli paesi dedicheranno un interesse crescente ai nuovi bisogni ed alle nuove aspirazioni, in ragione della loro stessa novità e del fatto che i poteri pubblici sono in minor misura preparati a far loro fronte.

In una prima parte del documento, il gruppo di esperti definisce le ipotesi dalle quali è partito ed esamina sommariamente i problemi che ostacolano l'impostazione di una politica della ricerca scientifica sia su scala nazionale che su scala internazionale. La seconda parte è dedicata allo studio dei rapporti fra la società, lo sviluppo economico e l'evoluzione della scienza e della tecnica. Nella terza parte si esaminano criticamente le realizzazioni, le insufficienze e i limiti della politica scientifica nel corso dell'ultimo decennio. Nella quarta parte si tenta di definire il nuovo quadro e i nuovi punti di vista che, nei prossimi dieci anni, dovrebbero consentire di dare maggiore efficacia alla politica scientifica. Seguono, infine, le conclusioni e le raccomandazioni che il gruppo di studiosi rivolge all'Organizzazione ed ai paesi membri.

Il rapporto sarà fra breve oggetto di esame da parte dei ministri della ricerca scientifica dei paesi che fanno parte dell'OCDE. Esso, si dovrà dire subito, non sfugge alla colpa di una certa genericità, inevitabile per il giustapporsi di indirizzi ed esigenze diversi. Tuttavia si dovrà riconoscergli il merito di avere enunciato alcuni almeno dei problemi che si vanno ponendo in termini via via più pressanti e che i governi nazionali si trovano già oggi a dovere affrontare in termini di scelta.

« La delusione generata nel vasto pubblico davanti agli effetti dell'applicazione delle tecnologie più progredite - si legge nel documento - è verosimilmente destinata ad aggravarsi. Attualmente questa delusione si manifesta principalmente nelle recriminazioni contro alcuni fenomeni allarmanti, come la degradazione dei tessuti urbani, l'inquinamento atmosferico, il diminuito impiego di mano d'opera. Inoltre si va sempre più lucidamente ponendo in discussione il fondamento etico di una ricerca i cui risultati potrebbero servire ad una manipolazione dell'individuo o della società. La minaccia della guerra nucleare continua a pesare sulla società, mentre ancora si fa uso delle armi convenzionali, la cui capacità di distruzione va di continuo aumentando ».

Ed ancora: « La natura dei nuovi problemi e la complessità dei nessi che li uniscono creano una situazione nuova, alla quale i governi, e le strutture cui sono affidati l'insegnamento e la ricerca, non sono ancora in grado di far fronte ».

« La situazione attuale - si legge più oltre - rende necessario un riesame della posizione e del ruolo della scienza e dei particolari settori scientifici in una prospettiva culturale, poichè proprio dai risultati della ricerca e dalle loro applicazioni tecnologiche deriveranno in larga misura i mutamenti che si produrranno nella società. Poichè gli obiettivi sociali sono incerti e mutevoli, è particolarmente importante che nella loro determinazione si faccia appello alle più recenti valutazioni scientifiche delle interazioni fra tecnologia, economia e tendenze sociali. I responsabili della politica scientifica dovranno partecipare più attivamente alla determinazione dei fini della collettività e della loro graduatoria, così che quei fini siano fissati realisticamente e che scienza e tecnologia siano poste in grado di agire incisivamente ed in modo creativo ».

Non manca, come si vede, un ampio margine di astrattezza e di evasività. Nè, allo stato attuale delle cose, si riesce a vedere come scienza e tecnologia possano sostituirsi ai consapevoli atti di scelta che un equo sviluppo sociale non può in nessun caso eludere. Resta, comunque, il fatto che il rapporto potrebbe offrire ai ministri dell'OCDE l'occasione per un dibattito aperto e franco, per uno spregiudicato riesame della realtà.

The Role of Science in the Development of Society

The new interest that is being shown in the qualitative aspects of economic and social development, together with a broader conception of welfare, will necessitate a more complete international co-operation on the level of the policy of scientific research, to the extent in which it may determine the evolution

of the living conditions of a society. This is the fundamental conclusion reached in a report recently published by the OCDE under the title: « Science, development and society: a new prospect ». The document is the result of a study carried out by a group of experts of various nationalities, under the presidency of Prof. Harvey Brooks, Dean of Harvard University.

According to the report, the present tendencies that appear in the form of a partial decline in the preoccupations associated with defence, national prestige and the purely quantitative aspects of economic development, are likely to become consolidated in the course of the present decade. At the same time, although many old needs of society must be expected to retain great importance, the report states that the individual countries will devote increasing interest to the new needs and aspirations, by reason of their very novelty and of the fact that the public powers are to a less extent prepared to deal with them.

In the first part of the document, the group of experts defines the hypotheses taken as a starting-point and briefly examines the problems that hinder the formulation of a policy of scientific research on both a national and an international scale. The second part is devoted to the study of the relationships between society, economic development and the evolution of science and technology. The third part presents a critical examination of the achievements, shortcomings and limitations of scientific policy in the course of the last ten years. In the fourth part there is attempt to define the new picture and the new points of view which, in the next ten years, should give greater efficacy to scientific policy. Finally, the report gives the conclusions and recommendations that the study group presents to the Organization and the member countries.

The report is shortly to be studied by the ministers of scientific research in the countries belonging to the OCDE. It must be said at once that it does not escape the fault of a certain generality, inevitable in view of the juxtaposition of different aims and needs. However, it must be accorded the merit of having stated at least some of the problems that are being posed in increasingly pressing terms and that national governments are already having to face in terms of choice.

In the words of the document: « The disappointment aroused in the general public with regard to the effects of the application of the more advanced technologies is likely to become aggravated. At present this disappointment reveals itself mainly in recriminations against certain alarming phenomena, such as the degradation of urban networks, the pollution of the atmosphere and the reduced employment of labour. In addition, increasingly clear expression is being given to discussion of the fundamental ethics of a research whose results might serve for manipulation of the individual or of society. Society continues to be oppressed by the threat of nuclear war, while use is still made of conventional weapons, the destructive capacity of which is continually increasing ».

And again: « The nature of the new problems and the complexity of the associations that unite them create a new situation, which governments and the structures responsible for training and research are not yet able to face up to »...... « The present situation necessitates a re-examination of the position and role of science and the particular scientific sectors in a cultural perspective, since it is the very results of research and their technological applications that will largely lead to the changes that will be brought about in society. Since the social objectives are uncertain and changeable, it is particularly important that appeal should be made to the most recent scientific appraisals of the interactions between technology, economics and social tendencies. Those responsible for scientific policy will have to play a more active part in determining the aims of collectivity and their order of priority, in order that those aims may be established realistically and that science and technology may be enabled to operate in an incisive and creative manner ».

As may be seen, the report is not without a wide margin of abstraction and evasiveness. Nor can it be seen, in the present state of affairs, how science and technology can replace the conscious acts of choice that can in no case be evaded by reasonable social development. In any case, the fact remains that the report could give the ministers of the OCDE the opportunity for a frank and open debate and an unprejudiced reappraisal of the present reality.

RECENSIONI - BOOK REVIEWS

V. Somenzi (ed.), *L'evoluzionismo* (An anthology from the writings of Lamark, Darwin, Huxley and Haeckel, with historico-critical essays by Montalenti, Omodeo, Cassirer, Farrington and Medawar), (Series: *Classici della Filosofia*), Editore Loescher, Torino, 1971, xii-298 pp., L. 1.650.

This recent volume by V. Somenzi, the second edition of which is being printed after the first had sold out in only a few months, fills a wide gap in Italian philosophical culture relative to the field of study and research in Evolutionism. The anti-positivist reaction of the second half and end of the last century and the assertion, in the philosophical field in the present century, of Neo-idealism and more or less concealed forms of vitalism seemed to have relegated evolutionary problems and their associated philosophical reflections to the category of things of the past, just at the very time when the scientific and material, experimental and mathematical bases were being created for that impetuous burgeoning of contemporary biological research and the renewed resumption of evolutionistic themes that are testified by the names of Watson, Crick and Monod. An ever-increasing detachment had thus derived between philosophical reflection and the reality of science - a detachment that had ruinous repercussions in the field of teaching and culture. In this sense, filling the gap mentioned above necessarily meant trying to retie the threads that had been knowingly broken, which in the 19th century had nurtured, albeit at times dogmatically, the science of philosophy and vice-versa (at least in the specific field of evolutionistic theories). However, it also meant, implicitly, presenting a proposal, a new project (even if subdued and incomplete) for a new role of Philosophy within the framework of contemporary scientific research and teaching. Somenzi's solution of this difficult task is the result of a series of choices made in silence and in a lucid manner. First and foremost, it was a question of not indulging in the habit of criticizing, on the basis of abstract and ideological principles, an operature of fracture that had already occurred and had in fact been born from an ideological overlapping of Philosophy and Science. On the contrary, as far as possible, it was necessary that the protagonists (ideas, research facts, debates) should speak for themselves, by telling their story in such a way that the critical function of the editor actually consisted in retying the threads and not in weaving a personal and evanescent picture. In accordance with this assumption, with the article by P. Omodeo on « Darwin and the inheritance of acquired characteristics » and G. Montalenti's introduction to the new 1967 edition of « The Origin of Species », Somenzi shows the living presence of Darwin in Italian scientific culture and identifies, on the other hand, the deeper roots of a subtle avversion to the Darwinism of Italian philosophical culture rather than in the residual and discredited forces of Neo-idealism, in the anti-dogmatic prejudices of the neo-Kantian criticism of Marburg and of attitudes of generic Kantian derivation, of which Somenzi gives exhaustive examples through a return to that part of Cassirer's « History of Modern Philosophy » devoted to « The ideal of knowledge in Biology and its transformations ». This criticism, which is anti-dogmatic in intent and actually shows itself to be permeated with ideology, is ideally answered by Medawar's essay on « H. Spencer and the law of general evolution », in which, in a manner founded on scientific bases, Spencer's assessment arises from a critical examination of the present state of science; the crux of the contradictions and truths of the past is interpreted in the light of the kernel of the problems of present scientific research and not on the basis of an abstract, albeit purely methodological, criterion of truth.

Another example of « criticism from within » is provided by Farrington's chapter on « Darwin's place in the history of thought », in which the study is extended from themes of pure scientific criticism to themes of the social history of science. However, for the reader, perhaps the most « difficult » and stimulating discovery will be the resumption of the main themes of the classical evolutionistic debate through the selections, with critical notes, from Lamark, Darwin, Huxley and Haeckel. In fact, the present volume will enable the reader to realise how much of the modern world and how much of contemporary scientific theories - for example, the theory of systems - was present, in essence, as Weyl had seen, in these early works; how the Lamark-Darwin controversy, in spite of the victory for Darwin, is far from being exhausted, and how it may still prove fruitful for science.

Above all, the reader, the student and the cultivated man, will be able to discover in these writings the complex web of the philosophical motives and the philosophical concepts that subtend them: a web that is here presented as the best guide for revealing the sources, including the philosophical ones, of the problems of that contemporary science the birth of which, in the evolutionistic field, is to be found, as regards certain aspects, in these very works. In the critical « re-tying » of the broken threads, in the synthetical recapitulation of the past, in the immersing of the past in the present, in the presentation of a colloquy between old and modern writers, in the free confrontation of ideas according to a precise methodological line, the reader is thus led, in diachronic perspective, to retrace, in its internal and external associations, the course of the ensemble of evolutionistic problems; hence he is invited to contemplate first the recovery (free of ideological or metaphysical superstructures) of the systematics of the past through the critical function of the present and then, by means of the discovery of the deeper themes of discussion of the past, the elaboration of a broadening of present-day problematics outside any restrictive and unilateral direction. Here Somenzi implicitly proposes a new role for philosophy and explicitly elaborates a concrete attempt to teach it according to these new methods. Philosophical reflection is presented here as the path of awareness, a rediscovery of the roots (not those of Being but rather those of man and human society), the Baconian and Galilean destruction of the ideological scaffolding and superstructures, and a critical and conscious preparation for the exercise of rational activity. Through the work-bench of ideas as « historical facts » the student is led to forge the expressive and evolutional potentialities of his own mental functions in concrete form.

Roma, Università, Istituto di Filosofia.

A. Carsetti

W. Stegmüller, *Main Currents in Contemporary German, British, and American Philosophy*, 4th rev. ed., D. Reidel Publishing Company, Dordrecht, 1969, xviii+568 pp., Dfl. 72.

This volume is an English translation of the fourth German edition of *Hauptströmungen der Gegenwartsphilosophie,* whereof the first edition, dated 1952, underwent important changes in the course of producing a second edition in 1960. The English edition differs in turn from the fourth German edition by the addition of a rather lengthy chapter on Wittgenstein, besides an appendix expounding Chomsky's theory of language and his modern variant of the theory of innate ideas.

It contains a very accurate analysis of today's most important philosophical concepts: namely, the philosophy of Brentano, the phenomenology of Husserl and Scheler, the existentialism of Heidegger and Jasper, the critical realism of N. Hartmann, and the neo-positivism of the Vienna circle, besides analytical philosophy, and, as already stated, a full analysis of Wittgenstein's philosophy. The treatment of the last mentioned three topics is particularly deep, so as to match the importance of the problems that arise from exploring the foundations of the sciences, and, especially, the philosophical implications of researches into the foundations of mathematics and inductive inference.

As is emphasized by the author, its aim is firstly to supply the interested reader with a conspectus of present-day philosophers, and, secondly, to help the student who wishes to form a preliminary idea of modern philosophy. In our opinion the author has fully achieved both of these objectives.

Perugia, Università, Istituto di Matematica.

D. Costantini

W. V. O. Quine, *Parola e oggetto* (Series: *La Cultura*, No. 31), Il Saggiatore, Milano, 1970, xlviii+354 pp., L. 4.000.

To describe Quine's philosophy as being merely "a realistic philosophy of medium sized objects and of the theoretical entities of physics" is, as the author himself emphasizes, to confine the consideration of his *Word and Object* to one of its secondary themes while disregarding the fundamental rôle played by the idea underlying the word 'theory'. In this book, "the theories can be characterized as a fabric (or network) of sentences variously associated to one another and to non-verbal stimuli by the mechanism of conditioned response". Obviously the term 'theories' is not to be understood here in its technical sense, and it is not merely by chance that reference is made to the "mechanism of conditioned response". The relation between a scientific theory and the ideal objects postulated therein is similar to that established, in the environment of social intercourse, between ordinary thinking and the "middle sized bodies which are objects of reference *par excellence*". It is the symbolic values attaching to the objects that constitute the variables in the quantification of a theory that determine (even if incompletely) the theory itself, which, without these, would remain a purely conventional system. In direct contrast with Carnapian theories, Quine maintains that in reality no distinct line of separation can be drawn between theoretical statements and those directly linked with non-verbal stimuli. According to him there are, instead, two modes of considering a theory: the material mode, which refers (inside the theory) to whatever objects fall within the scope of its quantification variables, and the formal mode which regards it from the standpoint of simplicity and economy. The passage, which is both a natural and a necessary one, from the material to the formal mode, is one of the fundamental aspects of the process which Quine calls "*semantic ascent*". For instance, philosophy constitutes a "*semantic ascent*" from physics, the first-mentioned operating upon the words which the latter normally employs.

Roma, Università.

V. Cinanni

S. Devons (ed.), *Biology and the Physical Sciences*, Columbia University Press, London, 1969, xvi+380 pp., figs and tables, £5.63.

The present volume will be found particularly attractive since it is devoted to general questions of Biology viewed from the aspect of the relationships between Biology and the physical sciences. It is therefore aimed both at biologists and at physicists and is the account of the activities of a continuing symposium on the relationships between biology and the physical sciences, held at Columbia University between 1965 and 1967.

An introduction by S. Devons is followed by the following contributions: G. W. Beadle: Genes, chemistry and the nature of man; A. Szent-Györgyi: Fifty years of poaching in science; G. Pontecorvo: Template processes in heredity and evolution; A. Kornberg: Current problems in genetic code; D. A. Glaser: Biological control mechanisms in simple organisms; W. M. Elsasser: Atoms and organism theory; J. C. Kendrew: Physics, molecular structure and biological function; H. E. Huxley: Structural organization and the contraction mechanism in striated muscle; S. Inoué: The physics of structural organization in living cells; C. H. Waddington: Biological organization and physical systems; H. A. Schneiderman: Control systems in insect development; H. Mohr: Light and morphogenesis; G. Wald: The molecular basis of human vision; W. A. H. Rushton: Light, physiology and visual sensation; A. (Katzir) Katchalsky: Chemical dynamics of macromolecules and its cybernetic; W. A. Rosenblith: Sensory communication: quantification, concept and models; H. J. Morowitz: The physical foundations of prebiological evolution; S. L. Miller: The origin of life.

The volume concludes with a Postscript by the author of the Introduction, S. Devons, who co-ordinates the various concepts resulting from the individual articles and draws general conclusions from them.

As may be seen, the authors are all leading biologists and the contributions are all concerned with general problems - many of them being among the greatest problems in the field of Biology. It can therefore be confidently stated that the volume constitutes one of the most important works on general biology to be published, not only in recent years but of all time.

Napoli, Università, Istituto di Patologia Generale.

L. Califano

M. FLORKIN and E. H. STOTZ (ed.), *Comprehensive Biochemistry*, VOL. 21: *Metabolism of Vitamins and Trace Elements*, Elsevier Publishing Company, Amsterdam, 1971, XVI+298 pp., figs. and tables, Dfl. 65.00.

Volume 21 of the treatise on Biochemistry edited by Florkin and Stotz is entitled « Metabolism of Vitamins and Trace Elements », the association of subjects probably being due more to editorial reasons than to scientific considerations.

In accordance with the title, the work comprises two chapters. The first is concerned with the vitamins - though only the hydrosoluble ones - and is divided into seven sections: a) The biosynthesis of thiamine (G. M. Brown); b) The biosynthesis of riboflavin (G. W. E. Plaut); c) The metabolism of Vitamin B_6 (E. E. Snell and B. E. Haskell); d) Biosynthesis of pantothenic acid and coenzyme A (G. M. Brown); e) The metabolism of biotin and analogues (D. B. McCormick and L. D. Wright); f) The biosynthesis of folic acid and 6-substituted pteridine derivatives (T. Shiota); g) The metabolism of the cobalamins (L. Mervyn).

The second chapter, on Trace Elements, is by B. L. O'Dell and B. J. Campbell. An introduction is followed by an account of the present knowledge on the metabolism of the following elements: iron, copper, zinc, cobalt, manganese, chromium, molybdenum, selenium, iodine and fluorine.

From every point of view, the standard of the present work is on a level with that of the previous volumes published in this treatise on Biochemistry, which is by now of classical importance.

Napoli, Università, Istituto di Patologia Generale.

L. CALIFANO

K. BENIRSCHKE (ed.), *Comparative Mammalian Cytogenetics* (An International Conference at Dartmouth Medical School, Hanover, New Hampshire, July 29-August 2, 1968), Springer-Verlag, Berlin, 1969, XXII+474 pp., figs. and tables, DM 99.20.

The present volume contains 27 contributions by various authors that took part in an international conference held in the summer of 1968 at Dartmouth Medical School, Hanover, USA. The result is a work rich in data on the cytology of the Mammals, in its relationships with genetics, systematics and evolution.

Specialists will find here many facts on the cytogenetics of the Marsupials (Haimann and Martin), Insectivores (Borgaonkar; Gropp), Rodents (Nadler), Carnivores (Wurster), Edentates (Bernischke *et al.*), Artiodactyls (Taylor *et al.*), and Primates (Egozcue). Those particularly interested in human cytogenetics will find articles on the anomalies of the sex chromosomes in man (Sarto *et al.*), lethal chromosome errors and human karyological mosaics observed from the embryonal stages (Schwarzacher); in an article by Hirschhorn and Cohen on the chromosome aberrations induced by various mutagenic agents, the reader will find a discussion on the effects of the addition of LSD and will be able to assess the relative findings, which in my opinion are of very slight probative value. A zoogeographical naturalist will appreciate a work by Wahrman *et al.* on the distribution in Israel of various karyotypes ($2n = 52$, 54, 58, 60) of the Spalax, a Rodent that lives underground like the moles and therefore has restricted motility - the evident cause of its relative isolation - hence the establishment and distribution of the different karyotypes.

The volume will also interest a biologist that has not specialised as a karyologist, since several chapters deal with more general problems. Mayr, a naturalist that has considerable knowledge not only of the systematics but also of the phylogenesis of the Vertebrates, discusses the concept of species, the reproductive isolation that lies at the basis of speciation and the modifications in the chromosome sets that occur during evolution. Hsu (the author that has perfected modern karyological technique) and Mead explain the mechanisms of chromosomal changes in Mammalian speciation, describing the evolution of the karyotypes according to the Robertsonian type (one metacentric chromosome that divides into two acrocentric chromosomes by fission, or two acrocentric chromosomes that unite by fusion into one metacentric chromosome). With admirable conciseness, Ohno gives a precise exposition of certain fundamental problems on the evolution of the genome in Mammals, considering the possibility of duplication of individual genes as the origin of genetic redundance, which would result in a greater possibility of mutation of the new gene, which escapes the stabilising selection to which the original gene is subject and can thus assume a new function. In this way, differences in the karyotypes arise during the course of evolution and spread to an increasing extent, though these differences cannot be due to chromosome duplications or deficiencies of any type, because the heterozygotes that appear through translocations or inversions are semi-sterile. The only translocations useful to speciation are those of the Robertsonian type, because heterozygotes created by fusion of fission continue to produce balanced gametes. If these assumptions are true, it may be expected that the association groups must have already been established in the early Mammals and been fundamentally preserved up to the present day. In fact, in the group of gene loci known to be X-linked, we find various cistrons common to man and to other Mammals that are systematically very remote from man. The genes for A and B hemophilia have been found in the X-chromosome of man and of the dog; various alleles of the gene for glucose-6-phosphate dehydrogenase (G-6-PD) are carried by the X-chromosome in man, in the horse and donkey, in the hare and even in several species of birds. The gene for the enzyme hypoxanthine-guanine phosphoribosyl transferase is sex-linked in man and in two rodents, the mouse and the hamster. Finally, the locus for the Xg blood group antigen has been found in the X-chromosome of man and the gibbon, and also of the mouse.

I have dwelt somewhat upon this contribution by a well-known karyologist, in order to draw attention to the vastness of the problems that can be related with the morphological data provided by a study of chromosomes and to stir the non-specialist reader to consult this volume, which has one drawback only: its high price.

Firenze, Università, Istituto di Anatomia Comparata.

E. PADOA

L. G. Wilson (ed.), *Sir Charles Lyell's Scientific Journals on the Species Question*, Yale University Press, New Haven, 1970, 572 pp., figs., £7.85.

In 1961, while examining the correspondence and note-books of Sir Charles Lyell, at Kinnordy House, Kirremuir, in Scotland, the author found a series of seven note-books in which the famous geologist had recorded his own ideas and reflections on the problem of the transmutation of species. Wilson undertook the transcription of these notes - a task that proved very arduous - and eventually published them.

As is well known, Lyell was an innovator in the geological sciences, as chief advocate of the theory of actual causes (actualism) as opposed to the theory of catastrophes and other theories, which in order to explain geological phenomena invoke causes different from those that are really in action. Lyell, who was ten years older than Darwin, meditated at length on the problem of the extinction of species - a problem documented by the paleontological findings - and was led to consider the relationships between species and environment, thus arriving at concepts very close to evolutionism. Darwin, on his part, during the voyage of the Beagle, read with great interest the second volume of Lyell's *Principles of Geology*, published in 1832. It was only much later, in 1856, that the two men met: Lyell called on Darwin at Down House and Darwin expounded his theory of natural selection.

The seven note-books now published, which are supplied with numerous notes and an introduction summing up Lyell's work and ideas, cover the period from November 28th, 1855 to December, 1861 and testify Lyell's reflections on the problem of evolution and the toilsome and heart-searching task that was necessary in order to reconcile his religious convictions with the revelations of science. In the end, Lyell had also to accept the theory of the evolution of man by natural selection.

The historians of biology will be very grateful to Wilson for having embarked upon such an arduous undertaking in order to make accessible to everybody a document of considerable importance and great interest for the history of evolutionism and also for having provided an analysis of such an eminent scientific personality as this great English geologist.

Roma, Università, Istituto di Genetica.

G. Montalenti

K. Krishna and F. M. Weesner (eds.), *Biology of Termites*, Vols. I and II, Academic Press Inc., New York, 1969/1970, XIV+598 and XIV+644 pp., figs. and tables, $32.00 each volume.

The study of termites began with the classic monograph by G. B. Grassi and A. Sandias, (1893) entitled "Costituzione e sviluppo della società dei Termitidi" (Constitution and development of the society of Termitids). Since then, studies have become increasingly abundant, both on the two European species studied by the above authors and on the very numerous species of the other continents. A wonderful world was discovered, posing many problems for the biologist: the evolution of the social structure up to extremely complicated forms; the caste differentiation; the instincts that guide the construction of nests of an amazing complication; the phenomena of symbiosis with the flagellates that live in the intestine, with the fungi cultivated in special "gardens" and with other termitophil insects; the methods of communication between individuals, etc. The practical importance of termites - the feared destroyers of wood and cellulose and hence of many manufactured products - has contributed in prompting research on them, in the hope of finding methods of attack and defence against them.

Several authors, since 1920, have written comprehensive works on these insects, some of them being very complete and of great value. But the variety of phenomena and problems involved is such that a single person is not capable today of dealing with the whole subject. For this reason, Krishna and Weesner have assembled and co-ordinated a series of monographs by authors competent in the various sectors and have produced the present study. The volume consists of thirty chapters, written by 25 biologists, covering the more important aspects of the biology of termites, based on the works published up to the year 1968.

The volume is handsomely produced and printed and contains exhaustive bibliographical references; no library of zoology and general biology should be without it.

Roma, Università, Istituto di Genetica.

G. Montalenti

P. A. Jacobs, W. H. Price and P. Law (eds.), *Human Populations Cytogenetics*, (*Pfizer Medical Monographs*, No. 5), The University Press, Edinburgh, 1970, 326 pp., figs., £3.75.

After the discovery, made by Lejeune and Turpin in 1959, that mongoloid idiocy or Down's syndrome is due to the presence of a supernumerary chromosome in pair 21 (trisome 21), the search for chromosome anomalies in man was given a very considerable impulse. Of particular importance have been observations on the anomalous distributions of the sex chromosomes: that is to say, XX in woman, XY in man. Individuals with a single X and no Y chromosome (XO) are of the female type but show characteristics of hypogenitalism Turner's syndrome); individuals with two X and one Y chromosome (XXY) are males, affected with hypogenitalism (Klinefelter's syndrome).

Much interest has been aroused, especially on account of the possible applications in the legal field, by the observation, originally made by Patricia Jacobs, that the frequency of indiduals having the XYY set (who are males of normal appearance) is far higher among the inmates of criminal asylums than among the new-born population. This suggests that, by their genetic constitution, XYY individuals are predestined to criminality and especially to sex crimes.

This problem is still *sub judice*, as are many other questions relative to human cytogenetics. In the present monograph, highly qualified specialists discuss in seventeen reports most of these problems as they may be summed up today and also consider certain interesting aspects of the research techniques. The volume is therefore indispensable both for the human geneticist and for the general geneticist.

Roma, Università, Istituto di Genetica.

G. Montalenti

W. F. Loomis jr. (ed.), *Papers on Regulation of Gene Activity during Development*, Harper and Row, New York, 1970, 442 pp., figs., DM 28.50.

One of the most important developments in genetics in recent years is represented by applications of the facts and methods of biochemical and molecular genetics to the problems of embryo differentiation. Since every cell possesses a complete genic set, in order to understand how only certain genes come into action and others remain inactive in the different types of cell, it is necessary to postulate control and regulation mechanisms that operate in the nucleus and are sensitive to stimulations from the cytoplasm.

Since the publication of the fundamental work by Jacob and Monod (1961) on the mechanisms regulating the synthesis of proteins in bacteria, many experimental studies have been carried out on both bacteria and eukaryotic pluricellular organisms. This line of research, which has opened the way to the solution of an embryological problem that seemed to exclude any possibility of experimental study, is still being developed. Instead of writing a synthetical review, W. F. Loomis has thought fit to assemble in one volume the more significant original works on the subject. He has classified them under five sections: Endogenous Control of enzyme synthesis in eukaryotic micro-organisms; Differential synthesis of proteins in developing organs; Chromosomal differentiation; Control of enzyme content in liver cell; Hemopoiesis.

This collection will prove extremely useful to all geneticists and particularly to young biologists intending to specialise in molecular genetics and embryology.

Roma, Università, Istituto di Genetica.

G. Montalenti

J. Marmor (ed.), *L'inversione sessuale* (I diversi aspetti dell'omosessualità), Feltrinelli Editore, Milano, 1970, 350 pp., tables, L. 4.000.

This textbook in the fine series called 'Problems of Sexology' which is published by Feltrinelli in Italy contains an admirable collection of the more important papers written during recent years on the different aspects of homosexuality. Its editor, Juld Marmor, has omitted no facet of this complex of problems.

A full introduction by the author in justification of the choice of papers is followed by Demiston's outstanding work on ambi-sexuality of animals, that of Perloff on what is now known of the relations between morals and homosexuality, and the classic paper by Pare treating the phenomenon from the standpoint of genetics and chromosomes. The second part deals with it from the sociocultural angle by tracing its historical and mythological origins (Taylor and Fisher) and considering what are its particular anthropological aspects (Opler).

The clinical side is surveyed in the classic work of Sandor Rado dealing with the concept of bisexuality and in the investigations by Ovesey and Selzmar, which throw light on the clinical differentiations between 'latent' homosexuality and what is properly so called. The differences between male and female homosexuality are treated by Bierher, Wilburand Romm.

In conclusion there is a psycho-therapeutic report by Myerson and Lief which includes some reassuring data as to the curative possibilities in this field, traditionally so resistant to therapeutical action.

The volume taken as a whole provides a comprehensive and stimulating anthology of up-to-date scientific enquiries into homo-sexuality.

Bologna, Università, Istituto di Psicologia.

R. CANESTRARI

W. ROSS ASHBY, *Progetto per un cervello*, Casa Editrice Valentino Bompiani, Milano, 1970, 378 pp., figs., L. 4.500.

The word "biology", with its idea (implied in its etymology) of "science of life", is falling more and more into discredit today among scientists - even among those that continue conventionally to call themselves biologists. This fact is very understandable, since the term suggests the existence of something - *life*, in fact - that escapes all attempts to give it a scientific definition. Today a human being is defined as a self-regulating and self-adapting system; but since we know and can construct "non-living" systems endowed with these two properties, the definition cannot be reversed, as it stands; that is to say, the science of self-regulating and self-adapting systems is a far more extensive science than the traditional "science of life", which it embraces and treats from a more general point of view that is, it would seem, scientifically more valid: the operative point of view. This more general and extensive science is, in fact, cybernetics.

All this is shown, albeit marginally, in the present volume by Ross Ashby. But the author does not mention a third property that would distinguish living systems from artificial systems: self-reproduction. Is not this, by its very nature, the irreducible property that still causes us to speak of living beings and still to keep biology distinct from (for example) physics? The cyberneticists flatly deny this, asserting that the construction of a machine capable of reproducing itself - or rather, a machine that constructs another machine identical to it - is only a matter of time and patience. We shall see. In any case, this statement in no way affects the arguments presented by Ashby, who is solely concerned with the *individual* brain (which has therefore nothing to do with reproduction) and the way in which it regulates and adapts itself to its environment, by constructing a mechanical model of it which, while not reproducing its structure, reproduces - as far as possible - its functioning and reactions. In other words, Ashby does not try to open the Black

Box, since this would mean destroying it; rather, he seeks to build another Black Box (a model) that exactly reproduces the behaviour of the first, with the unconcealed hope that this other Black Box - for the very fact that *it has been constructed* - may provide information on the structure of the first Black Box. The reasoning upon which this hope rests is obviously as follows: every Black Box that *behaves exactly* like another Black Box *is like* this other Black Box. But the paralogism is obvious here, since it takes for granted the very thing that was to be proved: that the first Black Box behaves *exactly* like the second. In order to prove an assumption of this kind it would be necessary to confront the two Boxes with *all* the possible situations and see whether they actually behave in the same way in each case.

Roma, Università.

A. VERDINO

J. H. SCHULTZE (ed.), *Aktuelle Probleme Geographischer Forschung* (Series: *Geographische Abhandlung*, No. 13), Dietrich Reimer Verlag, Berlin, 1970, 550 pp., figs. and plates, DM 60.

To mark the sixty-fifth birthday of Professor J. H. Schultze of Berlin University, who is well known for his studies of Great Britain, Eastern Africa and geographical methods, 28 colleagues (including some 20 from Germany and the remainder from 6 other countries) have written a collection of papers in his honour. Some are concerned with general aspects of method, such as those of E. Winkler on the systematics of earth sciences and geography (pp. 19-36), R. E. Dickinson on problems of urban geography (pp. 189-211), and A. Gerstenhauer (pp. 125-135) on the genesis of poljes in tropical areas with seasonal rainfall; others deal with regions of Africa, for example K. Kaiser on the explanation of surface features in Tibesti (pp. 147-188), H. Hecklau on irrigation in Kenya (pp. 475-492), and D. O. Müller on studies on the southern border of Uganda (pp. 493-513). It is impossible to list all the papers, but mention should also be made of the contributions by G. Niemeier on Salzgitter and Wolfsburg, the home of the Volkswagen (pp. 219-234), and by B. Hofmeister (pp. 345-363) on the structure and evolution of the great cities of North America. There is also a list of publications by Prof. Schultze himself.

Roma, Università.

E. MIGLIORINI

H. G. GIERLOFF-EMDEN, *Mexico* (Eine Landeskunde), Walter de Gruyter & Co., Berlin, 1970, xxiv+634 pp., figs. and tables, DM 136.

This is a valuable review of our knowledge of Mexico, based on the author's long familiarity (since 1955) with the places he describes and on precise research in universities of the United States. It contains over 1400 titles of previous publications, headed by the classic of A. von Humboldt (*Essai politique sur le royaume de la Nouvelle Espagne*). In Spanish there is the four-volume work by Tamayo (1962), but Gierloff-Emden is more manageable and deeper, if a little encyclopaedic. The arrangement is the customary one. A long chapter on the nature of the country (pp. 15-128) is followed by an extensive treatment of its history (pp. 128-237), leading to the discussion of present-day Mexico, its people and its economic problems (pp. 238-521). The most original chapters are those dealing with the use of space and especially with hydraulic systems. Agricultural problems are also thoroughly examined. There are excellent illustrations, both photographs (including many taken from the air) and maps (including an original map on a scale of 1 : 5,500,000). It is a book which will long remain valuable.

Roma, Università.

E. Migliorini

J. Dreyer, *Storia dell'astronomia da Talete a Keplero* (Series: « *I fatti e le idee, Saggi e biografie, Storia della Scienza* »), Feltrinelli Editore, Milano, 1970, 416 pp., figs., L. 4.000.

It is the reissue in Italian version, operated by Libero Sosio of the now classic work by J. Dreyer, former director of Armagh Observatory, Ireland. The original title of his work, published in 1906, reads *History of the Planetary Systems from Thales to Kepler*. Being widely requested by people with interest in the history of astronomy, it had already undergone a new printing in its original language by *Dover Publications*, 1953.

The work is one among the really few, according to Stahl's preface, if not the sole adequately dealing with the development of cosmological conceptions since the most ancient times till Newton's age.

The first foundations of the Greek astronomical science date back to Thales, in his position of initiator of the Ionian school. Beginning then from Thales, one finds in Dreyer's book the full history of the growth and improvement of cosmological ideas, come to a standstill of many centuries with Ptolomaeus and, at last, to the new Copernican conception, already proposed by Aristarchus from Samos. But Kepler's arrival was yet necessary to provide full explanation of planetary motions in the solar system. The present edition, in its elegant veste, will be appreciated by Italian students, specially by those preferring works translated into Italian.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

T. Bastin (ed.), *Quantum Theory and Beyond* (Essays and Discussions arising from a Colloquium), Cambridge University Press, London, 1971, x+346 pp., figs., £5.00.

This book contains the proceedings of a colloquium on conceptual foundations of the quantum theory, held at Cambridge in July 1968, in which a limited number of specialists from various countries were invited to participate.

The greater part of the discussion was concerned with measurement theory. In the quantum theory, it is in general possible to use the probability amplitude (also called the state function) to calculate the probability that a given operation of measurement of a physical quantity pertaining to a microscopic object, such as the energy of a particle, will give a certain result. It is just the measurement process which clearly distinguishes the properties of a macroscopic body from those of a microscopic object (an atom or a fundamental particle). The properties of a macroscopic body can be determined by suitable direct measurements on it with an instrument that does not cause any perceptible perturbation; but to determine an observable property of a microscopic object it has to be made to interact with a macroscopic body (measuring instrument) whose interaction with the microscopic object causes it to undergo macroscopic modifications. These, however, depend on, indeed are determined by, the state of the microscopic object.

The theory of such a process is quite complicated, and was first worked out by von Neumann in 1927. Recently, however, this theory has been criticised and alternative theories proposed: one of them, by Loinger and Prosperi, remains essentially within the orthodox Copenhagen interpretation of the quantum theory; another, by Bohm, is based on the significance of 'hidden variables'.

At the colloquium, these two viewpoints were discussed by the participants in a both careful and lively manner.

The final section deals with more general philosophical aspects of physics, and the contributions by von Weizsäcker, Bunge and Bastin seem particularly interesting.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. CALDIROLA

O. SVELTO, *Principi dei Laser*, Tamburini Editore, Milano, 1970, VIII+262 pp., figs., L. 3.200.

This book contains an account of the physical principles on which the operation of the laser is based, together with a useful discussion of the properties of the radiation generated by lasers and a mention of the chief applications in both basic and applied sciences.

The treatment is clear, where possible avoiding the formal difficulties due to the typically quantum nature of the phenomena on which laser science is founded. In consequence, the reader having only a very elementary knowledge of quantum mechanics can use Svelto's attractive book to acquire some useful knowledge of the proper use of the laser and some basic ideas on the new subject of quantum electronics.

For these reasons, and because of the considerable and growing significance of lasers in various fields of pure and applied science, this book may be useful to many readers, ranging from physicists and engineers to teachers in secondary schools and technical schools.

Milano, Università, Istituto di Science Fisiche « A. Pontremoli ».

P. CALDIROLA

L. **Daboni**, *Calcolo delle probabilità ed elementi di statistica*, Unione Tipografico-Editrice Torinese, Torino, 1970, xvi+388 pp., figs., L. 9.500.

This treatise on the calculus of probabilities should be particularly welcome and useful to those who wish to learn the basic points with certainty and to get a clear view of some more advanced topics. These needs are met by the two parts of each of the eleven chapters: text and appendices. In the text, subjects are discussed at length and very clear proofs are given; in the appendices are informative comments on further cases and problems, using simple illustrations or outlines of proofs.

The author's declared standpoint is the subjective-Bayesian one, but he also refers to the interpretations which correspond to the objectivist (classical or statistical) treatment, and the approach is in general 'neutral', not preventing anyone from adopting whichever interpretation is preferred. The same applies to the second part (*Elements of statistics*, consisting of chapters X and XI), although here the opposition of the Bayesian and objectivist views of problems is bound to be very obvious (at least in the formulations; it is in general unlikely that they will reach quite different conclusions).

Roma, Università, Istituto Matematico.

B. de Finetti

G. **Cullmann**, *Recherche opérationnelle* (Théorie et pratique), Masson & C.ie, Paris, 1970, vi+244 pp., figs. and tables, 80 F.

The object of this book is to give a succinct account of the methods that are collectively termed operational research. Generally speaking these relate, as is well known, to problems arising in the management of undertakings, and the manner of dealing with these questions is eminently of a type that can be applied in practice.

After having reminded the reader of various mathematical devices such as matrix calculations and the calculus of probabilities, etc., the author deals with some problems that are characteristic of modern management such as the theories of queueing, stock control, linear programming, transport problems, and others. In conclusion a very brief account of game theory is given.

Perugia, Università, Istituto di Matematica.

D. Costantini

H. **Jeffreys**, *The Earth* (Its Origin, History and Physical Constitution), 5th ed., Cambridge University Press, London, 1970, xii+526 pp., figs. and plates, £7.00.

The new updated edition of Sir Harold Jeffreys' book once more confirms the correctness of the physical and mathematical approach of the author's analysis in all problems for which the experimental data (e.g. seismic or gravimetric) are sufficiently extensive and accurate to justify such an analysis. For instance, the results given by the orbits of artificial satellites are in agreement with the author's estimates regarding the figure of the Earth.

There are, however, problems where experimental data are almost non-existent or are difficult to interpret (for example, those relating to the strength or viscosity of materials beneath the Earth's crust) or are incomplete and local and sometimes also capable of various interpretations (for example, many geological - particularly tectonic - data, and those concerning the ocean beds). In this field, during the last twenty years, other geophysicists have come to accept continental drift hypotheses and theories, on the basis of the results of new research in oceanography and palaeomagnetism. This raises again the question of the origin of the continents and oceans, their stability in geological times, the formation of mountain chains, and other problems concerning the Earth as a whole. The interpretations of these new data often seem pushed to extremes which, though logical, must be more reliably verified before being extended to global concepts. Jeffreys takes a strongly critical attitude to such concepts, but seems to deny the validity of the new data mainly because they have been made the basis for interpretations which he regards as incorrect. His criticism thus seems ill-founded, especially as his own physical and mathematical treatment of these problems is based on premises which are not always entirely justifiable from the naturalistic point of view. The author's pre-eminent position then leaves the difficult choice between unexceptionable deductions from arbitrary assumptions and over-ambitious interpretations of objective data.

Torino, Università.

A. Debenedetti

T. N. V. Karlstrom and G. E. Ball (eds.), *The Kodiak Island Refugium* (Its Geology, Flora, Fauna and History), The Ryerson Press, Toronto, 1969, xiv+262 pp., figs., tables and plates, $10.00.

The refugia, which made it possible for various animal and vegetable species to survive in the course of one or more glacial epochs, have generally been identified first by zoologists or botanists. On the contrary, the closed refugium of "nunatak" type situated in the western part of Kodiak Island (Alaska) was discovered and studied by the geologist T. N. V. Karlstrom and collaborators (1961-1964), which considerably facilitated the work of biologists, who, in the summer of 1962, carried out a series of studies there, together with Karlstrom himself. Though the expedition lasted less than a month, it was possible to collect a vast quantity of data, thanks to the efficiency of those taking part, their thorough knowledge of the area, the excellent weather conditions and the use of aircraft and helicopters.

For various reasons, certain groups of plants and animals failed to provide significant data, whereas useful results were obtained from the study of the vascular plants and Carabid beetles, some of which would appear to have survived the last - and, in some cases, also the penultimate - glaciation on the southern slopes of the refugium, at low altitude. From there, during the postglacial period, they probably migrated later to the higher areas of the refugium, where they are mostly to be found at present. It is also likely that the fish *Salvelinus alpinus* is to be considered a glacial relic. However, most of the species present today in the refugium must be considered as postglacial immigrants; these include, for example, all the Mammals.

The volume contains contributions by T. N. V. Karlstrom (Introduction. Regional setting and geology), C. H. Lindroth (The biological importance of Pleistocene refugia. An analysis of the Carabid beetle fauna of the refugium. Concluding remarks concerning the importance of the Kodiak Island refugium for the survival of the biota.), C. H. Lindroth and G. E. Ball (An annotated list of invertebrates of the Kodiak Island refugium), G. E. Ball (The species of the Subgenus *Cryobius* of the Kodiak Archipelago: *Pterostichus*, Carabidae, Coleoptera), E. Hultén (Vascular plants), H. Persson (Bryophytes), H. Krog (A list of Lichens from the Kodiak Island refugium), D. K. Hilliard (Studies on Chrysophyceae from some ponds and lakes in Alaska. III. New or unusual Chrysophytes from Pinguicola Lake, Kodiak Island, Alaska), J. D. McPhail (The fishes of the Kodiak Island Refugium) and R. L. Rausch (Origin of the terrestrial Mammalian fauna of the Kodiak Archipelago).

Firenze, Università, Istituto di Zoologia.

B. LANZA

LIBRI RICEVUTI - BOOKS RECEIVED

Le opere elencate nella rubrica "Libri ricevuti,, non vengono raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

The works listed in the section "Books received,, are not grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

ADAMS J. A. S. and GASPARINI P. - Gamma-Ray Spectrometry of Rocks.
ALLEN C. and JOHNSON R. W. (eds.) - African Perspectives.
AMIEL J. - Cours de Chimie.
Atlas der mittel-und jungtertiären dispersen Sporen- und Pollensowie der Mikroplanktonformen des nördlichen Mittelleuropas.
BADINA O. and NEAMTU O. (eds.) - D. Gusti, Opere.
BARRIE M.-L. - La kinésithérapie de la maladie de Parkinson.
BASTIN T. (ed.) - Quantum Theory and Beyond.
BERCIU D. - Arta traco-getica.
BOMET C. - Exposés schématiques de soins pré et post-opératoires.
BORGER R. and CIOFFI F. (eds.) - Explanation in the Behavioural Sciences.
BOSCOLO P. - Cibernetica e didattica.
BURNS R. G. - Mineralogical Applications of Crystal Field Theory.
BURRIDGE K. - New Heaven, New Earth.
BURSELL E. - An Introduction to Insect Physiology.
CARNAP R. - Analiticità, significanza, induzione.
CHIRGWIN B. H. and PLUMPTON CH. - A Course of Mathematics for Engineers and Scientists.

Ciranna C. - Federico Froebel.
Comtet L. - Analyse Combinatoire.
Coury Ch., Bonniot R. and Coletsos P.-J. - La tuberculose pulmonaire et son traitement.
Crick F. - Uomini e molecole.
Crow J. F. and Kimura M. - An Introduction to Population Genetics Theory.
Demazure M. and Gabriel P. - Groupes algébriques.
Deuve J. - Serpents du Laos.
Dobzhansky T. - Genetics of the Evolutionary Process.
Doligez M. and Doligez G. - Electromagnétisme.
Dollfus A. (ed.) - Surfaces and Interiors of Planets and Satellites.
Dousset H. and Plard C. - Vademecum encyclopédique du médecin praticien.
Eccles J. C. - Facing Reality.
Edelen D. G. B. and Wilson A. G. - Relativity and the Question of Discretization in Astronomy.
Ehrlich P. R. and Ehrlich A. H. - Population, Resources, Environment.
Ewer R. F. - Ethology of Mammals.
Félix L. - Notions de mesures et nombres réels.
Ferrio C. - Trattato di psichiatria clinica e forense.
Florkin M. and Stotz E. H. (eds.) - Comprehensive Biochemistry.
Froissart M. (ed.) - Hyperbolic Equations and Waves.
Genermont J. - Recueil d'exercices de génétique des populations.
Gilbert J. (ed.) - The Complete Aquarist's Guide to Freshwater Tropical Fishes.
Guérin P. - Planètes et satellites.
Gulian C. I. - Hegel.
Hall A. S. - An Introduction to the Mechanics of Solids.
Handbook of Sensory Physiology.
Harant H. and Delage A. - Parasitologie médicale et pathologie exotique.
Harris H. - The Principles of Human Biochemical Genetics.
Hilton P. - General Cohomology Theory and K-Theory.
Höhler G. (ed.) - Springer Tracts in Modern Physics.
Holden A. - La nature des solides.
Hörmann H. - Psycholinguistics.
Horn G. and Hinde R. A. (eds.) - Short-term Changes in Neural Activity and Behaviour.
Huetz de Lemps A. - La végétation de la terre.
Jeffreys H. - The Earth.
Karlstrom T. N. V. and Ball G. E. (eds.) - The Kodiak Island Refugium.
Kemp R. - Cell Division and Heredity.
Kervran C. L. - Alla scoperta delle trasmutazioni biologiche.
Kleber W. † - An Introduction to Crystallography.
Landolt-Börnstein.
Leghissa S. - Morfologia dei vertebrati.
Le materie scientifiche nelle scuole secondarie.
Le Portugal.

LERNER I. M. - Genetic Homeostasis.

LIDZ T. and EDELSON M. (eds.) - Training Tomorrow's Psychiatrist.

LIPPARD S. J. (ed.) - Progress in Inorganic Chemistry.

LOCKWOOD A. P. M. - Aspects of the Physiology of Crustacea.

LONCHAMP J.-P. - Thermodynamique et introduction à la physique statistique.

LORRAIN P. and CORSON D. R. - Electromagnetic Fields and Waves.

MANNING A. - An Introduction to Animal Behaviour.

MARGULIS L. - Origin of Eukaryotic Cells.

MASTERS W. H. and JOHNSON V. E. - Patologia e terapia del rapporto coniugale.

MATIJEVIC E. (ed.) - Surface and Colloid Science.

MENZEL W. - Theorie der Lernsysteme.

MERCIER R., VALETTE J. and VANNEUVILLE G. - Affections vasculaires des membres et kinésithérapie.

MOSINGER M., DUMOULIN J., de BISSCHOP G. and CLAUSES I. - L'exploration neuro-musculaire.

MOTOKAWA K. - Physiology of Color and Pattern Vision.

MUSU A. M. - Motivi e strutture dell'istruzione.

NANTET J.-P. - Ordinateurs en temps réel.

NEUMAN M. - Vade-Mecum des antibiotiques et agents chimiothérapiques anti-infectieux.

O'MALLEY J. (ed.) - Critique of Hegel's 'Philosophy of Right' by Karl Marx.

Organization and Control in Prokaryotic and Eukaryotic Cells.

ORLOFF S. - Etude sur l'ostéolathyrisme.

PEREZ CH. - Répertoire des variations biologiques en pathologie clinique.

PERRY N. W. JR. and CHILDERS D. G. - The Human Visual Evoked Response.

PÉTRACHÈNE M. and TRIFONOV E. - Applications de la théorie des groupes à la mécanique quantique.

Physique des dispositifs à semiconducteurs.

REID C. - Hilbert.

ROSSI DELL'ACQUA A., TORNAGHI L., PALLOTTINO P. and CAPELLO C. (eds.) - Schede di lavoro per il secondo ciclo.

ROUBAULT M. - Peut-on prévoir les catastrophes naturelles?

RUSINOV V. S. (ed.) - Electrophysiology of the Central Nervous System.

RUSSEL F. S. and YONGE M. (eds.) - Advances in Marine Biology.

SACHS L. - Statistische Methoden.

SALMON AM. - Philippe de Beaumanoir.

SALZANO F. M. and FREIRE-MAIA N. - Problems in Human Biology.

SCHULTZE J. H. (ed.) - Aktuelle Probleme Geographischer Forschung.

SOMENZI V. (ed.) - L'evoluzionismo.

STANCOVICI V. - Ideea de numar.

SUTHERLAND J. M. and TAIT H. - The Epilepsies.

TAVERNIER J. and CALECKI D. - Introduction aux phénomènes de transport linéaires dans les semiconducteurs.

The Creative Force of the Leninist Ideas.

TUSQUES J. - Initiation à la psychologie médicale.

VACHET A. - L'idéologie libérale.
WATT K. E. F. - Ecology and Resource Management.
WEGENER A. - The Origin of Continents and Oceans.
WHITE R. S. - Space Physics.
WHITTERIDGE G. - William Harvey and the Circulation of the Blood.
WILLIAMS J. G. - A Field Guide to the Butterflies of Africa.
WILLIAMS J. G. - Säugetiere und seltene Vögel in den Nationalparks Ostafrikas.
WILLIAMS R. A. - Handbook of the Atomic Elements.
WOOLFSON M. M. - An Introduction to X-Ray Crystallography.
YAPP W. B. - The Life and Organization of Birds.

ADAMS, J. A. S. and GASPARINI, P., **Gamma-Ray Spectrometry of Rocks** (Series: *Methods in Geochemistry and Geophysics*, No. 10), Elsevier Publishing Company, Amsterdam, 1970, XII+296 pp., figs. and tables, Dfl. 65.00.

The aim of this book is to summarize all the information necessary for designing a laboratory for gamma-ray spectrometry of natural samples, planning a research program for mineral exploration, or just dealing with natural radioactivity. Special emphasis has therefore been given to tables and figures, through which the decay schemes of natural radioisotopes and the characteristics of different detectors have been illustrated. A chapter has been devoted to the main collaborative techniques, and their auxiliary relationship to gamma-ray spectrometry is discussed. Case histories on the application of gamma-ray spectrometry to different geological problems conclude the book.

ALLEN, C. and JOHNSON, R. W. (eds.), **African Perspectives** (Papers in the History, Politics and Economics of Africa Presented to Thomas Hodgkin), Cambridge University Press, London, 1970, XX+440 pp., figs. and tables, £5.25.

Scholars from all over the world contributed to the Festschrift volumes presented as a tribute to Thomas Hodgkin on his sixtieth birthday. This book contains a selection from those contributions, reflecting the wide scope of Hodgkin's own interests, work and influence in the field of African and Tiers Monde studies. All the contributions represent original and previously unpublished work in the fields of African history, Islamic studies and the sociology of religion, and also of contemporary political and economic studies. They are particularly concerned to examine questions of a general and theoretical importance, often by means of detailed case studies. Among the countries thus discussed are Ghana, Guinea, Nigeria, Senegal, Guiné-Bissao, Zambia, Ivory Coast, Tanzania, Gambia —and, as part of the African diaspora, Jamaica. Fittingly, these essays share one of the main features which have distinguished Thomas Hodgkin's own work— they take a critically evaluative view of some of the established interpretations and conventional wisdoms surrounding their subjects.

CONTENTS. - I. Historical Perspectives. *I: Islam in the History of West Africa.* - *II: Colonial Misfortune and Religious Response.* - II. Political and Economic Perspectives. *I: Political Science and Political Economy.* - *II: Case-Studies of Political Change.*

AMIEL, J., **Cours de Chimie**, VOL. II: **Chimie générale** (Fin). **Chimie descriptive** (Series: *Dunod Université*), Dunod, Paris, 1971, VIII+600 pp., figs., 74 F.

CONTENTS. - I. L'édifice chimique. *I: Catalyse hétérogène. - II: Notions de thermodynamique chimique. - III: Les ions en solution. - IV: Piles; potentiels des électrodes.* - II. Chimie minérale. *V: Classification périodique des éléments. - VI: Hydrogène. - VII: Eléments du groupe I A: métaux alcalins. - VIII: Eléments du groupe II A: métaux alcalino-terreux. - IX: Eléments du groupe III B. - X: Eléments du groupe IV B. - XI: Eléments du groupe V B. - XII: Eléments du groupe VI B. - XIII: Eléments du groupe VII B. - XIV: Eléments de la colonne O: gaz rares. - XV: Eléments de transition. - XVI: Lanthanides. - XVII: Transuraniens. - XVIII: Complexes minéraux. - XIX: Mécanismes des réactions minérales.* - III. Chimie organique. *XX: Fonctions en chimie organique. - XXI: Mécanismes des réactions organiques. - XXII: Fonctions multiples. - XXIII: Fonctions mixtes. - XXIV: Hétérocycles fondamentaux. - XXV: Macromolécules organiques.*

Atlas der mittel-und jungtertiären dispersen Sporen- und Pollensowie der Mikroplanktonformen des nördlichen Mitteleuropas, VII: Monoporate, monocolpate, longicolpate, dicolpate und ephedroide (polyplicate) Pollenformen, Veb Gustav Fischer Verlag, Jena, 1970, 176 pp., figs., tables and plates, DM 99.

CONTENTS. - *I: Einleitung und allgemeine Vorbemerkungen zu den Pollenlieferungen sowie zur Lieferung VII im besonderen. - II: Bemerkungen zu den Pollengenera der Lieferung VII. - III: Botanische Deutung und taxonomischer Charakter der im Abschnitt B behandelten, revidierten bzw. neu beschriebenen Pollengenera. - IV: Taxonomie der Pollenspezies etc. der im Abschnitt B genannten Pollengattungen des mitteleuropäischen Mittel- und Juntertiärs. - V: Literaturnotizen.*

BADINA, O. and NEAMTU, O. (eds.), **D. Gusti, Opere,** VOL. IV, Editura Academiei Republicii Socialiste România, Bucuresti, 1970, 418 pp., Lei 34.

CONTENTS. - *I: Sociologia Natiunii. - II: Sociologia Relatiilor Internationale. - III: Sinteza Cunoasterii si Actiunii. - IV: Stiinta Pacii.*

BARRIE, M.-L., **La kinésithérapie de la maladie de Parkinson,** Librairie Maloine S.A., Paris, 1970, 114 pp., figs., 40 F.

Cet ouvrage didactique décrit, dans un ordre rationnel, tout ce qu'un masseur-kinésithérapeute rééducateur doit connaître sur la maladie de Parkinson. Il se divise en trois parties: La première partie est un rappel anatomique de l'atteinte des centres cérébraux touchés qui régissent la motricité et décrit le syndrome parkinsonien. La deuxième partie concerne la fiche du parkinsonien comprenant quatre feuillets: 1er, questionnaire, test de déambulation et d'équilibre; 2e, schémas d'attitude; 3e, test fonctionnel; 4e, test d'amplitude articulaire. La troisième partie, la plus importante, se divise en plusieurs chapitres. Le 1er chapitre fournit les principes généraux des soins physiques à dispenser aux extra-pyramidaux. Le deuxième chapitre passe en revue les traitements employés par le rééducateur (mouvements actifs, mouvements passifs, massages, infra-rouges et chaleur). Le troisième chapitre indique la pédagogie des cinèses, qualités, rythme, plan de travail du kinésithérapeute, quantité des cinèses, choix de celles-ci. Le quatrième chapitre intitulé: « Mode d'application », traite des quatre stades supposés de la maladie, se poursuit par la description de cinèses complémentaires classés dans l'ordre suivant: décubitus, procubitus, positions diverses et station debout, il se termine par l'indication de techniques particulières concernant le travail des mains et des muscles faciaux.

BASTIN, T. (ed.), **Quantum Theory and Beyond** (Essays and Discussions arising from a Colloquium), Cambridge University Press, London, 1971, x+346 pp., figs., £5.00.

Quantum theory attempts to describe the discrete or atomic nature of matter and the physical world. Certain paradoxes connected with the use of our familiar ideas of the theory have led some physicists to suggest that a revision of quantum theory at its

most fundamental level is now inevitable, while others think that the wide range of experimental success of the theory make such changes literally unthinkable. This book contains the edited papers presented at a small informal colloquium held in Cambridge in 1968 to discuss the need for fundamental revision in quantum theory. Most schools of thought on the foundations of the theory were represented, and to direct discussion some participants proposed actual changes. A principal aim was to pinpoint the sources of difficulty in current ideas or, failing that, to present alongside each other the various viewpoints about them. Indications of the trends emerging from the discussions are suggested in editorial introductions to each part of the book.

CONTENTS. - *I: Introduction.* - *II: Niels Bohr and Complementarity: The Place of the Classical Language.* - *III: The Measurement Problem.* - *IV: New Directions within Quantum Theory: What does the Quantum Theoretical Formalism Really tell us?* - *V: A Fresh Start?* - *VI: Philosophical Papers.*

BERCIU, D., **Arta traco-getica** (Series: *Biblioteca de Arheologie*, No. 15), Editura Academiei Republicii Socialiste România, Bucuresti, 1969, 234 pp., figs., Lei 31.

Le but de cet auvrage, écrit en roumain et qui contient un résumé en français, a été, en premier lieu, de rassembler tous les monuments d'art thraco-gétique, découverts jusqu'à maintenant sur le territoire de la Roumanie. En second lieu, l'auteur a procédé à une nouvelle interprétation, plus conforme à ses propres vues, de certaines trouvailles plus anciennes, tel le « trésor » dit de Craïova et le rhyton de Poroïna. L'auteur estime que les Thraco-Gètes ont créé un art propre, indépendant de celui des Celtes, Scythes ou Grecs, et qui est caractérisé par des traits d'une grande originalité et vitalité, bien que le style animalier thrace, respectivement thraco-gétique, ait été influencé par les Scythes, et surtout par la toreutique grecque et perse.

BOMET, C., **Exposés schématiques de soins pré et post-opératoires,** 2nd ed., Librairie Maloine S.A., Paris, 1971, 288 pp., 38 F.

BORGER, R. and CIOFFI, F. (eds.), **Explanation in the Behavioural Sciences,** Cambridge University Press, London, 1970, XII+520 pp., figs. and tables, £5.

There are sharp differences among psychologists, sociologists, philosophers and others concerned with the behavioural and social sciences, not simply about the adequacy of particular explanatory systems, but about the very forms of explanation appropriate in this field. The editors have therefore invited a number of distinguished contributors to bring their views into direct confrontation—each chapter takes the form of an exposition, a comment, and a final reply by the first author.

CONTENTS. - *I: Reasons and Causes,* by S. TOULMIN and R. S. PETERS. - *II: The Explanation of Purposive Behaviour,* by CH. TAYLOR and R. BORGER. - *III: Is the Brain a Physical Systems?,* by N. S. SUTHERLAND and J. H. GRUNDY. - *IV: Conditioning and Behaviour,* by D. W. HAMLYN and A. J. WATSON. - *V: Imperfect Rationality,* by J. WATKINS and A. DONAGAN. - *VI: Understanding and Explanation in Sociology and Social Anthropology,* by I. C. JARVIE and P. WINCH. - *VII: Situational Individualism and the Emergent Group-Properties,* by J. O. WISDOM and R. BROWN. - *VIII: The Relevance of Psychology to the Explanation of Social Phenomena,* by G. C. HOMANS and P. M. BLAU. - *IX: The Skinnerian Analysis of Behaviour,* by R. A. BOAKES, M. S. HALLIDAY and K. H. PRIBRAM. - *X: Explanation and the Concept of Personality,* by H. J. EYSENCK and D. BANNISTER. - *XI: Problems of Explanation in Linguistics,* by N. CHOMSKY and M. BLACK. - *XII: Freud and the Idea of Pseudo-Science,* by F. CIOFFI and B. A. FARRELL.

BOSCOLO, P., **Cibernetica e didattica** (Series: *Educatori Antichi e Moderni*, No. 245), La Nuova Italia Editrice, Firenze, 1969, XVI+294 pp., L. 1.700.

Quest'opera si articola in tre parti: la prima presenta i concetti fondamentali della cibernetica e della teoria dell'informazione, la seconda alcuni importanti contributi che queste modernissime discipline danno ai problemi dell'« apprendere-insegnare », alla tecnologia educativa e, su un piano più propriamente didattico, all'approccio cibernetico alle tradizionali materie di studio; nell'ultima parte emergono i nuovi problemi che la cultura tecnologica impone alla riflessione filosofica e pedagogica.

CONTENTS. - *I: Dall'uso improprio nel linguaggio comune alle definizioni scientifiche del termine « cibernetica ». - II: Il concetto di « sistema cibernetico ». - III: Il comportamento del sistema deterministico. - IV: Il concetto di « feedback ». - V: Introduzione al concetto di modello. - VI: Modelli cibernetici. - VII: La trasmissione di informazione. - VIII: Cibernetica e apprendimento. - IX: Componenti cibernetiche dell'insegnamento programmato. - X: Nuove prospettive cibernetiche dell'insegnamento programmato. - XI: Cibernetica e didattica (1). - XII: Cibernetica e didattica (2). - XIII: Cibernetica e pedagogia.*

BURNS, R. G., **Mineralogical Applications of Crystal Field Theory** (*Cambridge Earth Sciences Series*), Cambridge University Press, London, 1970, XIV+224 pp., figs. and tables, £4.

Crystal field theory is a model of chemical bonding, well known in chemistry, which has particular application to elements of the first transition series. These elements constitute a large proportion of the Earth's crust and occur in most minerals. This is the first book to apply the theory to mineralogy and geochemistry. Introductory chapters give a non-mathematical outline of the crystal field model and theory of transition metal spectra. Dr Burns then reviews the measurement of absorption spectra of minerals and methods for obtaining cation distributions in silicate crystal structures. These include recently discovered techniques involving electronic, infrared and Mössbauer spectroscopy. The absorption spectral and site population data are then used to explain colour, pleochroism, crystal chemistry and thermodynamic properties of minerals containing transition elements, distributions of the elements in the Earth's crust and properties of the mantle. Molecular orbital theory is introduced in the last chapter to explain aspects of the sulphide mineralogy of transition elements. The book is intended for senior undergraduate and graduate students pursuing courses in geochemistry and mineralogy, but will also be of interest to all earth scientists, chemists and spectroscopists engaged in research on the transition elements. An extensive bibliography is included.

BURRIDGE, K., **New Heaven, New Earth** (A Study of Millenarian Activities), (*Pavilion Series*), Basil Blackwell, Oxford, 1969, VIII+192 pp., £0.80.

To build a landing-ground for planes that will never come and to surround it with landing lights that are never lit because they are made of bamboo and are all pretence seems to us an act of madness, an extravagant comedy played out by the natives of Melanesia in their 'cargo cults'. There have been similar madnesses in our own civilization when men have awaited the coming of the millennium. What is the explanation for such behaviour? The author ranges widely through the fields of example and explanation, and shows that behind the bizarre actions can be discerned men in the crucifixion of uncertainty, men seeking perfection, understanding, human integrity, redemption. At one level this book is a critical and survey of theories about cargo cults.

CONTENTS. - *I: Introduction. - II: Opening the Problem. - III: Polynesian Illustrations. - IV: Matters Arising. - V: Some Melanesian Examples. - VI: Historical Interconnections. - VII: Aspects of Integrity. - VIII: Problems of Classification. - IX: General Pattern. - X: Explanation (1). - XI: Explanation (2). - XII: The Millennium. - XIII: Conclusion.*

BURSELL, E., **An Introduction to Insect Physiology**, Academic Press, London, 1970, XIV+276 pp., figs., £3.50.

The book concentrates on those aspects of the physiology of insects which appear to be characteristic of the class and though it deals with the theoretical concepts typical of the field, the stress falls on their foundation in experiment. The many illustrations from the research literature are given detailed references to make possible the study of a specific subject in greater depth.

CONTENTS. - *I: Somatic Physiology.* - *II: Neuromuscular Physiology.* - *III: The Physiology of Reproduction and Development.* - *IV: Aspects of Physiological Ecology.*

CARNAP, R., **Analiticità, significanza, induzione,** Società Editrice Il Mulino, Bologna, 1971, 556 pp., L. 8.000.

In questo volume sono raccolti i più importanti contributi di Carnap alla filosofia della scienza, stesi lungo un'arco di tempo che va dal 1936 al 1969. In ciascuna delle tre sezioni in cui esso è organizzato (analiticità, significanza, induzione) emergono quindi non soltanto le ultime posizioni di Carnap, ma anche lo sviluppo storico attraverso cui l'autore giunge alle formulazioni ultime, affinando i metodi di analisi e generalizzando i risultati. È così possibile cogliere come il concetto di analicità, definito in « Postulati di significato » (1952) soltanto per alcuni casi speciali, venga ridefinito in « Fondamenti filosofici della fisica » (1966) per l'intero linguaggio della scienza. Analogamente, per quel che riguarda la significanza empirica, si può cogliere nei suoi termini essenziali il decisivo mutamento internevuto tra « Controllabilità e significato » (1936-37) e « Il carattere metodologico dei concetti teorici » (1956), e, per quel che riguarda l'induzione, l'evoluzione che si ha fra « Il continuo dei metodi induttivi » (1952), « Un sistema d'assiomi per la logica induttiva » (1963) e « Logica induttiva e decisione razionale », un inedito di Carnap qui pubblicato per la prima volta. Nell'insieme il volume offre una vera e propria « summa » dell'opera di Carnap, presentandone sistematicamente i testi fondamentali.

CHIRGWIN, B. H. and PLUMPTON, Ch., **A Course of Mathematics for Engineers and Scientists,** VOL. I: **Mathematical Methods,** 2nd ed., Pergamon Press, Oxford, 1970, XII+548 pp., figs., £2.50.

CONTENTS. - *I: Introductory Concepts.* - *II: The Technique of Differentiation.* - *III: The Technique of Integration.* - *IV: Geometry of Two Dimensions.* - *V: Applications of Differentiation.* - *VI: Applications of Integration.* - *VII: Complex Numbers.* - *VIII: Statistics and Probability.* - Appendix: *The Digital Computer.*

CIRANNA, C., **Federico Froebel** (Series: « *I Cirannini* »), Editrice Ciranna, Roma, 1970, 152 pp., L. 1.000.

CONTENTS. - *I: La vita e le opere.* - *II: Il pensiero, la pedagogia, il metodo. Riassunti delle opere fondamentali.* - *III: Consensi e dissensi. Federico Froebel alla luce della critica.* - *IV: Questionari e colloqui, parallelismi differenziali.* - *V: Richiami di filosofia e pedagogia per l'inquadramento dell'opera froebelliana.* - Bibliografia.

COMTET, L., **Analyse Combinatoire,** 2 VOLS. (Series: « *Le Mathématicien* », No. 4/5), Presses Universitaires de France, Paris, 1970, 192 and 190 pp., figs., 20 F each volume.

CONTENTS. - **Vol. I.** - *I: Vocabulaire de l'analyse combinatoire.* - *II: Partitions d'entiers.* - *III: Identités et développements.* - **Vol. II.** - *IV: Formules de crible.* - *V: Nombres de Stirling.* - *VI: Permutations.* - *VII: Exemples d'inégalités et d'estimations.*

COURY, Ch., BONNIOT, R. and COLETSOS, P.-J., **La tuberculose pulmonaire et son traitement** (Series: *Pour Le Praticien*), Librairie Maloine S.A., Paris, 1970, 330 pp., figs., 56 F.

Les auteurs dénient à leur ouvrage la qualité d'un Traité ou d'un Précis: ils le définissent comme un simple « Aide-Mémoire » destiné à l'étudiant en médecine et au praticien. En fait, ce livre illustré de nombreux documents radiologiques démonstratifs, fournit au lecteur une mise à jour étoffée dans un domaine de la pathologie qui a subi depuis 25 ans de très profonds changements. Dégagé de toute considération théorique, il est toutefois précédé d'une introduction bactériologique et pathogénique assez copieuse

dont la rédaction a été confiée à un biologiste. Le corps de l'ouvrage s'adresse exclusivement aux cliniciens. La présentation se veut schématique, quitte à faire appel à certaines notions élémentaires, trop souvent oubliées. La première partie est consacrée aux manifestations de la maladie et au diagnostic de ses formes anatomo-clinique et radiologiques les plus fréquentes de nos jours. La seconde partie développe de façon analytique les moyens thérapeutiques nouveaux dont on dispose. Les règles générales du traitement que bien des médecins risquent d'enfreindre au détriment de leurs patients, les modalités particulières de leur application dans chacune des formes de la maladie sont ensuite énoncées en termes sobres et précis, valorisés par l'expérience des auteurs. Le livre se termine sur un exposé succinct des droits et obligations des malades aux termes de la législation sociale, et sur les mesures de prévention et de lutte antituberculeuse susceptibles d'améliorer la situation de la France face à un fléau collectif toujours menaçant; elles conduiront un jour peut-être à l'éradication presque totale de la maladie, comme cela est déjà le cas dans de nombreux pays étrangers.

CRICK, F., **Uomini e molecole** (È morto il vitalismo?), (Series: *Saggi Zanichelli*, No. 7), Zanichelli Editore, Bologna, 1970, 142 pp., L. 1.800.

Crick con il suo saggio che si affianca a quelli di Jacques Monod, di François Jacob e dello stesso Watson, vuole sottolineare e convincere il lettore che i progressi scientifici avvengono lottando non solo contro difficoltà tecniche ma anche contro le ideologie dominanti, contro vecchi schemi culturali che si oppongono al progresso e alle innovazioni. Il libro di Crick è un pamphlet contro il vitalismo e tutto ciò che resta di esso dopo le moderne conquiste della biologia. Se il vitalismo non è morto nei paesi anglosassoni dove, ricorda l'autore, permane ancora una diffusa avversione addirittura contro la teoria darwiniana posta al bando in alcuni stati del Nord America, in Italia la situazione non è migliore. Venti anni di fascismo non hanno certo facilitato il diffondersi delle moderne discipline biologiche che ancor oggi non sempre trovano posto nell'insegnamento, specie quello impartito nella scuola secondaria.

CROW, J. F. and KIMURA, M., **An Introduction to Population Genetics Theory**, Harper & Row, Publishers, New York, 1970, XVI+592 pp., figs. and tables, No price stated.

CONTENTS. - *I: Models of Population Growth.* - *II: Randomly Mating Populations.* - *III: Inbreeding.* - *IV: Correlation Between Relatives and Assortative Mating.* - *V: Selection.* - *VI: Populations in Approximate Equilibrium.* - *VII: Properties of a Finite Population.* - *VIII: Stochastic Processes in the Change of Gene Frequencies.* - *IX: Distribution of Gene Frequencies in Populations.* - Appendix: *Some Statistical and Mathematical Methods Frequently Used in Population Genetics.*

DEMAZURE, M. and GABRIEL, P., **Groupes algébriques**, VOL. I: **Géométrie algébrique, généralités, groupes commutatifs**, Masson & C.ie, Paris, 1970, XXVI+700 pp., 230 F.

CONTENTS. - I. Introduction à la géométrie. *I: Le langage.* - *II: Modules quasi-cohérents, applications.* - *III: Schémas algébriques.* - *IV: Morphismes lisses.* - *V: Morphismes propres.* - II. Groupes algébriques. *I: Schémas en groupes.* - *II: Représentations linéaires.* - *III: Cohomologie de Hochschild des schémas en groupes.* - *IV: Calcul différentiel sur les schémas en groupes.* - *V: Groupes localement algébriques sur un corps.* - *VI: Propriétés spéciales à la caractéristique 0.* - *VII: Propriétés spéciales à la caractéristique $p \neq 0$.* - III. Applications de la théorie des faisceaux aux groupes algébriques. *I: Théorie des faisceaux.* - *II: Quotient d'un schéma par une relation d'équivalence finie localement libre.* - *III: Espaces homogènes des faisceaux en groupes.* - *IV: Torseurs.* - *V: Interprétation et calcul de quelques groupes de torseurs.* - *VI: Extensions de faisceaux en groupes.* - IV. Groupes affines commutatifs, nilpotents, résolubles. *I: Groupes multiplicatifs.* - *II: Groupes unipotent et trigonalisables.* - *III: Groupes commutatifs.* - *IV: Groupes nilpotents et résolubles.* - V. Structure des groupes affines commutatifs. *I: Groupes unipotents commutatifs sur un corps parfait.* - *II: Catégories proartiniennes.* - *III: Isogénies de groupes affines commutatifs.* - *IV: Modules de Greenberg.* - *V: Anneau universel et vecteurs de Witt.*

DEUVE, J., **Serpents du Laos** (Mémoire O.R.S.T.O.M., No. 39), Office de la Recherche Scientifique et Technique Outre-Mer, Paris, 1970, 252 pp., figs., 72 F.

Contents. - *I: Généralités. - II: Identification. - III: Les espèces venimeuses du Laos. - IV: Typhlopidés (Typhlopidae). - V: Boidés (Boidae). - VI: Anilidés (Anilidae). - VII: Xénopeltidés (Xenopeltidae). - VIII: Colubridés-Elapidés. Table de détermination des genres. - IX: Colubridés (Colubridae) - s/fam Natricines (Natricinae). - X: Colubridés - s/fam Coronellinés (Coronellinae). - XI: Colubridés - s/fam Homalopsinés (Homalopsinae). - XII: Colubridés - s/fam Boiginés (Boiginae). - XIII: Elapidés (Elapidae). - XIV: Amblycéphalidés (Amblycephalidae). - XV: Vipéridés (Viperidae).*

DOBZHANSKY, T., **Genetics of the Evolutionary Process**, Columbia University Press, London, 1970, XIV+506 pp., figs. and tables, £4.95.

In this volume, the product of decades of study and research, the world's foremost geneticist surveys the major developments in what is emerging as the most important single area of scientific inquiry in the twentieth century: biological science in general, and the biological theory of evolution in particular. It is to these new questions that the author brings a lifetime of experience: the results of the discoveries of the molecular geneticists; lingering misconceptions about the biological theory of evolution and its genetic aspects; the profound truths of the linear conception of time which evolutionary theory confirms; the crucial importance of DNA and RNA in explaining the unity and diversity of life; aspects of the prodigious diversity of the organisms found on earth; an exposition of the two great strategies of life expansion which have been traced in our history; and an inquiry into the peculiar myths that have arisen around the question of the races of mankind.

Contents. - *I: The Unity and Diversity of Life. - II: Genetic Continuity and Change. - III: Mutation and Genetic Variability. - IV: Normalizing Natural Selection. - V: Balancing Selection and Chromosomal Polymorphisms. - VI: Balancing Selection and Genetic Load. - VII: Directional Selection. - VIII: Random Drift and Founder Principle. - IX: Populations, Races, and Subspecies. - X: Reproductive Isolation. - XI: Patterns of Species Formation. - XII: Patterns of Evolution.*

DOLIGEZ, M. and DOLIGEZ, G., **Electromagnétisme** (Contribution à la théorie corpusculaire des champs), Librairie Scientifique et Technique Albert Blanchard, Paris, 1970, 298 pp., figs., 86 F.

Contents. - I. Théorie des chocs. Structure de la matière. *I: Théorie des chocs. - II: Structure de la matière.* - II. Electricité magnétisme. *III: L'électron. - IV: Induction.* - III. Lumière ou plus généralement vibrations électromagnétiques. *V: Le photon. - VI: Réfraction (Cheminement à travers la matière). - VII: Influence d'une surface de séparation de deux milieux d'indices différents sur la trajectoire d'un photon. Réfraction. Réflexion. Polarisation. - VIII: Interférence. Diffraction. - IX: Invariance de la vitesse de la lumière.* - IV. Annexes.

DOLLFUS, A. (ed.), **Surfaces and Interiors of Planets and Satellites**, Academic Press, London, 1970, XII+570 pp., figs. and tables, £9.00.

This book is an analysis of present knowledge in planetary physics—a coordinated synthesis of the many scattered scientific papers by experienced experts in the field. It concentrates solely on the solid bodies in the solar system, thus avoiding areas where recent developments have defied synthesis. For the increasing number of scientists new to the field, this book gives a broad introduction which will orient them to research topics.

Contents. - *I: Détermination des masses des planètes et satellites*, by J. Kovalevsky. - *II: Diamètres des planètes et satellites*, by A. Dollfus. - *III: Radar Studies of the Planets*, by R. B. Dyce - *IV: Thermal Radio Emission of the Planets and Moon*, by C. H. Mayer. - *V: Photométrie des Surfaces Planétaires*, by G. de Vaucouleurs. - *VI: Photometry of Asteroids*, by T. Gehrels. - *VII: Physical Properties of Saturn's Rings*, by M. S. Bobrov. - *VIII: Internal Constitution of Terrestrial Planets*, by B. J. Levin. - *IX: Planetary Magnetic Fields*, by R. Hide. - *X: The Surface Environment and Possible Biology of Mars*, by C. Sagan.

DOUSSET, H. and PLARD, C., **Vademecum encyclopédique du médecin praticien,** 7th rev. ed., (Series: *Le médecin et son malade*), Librairie Maloine S.A., Paris, 1970, 1392 pp., figs., 88 F.

Pour chaque affection, l'omnipraticien trouvera dans ce manuel un bref rappel clinique de tous les symptômes, des examens complémentaires à faire pratiquer ainsi que l'énoncé détaillé du traitement, des modifications de celui-ci selon l'évolution de la maladie, et l'indication des erreurs à ne pas commettre. Lui sont présentés sous forme condensée, mais complète: — Ce quil faut savoir. — Ce qu'il faut faire. — Ce qu'il faut éviter.

ECCLES, J. C., **Facing Reality** (Philosophical Adventures by a Brain Scientist), (Series: *Heidelberg Science Library*, Vol. 13), Springer-Verlag, Berlin, 1970, xii+210 pp., figs., DM 22.60.

Contents. - *I: Introduction: Man, Brain and Science.* - *II: The Neuronal Machinery of the Brain.* - *III: Synaptic Mechanisms Possibly Concerned in Learning and Memory.* - *IV: The Experiencing Self.* - *V: The Brain and the Unity of Conscious Experience.* - *VI: Evolution and the Conscious Self.* - *VII: The Understanding of Nature.* - *VIII: Man, Freedom and Creativity.* - *IX: The Necessity of Freedom for the Free Flowering of Science.* - *X: The Brain and the Soul.* - *XI: Education and the World of Objective Knowledge.* - *XII: Epilogue.*

EDELEN, D. G. B. and WILSON, A. G., **Relativity and the Question of Discretization in Astronomy** (Series: *Springer Tracts in Natural Philosophy*, No. 20), Springer-Verlag, Berlin, 1970, xii+186 pp., figs. and tables, DM 38.

Contents. - I. Theoretical Considerations. *I: The Environment for Cosmic Structures.* - *II: An Equilibrium Theory of Galactic Isopleths.* - *III: Galactic Morphology and Scale.* - II. Observational Considerations. *IV: Observational Problems of Macroscale Discretization.* - *V: Observational Evidence for Discretization Phenomena.* - Appendix A: *Curvature Conventions and Signs.* - Appendix B: *The Spherically Symmetric Case.* - Appendix C: *On Conformally Related Metric Spaces, One of which Admits and Irrotational Isometry.* - Appendix D: *Analysis of the Complete Nonlinear Discretization Equation.*

EHRLICH, P. R. and EHRLICH, A. H., **Population, Resources, Environment** (Issues in Human Ecology), W. H. Freeman and Company, Berkshire, 1970, 384 pp., figs. and tables, £4.20.

This book is a comprehensive analysis of the worldwide crisis of overpopulation and the resulting demands on food, resources, and the environment. Taking a broad ecological approach, the Ehrlichs demonstrate that problems of modern society such as environmental deterioration, hunger, resource depletion, and war are closely interconnected and that together they constitute a challenge without precedent in human history. The authors draw upon a wide diversity of sources in documenting the reasons for growing alarm over mankind's prospects, and they offer many constructive proposals that might help brighten those prospects.

EWER, R. F., **Ethology of Mammals,** Plenum Press, New York, 1968, xiv+418 pp., figs. and plates, $26.00.

Ethology of Mammals is the first book to deal comparatively with such topics as food and feeding, social organization and territory, aggressive and friendly behaviour, courtship and mating, and parental and juvenile behaviour. Dr. Ewer begins with a brief outline of the fundamental ideas of ethology, and her summary of current knowledge of the more important aspects of behaviour in mammalian orders takes into account German literature on the subjects. Her reassessment of these ideas brings them into relation with more analytical laboratory studies, with a final summary of what constitutes the particular features distinguishing mammalian behaviour from that of the vertebrate classes.

CONTENTS. - *I: Some Basic Concepts.* - *II: Expression and Communication.* - *III: Food.* - *IV: Social Organisation and Territory.* - *V: Scent Marking.* - *VI: Fighting.* - *VII: Threat and Appeasement.* - *VIII: Amicable Behaviour.* - *IX: Courtship and Mating.* - *X: Parent and Child.* - *XI: Play.* - *XII: Ethological Theories and Mammalian Behaviour.* - *XIII: The Behaviour of Mammals.*

FÉLIX, L., **Notions de mesures et nombres réels** (Enseignement moderne des mathématiques au niveau du second degré), Librairie Scientifique Albert Blanchard, Paris, 1970, 180 pp., figs. and plates, 18 F.

CONTENTS. - I. Mesures et introduction des nombres réels. *I: Opérations physiques de mesuration.* - *II: Mesures d'ensembles.* - *III: Détermination des aires et volumes.* - *IV: Notions générales de grandeur. Mesures et intégrales.* - II. Etude de l'ensemble R des réels. *I: Opérations dans l'ensemble des réels absolus.* - *II: L'ensemble* R *des réels relatifs.*

FERRIO, C., **Trattato di psichiatria clinica e forense,** 2 VOLS., 2nd ed., Unione Tipografico-Editrice Torinese, Torino, 1970, XXXVI+2584 pp., figs. and tables, L. 35.000.

CONTENTS. - **Volume I.** - I. Patologia mentale. *I: Introduzione. I fondamenti biologici della psicologia e l'impostazione dello studio della patologia mentale.* - *II: La sensibilità.* - *III: La percettibilità.* - *IV: Il pensiero.* - *V: La memoria.* - *VI: L'affettività.* - *VII: L'istinto.* - *VIII: La volontà e la motilità.* - *IX: L'attenzione.* - *X: L'espressione.* - *XI: L'orientamento.* - *XII: La personalità e il carattere.* - *XIII: L'intelligenza.* - *XIV: La coscienza.* - II. Diagnostica generale. *XV: Introduzione alla diagnostica psichiatrica.* - *XVI: L'eziologia.* - *XVII: I fenomeni mentali in rapporto al tempo.* - *XVIII: L'esame somatico.* - *XIX: Sintomatologia somatica nella diagnostica psichiatrica.* - *XX: Alcuni « tests » utili nella diagnostica psichiatrica.* - III. Diagnostica psichiatrica speciale. *XXI: Le personalità psicopatiche.* - *XXII: Le reazioni soggettive abnormi.* - *XXIII: La frenastenia.* - *XXIV: La schizofrenia.* - *XXV: Le psicosi fasiche. La psicosi maniaco-depressiva. Le distimie.* - *XXVI: Le reazioni esogene.* - *XXVII: Le psicosi su base endocrina.* - *XXVIII: Le psicosi da infezioni.* - *XXIX: L'epilessia e le forme affini.* - *XXX: Le psicosi della senilità e della presenilità.* - *XXXI: La psicosi da tumori endocranici.* - *XXXII: I traumi del capo.* - **Volume II.** - IV. Gli istinti e la loro patologia. *XXXIII: La funzione sessuale nell'uomo e nella donna.* - *XXXIV: Patologia della funzione sessuale comune ai due sessi.* - *XXXV: Le tossicomanie.* - *XXXVI: Il suicidio.* - V. Commento forense alla medicina mentale. *XXXVII: Commento forense alla psicologia ed alla psicopatologia.* - *XXXVIII: Commento forense alla clinica psichiatrica.* - *XXXIX: Diagnostica psichiatrica a fini forensi.* - *XL: Traumatologia psichiatrico-forense.* - *XLI: Sessuologia psichiatrico-forense.* - *XLII: Terminologia psichiatrico-forense in rapporto alle leggi italiane.* - VI. Commento psichiatrico alle leggi italiane. *XLIII: Commento psichiatrico al libro I del Codice penale: « dei reati in generale ».* - *XLIV: Commento psichiatrico al libro II del Codice penale: « dei delitti in particolare ».* - *XLV: Commento psichiatrico al libro III del Codice penale: « delle contravvenzioni in particolare ».* - *XLVI: Commento psichiatrico ai codici di procedura.* - *XLVII: L'infermità di mente in rapporto alla capacità di agire, alla capacità di testare ed alla capacità di donare.* - *XLVIII: L'infermità di mente in rapporto al matrimonio.* - *XLIX: La legislazione sui minorenni.* - *L: La pericolosità degli infermi di mente.*

FLORKIN, M. and STOTZ, E. H. (eds.), **Comprehensive Biochemistry,** VOL. 26/C: **Extracellular and Supporting Structures,** Elsevier Publishing Co., Amsterdam, 1971, XII+304 pp., figs. and tables, Dfl. 67.50.

CONTENTS. - *I: Chitinous Structures,* by CH. JEUNIAUX. - *II: Resilin,* by S. O.

ANDERSEN. - *III: Elastin*, by C. FRANZBLAU. - *IV: The Tubes of Polychaete Annelids*, by R. DEFRETIN. - *V: Constituents of Brachiopod Shells*, by M. JOPE. - *VI: Dental Enamel*, by J. E. EASTOE.

FROISSART, M. (ed.), **Hyperbolic Equations and Waves** (Battelle Seattle 1968 Rencontres), Springer-Verlag, Berlin, 1970, VIII+394 pp., figs., DM 48.

The topic chosen for the 1968 Battelle Rencontres - hyperbolic equations and waves - has proved a cornerstone of physics for more than a century and extends over most fields of contemporary physics. It follows immediately that the wide range of physicists concerned could not be represented by more than a couple of specialists in any single field. Thus, aside from bridging the gap between mathematicians and physicists, the 1968 Rencontres provided a rather unique occasion to plug many intra-disciplinary gaps among physicists.

GENERMONT, J., **Recueil d'exercices de génétique des populations** (Avec rappels des définitions et notions fondamentales), Masson & C.ie, Paris, 1970, VIII+144 pp., figs., 44 F.

Le présent manuel est un recueil de 40 exercices portant sur les principaux domaines de la Génétique des Populations: description des structures génétiques, étude des systèmes de croisements, effets des mutations et de la sélection, importance de l'effectif. Les thèmes des exercices sont inspirés d'exemples relatifs soit à l'espèce humaine, soit au règne animal, soit au règne végétal. Certains sont conçus comme des introductions à des problèmes pratiques de Génétique appliquée. Ce recueil s'adresse donc à tous les étudiants s'intéressant à la Génétique des Populations, quelle que soit leur orientation ultérieure (aspects théoriques, génétique médicale, génétique appliquée animale ou végétale).

GILBERT, J. (ed.), **The Complete Aquarist's Guide to Freshwater Tropical Fishes**, Ward Lock Limited, London, 1970, 250 pp., figs., £4.50.

This book supplies expert and practical advice on all aspects of fish-keeping—setting up and embellishing an aquarium; problems of lighting, temperature and aeration; planting, stocking, feeding and controlling disease; methods of sexing and breeding. There are concise accounts of the appearance and behaviour patterns of almost 300 species, the majority of which are illustrated in detail and colour. Here are the most spectacular and diverting fishes from the ponds, streams and rivers of Asia, Africa and America—in all colours, shapes and sizes. The entire range is covered—Barbs and Rasboras, Lyretails and Pearl Fishes, Gouramies and Siamese Fighters, Mollies and Guppies, Platies and Swordtails, Angels and Acaras, Tetras, Catfishes and dozens more. Here too, for the first time in book form, is a detailed survey, by one of the world's leading authorities, of the keeping and breeding of *Symphysodon discus*, the beautiful Discus or Pompadour Fish.

GUÉRIN, P., **Planètes et satellites** (Mondes de l'espace), Librairie Larousse, Paris, 1970, 308 pp., figs., 94 F.

Cet ouvrage se propose d'exposer l'état actuel de nos connaissances sur la constitution physique des mondes planétaires qui gravitent autour du Soleil, et de dire quelques mots également des problèmes soulevés par l'existence d'autres systèmes de planètes autour des étoiles de la Galaxie, ainsi que des possibilités de vie à la surface de ces planètes. Ces différents sujets sont abordés dans un esprit résolument moderne. On passe très rapidement sur la description du mouvement des planètes et de leurs satellites, laissant de côté tout rappel historique des conceptions anciennes sur le système du monde: de nombreux auteurs ont traité la question, et le lecteur pourra, s'il le désire, se reporter à leurs écrits.

GULIAN, C. I., **Hegel** (Sau Filozofia Crizei), Editura Academiei Republicii Socialiste România, Bucuresti, 1970, 418 pp., Lei 20,50.

CONTENTS. - *I: Introducere. - II: Geneza Filozofiei Hegeliene. - III: Fenomenologia Spiritului. - IV: Evolutia Ideilor Sociale în Filozofia Hegeliana. - V: Filozofia Istoriei. - VI: Filozofia Religiei. - VII: Estetica. - VIII: Conceptia Despre Natura. - IX: Ontologie Si Metodologie. - X: Istoria Filozofiei. - XI: Hegel Si Epoca Noastra.*

HALL, A. S., **An Introduction to the Mechanics of Solids** (Stresses and Deformation in Bars), John Wiley & Sons Ltd., London, 1969, XIV+434 pp., figs., £5.40.

This book is intended for use by undergraduate students as a first course in structural mechanics. An effort has been made to use symbols, sign conventions and concepts that will require a minimum of adjustment in later work. The concepts of flexibility and stiffness simplify the present work as well as leading on to later phases. The book is written for engineering students, and, in consequence, the explanations are given in physical terms as far as possible. The problems and their solutions are expressed in terms of engineering concepts and not in those of applied mathematics.

CONTENTS. - *I: Definitions. - II: Analysis. - III: Axial Force and Deformation. - IV: Bending. - V: Shear. - VI: Torsion. - VII: Stresses and Strains at a Point. - VIII: Combined Stresses. - IX: Bar Assemblies. - X: Instability. - XI: Dynamic Loading.* - Appendices.

Handbook of Sensory Physiology, VOL. I: **Principles of Receptor Physiology,** edited by W. R. LOEWENSTEIN, Springer-Verlag, Berlin, 1971, XII+600 pp., figs., DM 168.

Written by world authorities in their fields, the volume deals with questions such as transduction of energy by receptors, reaction of membrane molecules to stimuli, the generation of nerve impulses, encoding of information, processing of information by the nervous system, and the making of neuronal circuits. The questions are illuminated from various points of view and analyzed with various techniques. The contributors are physiologists, physicists, chemists, geneticists and psychologists.

CONTENTS. - *I: Mechano-Chemical Conversion*, by A. KATCHALSKY. - *II: Proteins in Bioelectricity. Acetylcholine-Esterase and -Receptor*, by D. NACHMANSOHN. - *III: Transmission Action on Synaptic Neuronal Receptor Membranes*, by L. TAUC. - *IV: The General Electrophysiology of Input Membrane in Electrogenic Excitable Cells*, by H. GRUNDFEST. - *V: Formation of Neuronal Connections in Sensory Systems*, by M. JACOBSON. - *VI: The Relation of Physiological and Psychological Aspects of Sensory Intensity*, by L. E. LIPETZ. - *VII: Sensory Power Functions and Neural Events*, by S. S. STEVENS. - *VIII: Generation of Responses in Receptor*, by M. G. F. FUORTES. - *IX: Mechano-electric Transduction in the Pacinian Corpuscle. Initiation of Sensory Impulses in Mechanoreceptors*, by W. R. LOEWENSTEIN. - *X: A Biophysical Analysis of Mechano-electrical Transduction*, by T. TEORELL. - *XI: Responses of Nerve Fibers to Mechanical Forces*, by D. E. GOLDMAN. - *XII: The Early Receptor Potential*, by R. A. CONE and W. L. PAK. - *XIII: The Nature of the Photoreceptor in Phototaxis*, by M. E. FEINLEIB and G. M. CURRY. - *XIV: Sensory Transduction in Hair Cells*, by A. FLOCK. - *XV: Transducer Properties and Integrative Mechanisms in the Frog's Muscle Spindle*, by D. OTTOSON and G. M. SHEPHERD. - *XVI: Static and Dynamic Behavior of the Stretch Receptor Organ of Crustacea*, by C. A. TERZUOLO and C. K. KNOX. - *XVII: Patterns of Organization of Peripheral Sensory Receptors*, by B. L. MUNGER.

HARANT, H. and DELAGE, A., **Parasitologie médicale et pathologie exotique,** 6th rev. ed., (Series: *Les Précis pratiques*), Librairie Maloine S.A., Paris, 1971, 474 pp., figs., 48 F.

L'ouvrage du Professeur Hervé Harant (sixième édition écrite avec la collaboration du Docteur Alix Delage) est un Précis désormais classique dont la première édition date de 1939. Ce manuel paraît aujourd'hui considérablement augmenté (150 pages de plus que dans la cinquième édition) et remanié. Toutefois le plan initial est conservé: généralités sur la Parasitologie et les grands syndromes parasitaires; notions d'Entomologie médicale; maladies à Protozoaires; Helminthologie médicale; Bactérioses, Spirochétoses et Viroses transmises par les Arthropodes; notions de Mycologie médicale; aperçu sur les maladies exotiques non parasitaires, les envenimations, les toxicoses, les carences; aspects des grands syndromes cosmopolites dans les pays chauds. Au surplus cette nouvelle édition comporte un index alphabétique et un synopsis géographique.

Contents. - *I: Etude générale d'une maladie transmissible.* - *II: Maladies parasitaires cosmopolites ou localisées.* - *III: Maladies tropicales non parasitaires.*

HARRIS, H., **The Principles of Human Biochemical Genetics,** North-Holland Publishing Company, Amsterdam, 1970, x+328 pp., figs. and tables, Dfl. 22.50.

Recent advances in our understanding of the nature of inherited biochemical diversity among human beings have had an important impact on the development of ideas not only in human genetics, but also in medicine, biochemistry, anthropology and in biology in general. Much of this work is to be found only in original papers scattered in a wide range of different scientific and medical journals, or in specialised review articles. The purpose of this book is to present a general account of the subject, and in particular explain the principal concepts which underlie modern research in this field.

Contents. - *I: Gene Mutations and Single Aminoacid Substitutions.* - *II: One Gene-One Polypeptide Chain.* - *III: Duplications and Deletions, and their Effects on Protein Structure.* - *IV: Gene Mutations Affecting Rates of Protein Synthesis.* - *V: Quantitative and Qualitative Variation of Enzymes.* - *VI: The Inborn Errors of Metabolism.* - *VII: The Blood Group Substances.* - *VIII: Enzyme and Protein Diversity in Human Populations.* - *IX: Gene Mutations and Inherited Disease.* - Appendices.

HILTON, P., **General Cohomology Theory and K-Theory** (*London Mathematical Society Lecture Note Series,* No. 1), Cambridge University Press, London, 1971, 102 pp., £1.40.

These notes constitute a record of a short course of lectures given in Sao Paulo, Brazil, in the summer of 1968. The audience was assumed to be familiar with the basic material of homology and homotopy theory, and the object of the course was to explain the methodology of general cohomology theory and to give applications of K-theory to familiar problems such as that of the existence of real division algebras. The audience was not assumed to be sophisticated in homological algebra, so one chapter is devoted to an elementary exposition of exact couples and spectral sequences.

Contents. - *I: General Cohomology Theories.* - *II: Exact Couples and Spectral Sequences.* - *III: The Generalized Atiyah-Hirzebruch Spectral Sequence.* - *IV: K-Theory.* - *V: K-Theory, the Chern Character and the Hopf Invariant Problem.* - Appendix: *On the Construction of Cohomology Theories.*

HÖHLER, G. (ed.), **Springer Tracts in Modern Physics** (Ergebnisse der exakten Naturwissenschaften), Vol. 55, Springer-Verlag, Berlin, 1970, vi+290 pp., figs. and tables, DM 78.

Contents. - *I: Low Energy Pion-Pion Scattering,* by D. Morgan and J. Pisut. - *II: Analytic Extrapolations and the Determination of Pion-Pion Phase Shifts,* by J. Pisut. - *III: Coulomb Corrections in the Analysis of* πN *Experimental Scattering Data,* by G. C. Oades. - *IV: Kaon-Nucleon Interactions below 1 GeV/c,* by B. R. Martin. - *V: The* ΛKN *Coupling and Extrapolation below the* KN *Threshold,* by A. D. Martin. - *VI: Nucleon-Nucleon Interactions below 1 GeV/c,* by G. Kramer. - *VII: Coupling Constants from PCAC,* by H. Pilkuhn. - *VIII: Regge Residues,* by C. Michael. - *IX: Vector Mesons in Electromagnetic Interactions,* by M. Gourdin. - *X: Coupling Parameters of Pseudoscalar Meson Photoproduction on Nucleons,* by W. Pfeil and D. Schwela. - *XI: Compilation of Coupling Constants and Low Energy Parameters,* by G. Ebel, D. Julius, G. Kramer, B. R. Martin, A. Müllensiefen, G. Oades, H. Pilkuhn, J. Pisut, M. Roos, G. Schierholz, W. Schmidt, F. Steiner and J. J. de Swart.

HÖHLER, G. (ed.), **Springer Tracts in Modern Physics** (Ergebnisse der exakten Naturwissenschaften), Vol. 57, Springer-Verlag, Berlin, 1971, vi+270 pp., figs., DM 78.

Contents. - *I: Some Consequences of Unitarity and Crossing. Existence and Asymptotic Theorems*, by D. Atkinson. - *II: Analyticity, Unitarity and Crossing-Symmetry. Constraints for Pion-Pion Partial Wave Amplitudes*, by G. Wanders. - *III: New Methods in the Analysis of π-N Scattering*, by J. Hamilton. - *IV: Regge-Pole Phenomenology*, by E. J. Squires. - *V: Certain Problems of Two-Body Reactions with Spin*, by A. P. Contogouris. - *VI: Duality and Regge Theory*, by R. Oehme. - *VII: Complex Angular Momentum*, by R. Oehme. - *VIII: An Introduction to Dual Resonance Models in Multiparticle Physics*, by H. Satz. - *IX: Physical N-Pion Functions*, by H. R. Rubinstein. - *X: Application of Harmonic Analysis to Inelastic Electron-Proton Scattering*, by W. Rühl. - *XI: Small-Distance Behaviour in Field Theory*, by K. Symanzik. - *XII: Physics on the Light Cone*, by R. A. Brandt. - *XIII: Course on Padé Approximants*, by J. Zinn-Justin.

HOLDEN, A., **La nature des solides** (Du caillou au transistor), Dunod, Paris, 1970, viii+232 pp., figs., 28 F.

Contents. - *I: Théories. - II: La chaleur. - III: Capacité calorifique. - IV: Ordre et désordre. - V: Symétrie. - VI: Atomes et ions. - VII: Molécules et métaux. - VIII: Structures. - IX: Les mouvements atomiques. - X: Particules et ondes. - XI: Les électrons dans les atomes. - XII: Les électrons dans les solides. - XIII: Conduction électrique. - XIV: Les semi-conducteurs. - XV: Les aimants.*

HÖRMANN, H., **Psycholinguistics** (An Introduction to Research and Theory), Springer-Verlag, Berlin, 1971, xii+378 pp., figs. and tables, DM 68.

The object of this book is to present, and place into a context, the methods and findings of the new era of psycholinguistics and, thus, to outline one of several possible approaches to the study of the psychological laws of verbal processes. In a work on psycholinguistics one necessarily operates not only with concepts of psychology but also with those derived from linguistics. The latter are introduced and defined here only to the extent that was regarded necessary for a predominantly psychological discussion. It is worth mentioning that this book originated in a series of lectures given in the academic year 1964-1965.

Contents. - *I: Introduction: Organism, Language and World. - II: Sign, Expression and Symbol. - III: Linguistic Units. - IV: Language and Communication. - V: The Probability Structure of Language. - VI: Phenomenology of Verbal Associations. - VII: The 'Mechanism' of Association. - VIII: The Philosophical Background to Modern Psycholinguistics. - IX: The Field Concept of Meaning. - X: Mediation Theories of Language Processes. - XI: The Conditioning Theory of Meaning: Its Achievement, Weakness and Further Development. - XII: The Imitation of Sounds and Sound Symbolism. - XIII: The Psychological Reality of Grammar. - XIV: Genetic and Child Psychology as a Testing Ground for Psycholinguistics. - XV: The Influence of Language on Man's View of the World.*

HORN, G. and HINDE, R. A. (eds.), **Short-term Changes in Neural Activity and Behaviour** (A Conference Sponsored by King's College Research Centre Cambridge), Cambridge University Press, London, 1970, viii+628 pp., figs., £10.40.

This volume contains the proceedings of a Conference held to discuss changes in behaviour and in the nervous system which have time courses ranging from about a second to more than a day, and to specify the extent to which present knowledge permits the behavioural changes to be correlated with the neurophysiological changes. Habituation was chosen as one topic for discussion because it is a type of behavioural change in which the units are phenomenologically similar at finer levels of analysis. However, habituation shares characteristics with other forms of learning and it also proved fertile to discuss the neural mechanisms underlying these.

HUETZ DE LEMPS, A., **La végétation de la terre** (Series: *Initiation aux études de Géographie*), Masson & C.ie, Paris, 1970, 144 pp., figs. and plates, 25 F.

CONTENTS. - *I: Les plantes et le milieu géographique. - II: Les paysages végétaux des zones tempérées et froides. - III: Les paysages végétaux de la zone tropicale humide. - IV: La végétation des régions arides et semi-arides.*

JEFFREYS, H., **The Earth** (Its Origin, History and Physical Constitution), 5th ed., Cambridge University Press, London, 1970, XII+526 pp., figs. and plates, £7.00.

CONTENTS. - *I: The Mechanical Properties of Rocks. - II: The Theory of Elastic Waves. - III: Observational Seismology. - IV: The Theory of the Figures of the Earth and Moon. - V: The Figures of the Earth and Moon. Discussion of Observations. - VI: Stress-Differences in the Earth. - VII: Nutations, the Bodily Tide, and Free Vibrations. - VIII: Imperfections of Elasticity: Tidal Friction. - IX: The Age of the Earth. - X: The Thermal History of the Earth. - XI: The Origin of the Earth's Surface Features. - XII: Special Problems.* - Appendices.

KARLSTROM, T. N. V. and BALL, G. E. (eds.), **The Kodiak Island Refugium** (Its Geology, Flora, Fauna and History), The Ryerson Press, Toronto, 1969, XIV+262 pp., figs., tables and plates, $10.00.

CONTENTS. - I. Introductory Material. *Introduction*, by T. N. V. KARLSTROM. - *I: The Biological Importance of Pleistocene Refugia*, by C. H. LINDROTH. - II. Geological Investigation of the Kodiak Island Refugium. *II: Regional Setting and Geology*, by T. N. V. KARLSTROM. - III. Botanical Investigation of the Kodiak Island Refugium. *III: Vascular Plants*, by E. HULTÉN. - *IV: Bryophytes*, by H. PERSSON. - *V: A List of Lichens from the Kodiak Island Refugium*, by H. KROG. - *VI: Studies on Chrysophyceae from Some Ponds and Lakes in Alaska III. New or Unusual Chrysophytes from Pinguicula Lake, Kodiak Island, Alaska*, by D. K. HILLIARD. - IV. Zoological Investigation of the Kodiak Island Refugium. *VII: An Annotated List of Invertebrates of the Kodiak Island Refugium*, by C. H. LINDROTH and G. E. BALL. - *VIII: The Species of the Subgenus* Cryobius *of the Kodiak Archipelago* (Pterostichus, Carabidae, Coleoptera), by G. E. BALL. - *IX: An Analysis of the Carabid Beetle Fauna of the Refugium*, by C. H. LINDROTH. - *X: The Fishes of the Kodiak Island Refugium*, by J. D. MCPHAIL. - *XI: Origin of the Terrestrial Mammalian Fauna of the Kodiak Archipelago*, by R. L. RAUSCH. - V. Conclusions. *XII: Concluding Remarks Concerning the Importance of the Kodiak Island Refugium for the Survival of the Biota*, by C. H. LINDROTH.

KEMP, R., **Cell Division and Heredity** (Series: *The Institute of Biology: Studies in Biology*, No. 21), Edward Arnold (Publishers) Ltd., London, 1970, VI+48 pp., figs. and plates, £0.75.

It is the aim of this book to show that mitosis and more especially meiosis should be considered in relation to reproduction as a whole. Meiosis is not described in the sequence in which it occurs but interpreted by considering first the structure of bivalents at metaphase 1. For it is hoped that meiosis will pass from a simple halving of the chromosome number to a logical sequence of events through which the genetic information of all sexually reproducing species must pass, if they are to survive and have the variability necessary for evolution.

CONTENTS. - *I: Meiosis. - II: Meiosis and Mendel's Laws. - III: Mitosis. - IV: Variations in Meiosis and Mitosis. - V: Breeding Systems. - VI: Linkage.*

KERVRAN, C. L., **Alla scoperta delle trasmutazioni biologiche** (Riassunto delle spiegazioni a fenomeni biologici aberranti), Edagricole, Bologna, 1969, 112 pp., figs., L. 1.200.

CONTENTS. - *I: Introduction à l'édition italienne. - I: Presentazione. - III: Introduzione. - IV: Preliminari. - V: Osservazioni pratiche personali. - VI: Osservazioni anor-*

mali sulle piante. - *VII: Anomalie nei minerali.* - *VIII: Prove di come avvengono le trasmutazioni e loro meccanismo.* - *IX: Principali trasmutazioni studiate.* - *X: Ricerche sulle piante.* - *XI: Applicazioni e sviluppi.* - *XII: Visioni d'avvenire.* - *XIII: Medicina e biologia.* - *XIV: Annessi.* - *XV: Discussioni teoriche.* - *XVI: Aggiunta alla edizione italiana.*

KLEBER, W. †, **An Introduction to Crystallography,** Veb Verlag Technik, Berlin, 1970, 366 pp., figs. and tables, DM 36.

CONTENTS. - *I: Crystal Structure Theory and Crystal Morphology.* - *II: Physical-Chemical Crystallography.* - *III: Crystal Chemistry.* - *IV: Crystal Physics.* - *V: Appendix: The Bertaut Law of Lattice Energy and the Noar Relations.* - *VI: Bibliography.* - *VII: Index.*

Landolt-Börnstein (Zahlenwerte und Funktionen aus Naturwissenschaften und Technik), New Series, **III: Kristall- und Festkorperphysik,** VOL. 4, PART b, Springer-Verlag, Berlin, 1970, XVI+666 pp., figs., DM 428.

CONTENTS. - **Volume 4:** Magnetische und andere Eigenschaften von Oxiden und verwandten Verbindungen. **Part. b.** - *V: Nichteisengranate.* - *VI: Spinelle.* - *VII: Hexagonale Ferrite.* - *VIII: Substanzenverzeichnis.*

Landolt-Börnstein (Zahlenwerte und Funktionen aus Physik, Chemie, Astronomie, Geophysik und Technik), VOL. II: **Eigenschaften der Materie in ihren Aggregatzuständen.** PART I: **Mechanisch-Thermische Zustandsgrössen,** Springer-Verlag, Berlin, 1971, XVI+944 pp., figs. and tables, DM 560.

CONTENTS. - I. Allgemeine Tabellen. *I: Masssysteme.* - *II: Praktische Temperaturmessung.* - *III: Volumenbestimmungen.* - *IV: Druckmessungen.* - *V: Härte.* - *VI: Allgemeine Tabellen zur Systematik der chemischen Elemente.* - II. Mechanisch-thermische Konstanten homogener Systeme. *I: Dichte, Ausdehnung und Kompressibilität im thermischen Gleichgewicht als Funktion von Druck und Temperatur.* - *II: Gleichgewichte nichtionisierter Stoffe in Lösungen.*

LEGHISSA, S., **Morfologia dei vertebrati,** Unione Tipografico-Editrice Torinese, Torino, 1970, XII+746 pp., figs. and tables, L. 20.000.

CONTENTS. - *I: Storia evolutiva dei vertebrati.* - *II: Aspetti morfologici e fisiologici dell'organizzazione dei vertebrati.* - *III: Il sistema tegumentario.* - *IV: Il sistema nervoso centrale.* - *V: Il sistema nervoso periferico e gli organi di senso.* - *VI: Apparato scheletrico.* - *VII: Il sistema muscolare.* - *VIII: Il sistema alimentare.* - *IX: Apparati per la circolazione del sangue e della linfa.* - *X: Sistema uro-genitale.* - *XI: Il sistema endocrino.*

Le materie scientifiche nelle scuole secondarie (Un'inchiesta sulle opinioni degli studenti e degli insegnanti), (Series: *Problemi della Società Italiana,* No. 29), Società Editrice Il Mulino, Bologna, 1970, 244 pp., figs. and tables, L. 2.000.

CONTENTS. - *I: L'insegnamento delle materie scientifiche.* - *II: Le opinioni degli studenti.* - *III: Le opinioni degli insegnanti.*

Le Portugal (Series: *Monde et voyages*), Librairie Larousse, Paris, 1970, 160 pp., figs., 33 F.

On retrouve dans cet ouvrage toutes les qualités qui contribuent au succès de cette collection d'encyclopédies historiques et culturelles. *Le Portugal* offre, en 160 pages, toutes les connaissances qui permettent de comprendre et d'apprécier ce pays touristique par excellence. L'ouvrage est divisé en dix grands chapitres: les paysages - le passé -

le présent - les grandes étapes (Conimbriga, Coimbra, Porto, Guimarães, Alcobaça, Batalha, Evora, Tomar, Sagres, Beja, Sintra, Lisbonne) - la vie quotidienne - les traditions - l'art - la littérature - la musique - les vacances. On ne se lassera pas de consulter *Le Portugal* avant, pendant et après le voyage: mais ce genre d'encyclopédies n'intéresse pas seulement le touriste. Par l'esprit de synthèse qui anime cet ensemble et qui s'appuie sur des milliers de petits faits vrais, par l'actualité de la documentation réunie, l'ouvrage constitue une initiation remarquable qui sera utile aussi bien à l'homme d'affaires qu'à l'amateur d'art ou d'histoire, ou tout simplement à tous ceux, soucieux de connaître, en peu de pages, le vrai visage du Portugal d'aujourd'hui.

LERNER, I. M., **Genetic Homeostasis,** Dover Publications, Inc., New York, 1970, VIII+134 pp., figs. and tables, $2.25.

The total of steady states thus achieved within the organism is denoted as homeostasis. In this book the thesis is advanced and developed that a parallel homeostatic process occurs at the level of interbreeding groups making for stability in the inheritance of a given population. The author, Professor of Genetics at the University of California in Berkeley, holds that Mendelian populations possess self-regulating properties, that a connection exists between genetic and developmental homeostasis, and that heterozygosity possibly provides a basis for both phenomena. These concepts are presented in terms of results of investigations of various animal populations: chickens, *Drosophilia*, higher animals, and plants, The evidence concerns data on artificial selection, observations on phenotypic deviants and estimates of variability of homozygotes and heterozygotes. Genetic models underlying the theory are then presented, followed by theoretic justification. An introduction to the general concept of homeostasis and its physiological, social and ecological applications precedes the main body of the work. The author's entrance to the subject is via his well known work in animal improvement, and his evidence is buttressed by mathematical and diagrammatic analysis.

LIDZ, T. and EDELSON, M. (eds.), **Training Tomorrow's Psychiatrist** (The Crisis in Curriculum), Yale University Press, London, 1970, XIV+94 pp., £0.90.

CONTENTS. - *I: Toward Schools of Psychiatry*, by T. LIDZ. - *II: The Integration of the Behavioral Sciences and Clinical Experience in Teaching Medical Students*, by M. EDELSON. - *III: The « Mixed Psychiatry-Medicine » Internship Program of the Upstate Medical Center*, by D. OKEN. - *IV: Teaching of Community Psychiatry in Psychiatric Residency Training Programs*, by S. R. KAPLAN.

LIPPARD, S. J. (ed.), **Progress in Inorganic Chemistry,** VOL. 12, John Wiley & Sons Ltd., London, 1970, 400 pp., figs. and tables, £9.25.

CONTENTS. - *I: Dimethyl Sulfoxide in Inorganic Chemistry*, by W. L. REYNOLDS. - *II: Electron Transfer Spectra*, by C. KLIXBÜLL JORGENSEN. - *III: Spectroscopy of 3d Complexes*, by J. FERGUSON. - *IV: Structural Systematics of 1,1- and 1,2-Dithiolato Chelates*, by R. EISENBERG.

LOCKWOOD, A. P. M., **Aspects of the Physiology of Crustacea** (Series: *University Reviews in Biology*, No. 8), Oliver & Boyd, Edinburgh, 1968, x+328 pp., figs. and tables, £2.75.

CONTENTS. - *I: Introduction. - II: Osmotic and Ionic Regulation. - III: Moulting. - IV: Hormones. - V: The Blood System. - VI: Respiration and Metabolism. - VII: The Neuromuscular System. - VIII: Sense Organs. - IX: Feeding and Digestion.*

LONCHAMP, J.-P., **Thermodynamique et introduction à la physique statistique** (Enseignement supérieur, premier cycle, MP-PC, 2e année), Masson & C.ie, Paris, 1970, 210 pp., figs., 28 F.

CONTENTS. - *I: Terminologie. Notion de température. - II: Thermométrie. - III: Equation d'état. Gaz parfait. Coefficients élastiques. - IV: Travail. - V: Premier principe de la thermodynamique. - VI: Applications du premier principe. - VII: Second principe de la thermodynamique. - VIII: Entropie. - IX: Quelques résultats découlant des deux principes. - X: Les potentiels thermodynamiques. - XI: Etude macroscopique des gaz réels. - XII: Changements d'état des corps purs. - XIII: Le troisième principe de la thermodynamique ou principe de Nernst. - XIV: Les concepts élémentaires de la mécanique statistique. Statistique classique de Maxwel-Boltzman. - XV: Quelques applications de la statistique de Maxwell-Boltzman.*

LORRAIN, P. and CORSON, D. R., **Electromagnetic Fields and Waves**, 2nd ed., W. H. Freeman and Company, Berkshire, 1970, XXVI+706 pp., figs. and tables, £4.00.

CONTENTS. - *I: Vectors. - II: Electrostatic Fields I. - III: Electrostatic Fields II. - IV: Electrostatic Fields III. - V: Relativity I. - VI: Relativity II. - VII: Magnetic Fields I. - VIII: Magnetic Fields II. - IX: Magnetic Fields III. - X: Maxwell's Equations. - XI: Propagation of Electromagnetic Waves I. - XII: Propagation of Electromagnetic Waves II. - XIII: Propagation of Electromagnetic Waves III. - XIV: Radiation of Electromagnetic Waves.*

MANNING, A., **An Introduction to Animal Behaviour** (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1968, VIII+208 pp., figs. and plates, £2.80.

This book is an introductory text which deals with animal behaviour from a biological point of view, linking it with physiology on the one hand and experimental psychology on the other. Ethologists pioneered the modern biological approach to behaviour, but their work and outlook have changed greatly over the past decade. This book includes much of the more recent ethological work, which has so far been described only in one or two advanced texts.

CONTENTS. - *I: Reflexes and Complex Behaviour. - II: The Development of Behaviour. - III: External Stimuli. - IV: Motivation. - V: Conflict Behaviour. - VI: Hormones and Behaviour. - VII: Evolution. - VIII: Learning-1. - IX: Learning-2.*

MARGULIS, L., **Origin of Eukaryotic Cells** (Evidence and Research Implications for a Theory of the Origin and Evolution of Microbial, Plant, and Animal Cells on the Precambrian Earth), Yale University Press, London, 1970, XXII+350 pp., figs. and tables, £6.75.

This book presents a theory of the origin of eukaryotic cells ("higher" animal and plant cells which divide by classical mitosis), in which hereditary endosymbiosis is reconsidered as a major evolutionary mechanism. Based on her cell symbiosis theory a single, dipartite family tree for all the lower forms is constructed for the bacteria, blue green algae and their relatives, the protozoa; the green, brown, and red algae; the fungi and other groups of lower organisms. This phylogenetic tree offers new ways of looking at microbial evolution during the eons of the Precambrian; more importantly, it provides laboratory scientists with experimentally testable hypotheses. Although a direct outgrowth of late nineteenth- and early twentieth-century biological thought, in its present form the theory is new. Much of the argument is based upon reinterpretations of cell biology and cytoplasmic inheritance data.

CONTENTS. - *I: Problems of Cellular Evolution. - II: The Symbiotic Theory. - III: Precellular Origin of Life. - IV: Prokaryote Microbes on the Ancient Earth. - V: Photosynthesis and the Elimination of Oxygen. - VI: Symbiosis. - VII: Aerobiosis and the Mitochondrion. - VIII: Primitive Eukaryotes: Sex and the Complex (9+2) Flagellum. - IX: Lower Eukaryotes. - X: Eukaryotic Plants. - XI: Epilogue: Cellular Evolution During Phanerozoic Times.*

MASTERS, W. H. and JOHNSON, V. E., **Patologia e terapia del rapporto coniugale** (Le insufficienze sessuali nell'uomo e nella donna), 2nd ed., Feltrinelli Editore, Milano, 1971, 448 pp., figs. and tables, L. 4.000.

In questo Volume gli autori riferiscono sui risultati ottenuti in undici anni di trattamento clinico delle disfunzioni sessuali nel rapporto coniugale. In questo libro si esaminano — con una vasta esemplificazione casistica — le manifestazioni tipiche di ogni disfunzione e si illustrano dettagliatamente i sistemi di cura del trattameto intensivo e quelli da eseguire in privato.

MATIJEVIĆ, E. (ed.), **Surface and Colloid Science,** VOL. 3, John Wiley & Sons Ltd., London, 1971, XII+296 pp., figs. and tables, £8.00.

CONTENTS. - *I: Statistical Mechanics of the Capillary Layer,* by F. C. GOODRICH. - *II: Dynamic Surface Tension,* by R. DEFAY and G. PÉTRÉ. - *III: Dielectric Properties of Disperse Systems,* by S. S. DUKHIN. - *IV: Thin Liquid Films,* by J. S. CLUNIE, J. F. GOODMAN and B. T. INGRAM. - *V: Colloidal Microcrystal Polymer Science,* by O. A. BATTISTA, M. M. CRUZ and C. V. FERRARO.

MENZEL, W., **Theorie der Lernsysteme,** Springer-Verlag, Berlin, 1970, VIII+160 pp., figs., DM 22.

CONTENTS. - *I: Einleitung. - II: Terminologische Vorbemerkungen. - III: Grundbegriffe. - IV: Ordnungsbeziehungen. - V: Darstellung eines Lernsystems als Automat. Endlichkeitsbedingungen. - VI: Lernaufgaben und Kapazität. - VII: Lokalfinite Lernsysteme. - VIII: Finite Lernsysteme. - IX: Synthese finit lösender Lernsysteme. - X: Diskussion spezieller Modelle und Lerntypen.*

MERCIER, R., VALETTE, J. and VANNEUVILLE, G., **Affections vasculaires des membres et kinésithérapie,** Librairie Maloine S.A., Paris, 1970, 364 pp., figs., 72 F.

Cet ouvrage se propose de faire le point des connaissances actuelles utiles aux médecins et aux kinésithérapeutes en matière de maladies vasculaires des membres. Il expose en outre des techniques kinésithérapiques mises au point par les auteurs dans les suites d'artérites et de stripping. Après un rappel des notions élémentaires d'anatomie et de physiologie circulatoires (artérielle, veineuse mais aussi lymphatique), les auteurs décrivent séparément les principales maladies des artères, des veines et des lymphatiques. Pour chaque affection traitée, ils envisagent successivement la clinique et la thérapeutique qui est tout particulièrement développée.

MOSINGER, M., DUMOULIN, J., DE BISSCHOP, G. and CLAUSES, I., **L'exploration neuro-musculaire** (Series: *Les Précis Pratiques*), Librairie Maloine S.A., Paris, 1970, 590 pp., figs., 96 F.

Cet ouvrage envisage l'exploration neuro-musculaire périphérique dans le cadre des examens pour lesquels de plus en plus le praticien est sollicité dans ses investigations quotidiennes. Dans une première partie sont envisagées toutes les méthodes électriques ainsi que le testing musculaire qui peuvent être demandés aux électrophysiologistes lors d'examens qui entrent depuis une quinzaine d'années dans la pratique courante. La seconde partie étudie principalement les méthodes d'exploration histologique: biopsies musculaires, méthodes de colorations, anatomie pathologique, nouvelles classifications des myopathies ainsi que les méthodes biochimiques.

MOTOKAWA, K., **Physiology of Color and Pattern Vision,** Springer-Verlag, Berlin, 1970, 284 pp., figs. and tables, DM 72.

The purpose of this book is to present the major studies on vision performed in the laboratory of Tohoku University during the past twenty-five years, and also to describe in some detail related studies in other laboratories.

CONTENTS. - *I: Retina. - II: Visual Pathway. - III: Pattern Vision. - IV: Electrical Excitability of the Eye as an Indicator of Vision. - V: Retinal Induction. - VI: Wawe Nature of Retinal Induction. - VII: Electrical Excitation of the Eye with AC (Characteristic Frequencies). - VIII: Color Vision and its Theory. - IX: Optical Illusion and Figural Aftereffect. - X: Movement Sensation and Stereoscopic Vision. - XI: Simulation of the Visual Sistem.*

MUSU, A. M., **Motivi e strutture dell'istruzione** (Series: *Problemi del nostro tempo*), Vallecchi Editore, Firenze, 1970, 290 pp., L. 3.000.

CONTENTS. - Introduzione. - *I: La scuola. - II: Il linguaggio. - III: La scuola di cultura. - IV: L'apprendimento come momento scientifico dell'educazione permanente.* - Intermezzo. - *V: Strutture umane per l'istruzione. - VI: Motivi e strutture della verifica dell'istruzione.* - Appendice.

NANTET, J.-P., **Ordinateurs en temps réel** (Applications industrielles), (Series: *Monographies d'Electronique*), Masson & C.ie, Paris, 1970, VIII+330 pp., figs., 75 F.

CONTENTS. - *I: Généralités. - II: Logique et algèbre binaires. Rappels. - III: Technologie. Rappels. - IV: L'unité de traitement. Rappels. - V: Organes périphériques standards. - VI: Organes périphériques temps réel. - VII: Programmation. - VIII: Aides à la programmation. - IX: Connexion à un processus industriel. - X: Programmation de systèmes industriels. - XI: Applications industrielles hors ligne. - XII: Applications industrielles en ligne.*

NEUMAN, M., **Vade-Mecum des antibiotiques et agents chimiothérapiques anti-infectieux,** 2nd ed., Librairie Maloine S.A., Paris, 1971, 414 pp., tables, 48 F.

La 2e édition de cet ouvrage qui a connu un vif succès lors de sa parution en 1961 est une refonte intégrale du texte puisque, dans ce domaine, les 10 ans passés depuis la première édition ont apporté des changements fondamentaux dans le domaine si mouvant de la chimiothérapie anti-microbienne. Les différents antibiotiques sont groupés en familles; pour chaque famille, il y a une présentation d'ensemble ainsi qu'une description analytique de chaque antibiotique. L'ouvrage est illustré de nombreux tableaux synoptiques qui facilitent une vue de synthèse. On y donne également les synonymes étrangers de tous les antibiotiques.

O'MALLEY, J. (ed.), **Critique of Hegel's 'Philosophy of Right' by Karl Marx** (Series: *Cambridge Studies in the History and Theory of Politics*), Cambridge University Press, London, 1970, LXVIII+152 pp., £2.75.

In a substantial introduction, Professor O'Malley provides valuable information on Marx's intellectual development, guides the reader through the text by clarifying its methodological and doctrinal features, and sets it in historical context. The edition also includes a new translation of the introduction Marx wrote for his proposed revised version of the *Critique* which he never completed.

Organization and Control in Prokaryotic and Eukaryotic Cells (Twentieth Symposium of the Society for General Microbiology held at Imperial College, London, April 1970), Cambridge University Press, London, 1970, XII+458 pp., figs., tables and plates, £5.

This symposium of the Society for General Microbiology poses and attempts to answer the question of the significance of the differences between pro- and eu-karyotic cells. The differences revealed by electron microscopy appear quite basic. Prokaryotic cells do not have nuclear membranes or true chromosomes, nor do they have mitochondria, chloroplasts and other organelles characteristic of the cells of eukaryotes. Prokaryotic cells occur only in bacteria and bleu-green algae; all other organisms have eukaryotic cells. The symposium has not been restricted to consideration of the cells of the micro-organisms above, because in many areas it is the cells of higher plants and animals that have been studied. Wide generalizations have been made based on the study of few micro-organisms, but it is evident that deeper understanding of the apparent discontinuity between pro- and eu-karyotes and of the evolution of eukaryotes from prokaryotes requires much greater knowledge of the less studied groups of micro-organisms.

CONTENTS. - *I:* R. Y. STANIER, *Some Aspects of the Biology of Cells and their Possible Evolutionary Significance.* - *II:* C. R. WOESE, *The Genetic Code in Prokaryotes and Eukaryotes.* - *III:* H. G. WITTMANN, *A Comparison of Ribosomes from Prokaryotes and Eukaryotes.* - *IV:* U. E. LOENING, *The Mechanism of Synthesis of Ribosomal RNA.* - *V:* H. J. VOGEL, J. S. THOMPSON and G. D. SHOCKMAN, *Characteristic Metabolica Patterns of Prokaryotes and Eukaryotes.* - *VI:* P. MITCHELL, *Membranes of Cells and Organelles: Morphology, Transport and Metabolism.* - *VII:* D. ELLAR, *The Biosynthesis of Protective Surface Structures of Prokaryotic and Eukaryotic Cells.* - *VIII:* M. C. W. EVANS and F. R. WHATLEY, *Photosynthetic Mechanisms in Prokaryotes and Eukaryotes.* - *IX:* P. ECHLIN, *The Photosynthetic Apparatus in Prokaryotes and Eukaryotes.* - *X:* M. H. RICHMOND, *Plasmids and Chromosomes in Prokaryotic Cells.* - *XI:* W. F. BODMER, *The Evolutionary Significance of Recombination in Prokaryotes.* - *XII:* D. E. HUGHES, D. LLOYD and R. BRIGHTWELL, *Structure, Function and Distribution of Organelles in Prokaryotic and Eukaryotic Microbes.* - *XIII:* A. A. NEWTON, *The Requirements of a Virus.* - *XIV:* R. HOLLIDAY, *The Organization of DNA in Eukaryotic Chromosomes.* - *XV:* D. WILKIE, *Reproduction of Mitochondria and Chloroplasts.* - *XVI:* J. R. RAPER and A. S. FLEXER, *The Road to Diploidy with Emphasis on a Detour.*

ORLOFF, S., **Etude sur l'ostéolathyrisme** (Series: « *Médico-Monographies d'Agrégés* »), Editions Arscia S.A., Bruxelles, 1970, 160 pp., figs. and tables, 310 F.B.

CONTENTS. - I. Introduction. *I: Définition du lathyrisme.* - *II: Agents étiologiques.* - *III: Description des lésions de l'ostéolathyrisme.* - II. Résumé de l'introduction. - III. Exposé du travail. - IV. Matériel et méthodes. - V. Recherches personnelles. *I: Effet de l'intoxication lathyrique sur les propriétés physico-chimiques du collagène de l'embryon.* - *II: Etudes in vitro.* - *III: Etudes métaboliques préliminaires (par refroidissement de la température d'incubation des embryons).* - *IV: Etudes métaboliques.* - VI. Discussion.

PEREZ, Ch., **Répertoire des variations biologiques en pathologie clinique** (Approche instantanée du diagnostic), 2nd rev. ed., Librairie Maloine S.A., Paris, 1970, 330 pp., 60 F.

L'évolution de la Biologie Clinique a conduit l'auteur à refondre son livre et à l'enrichir de nombreuses informations récentes. Le présent ouvrage comprend deux parties: La première traite des modifications en pathologie humaine: pour chaque type d'affection sont indiqués le sens et l'amplitude des variations biologiques concernant l'hématologie, l'immunologie, la bactériologie, la sériologie, la parasitologie et surtout la chimique clinique. La seconde rassemble les affections possédant une même particularité biologique accompagnée elle-même de ses valeurs normales et de notules les concernant. L'obtention rapide des renseignements reste le but essentiel de l'ouvrage dont la présentation pratique évite au lecteur de longues et fastidieuses recherches dans des articles de revues et des ouvrages généraux de médecine ou de biologie clinique.

PERRY, N. W., Jr. and CHILDERS, D. G., **The Human Visual Evoked Response** (Method and Theory), Charles C. Thomas, Publisher, Springfield, 1969, XIV+188 pp., figs. and tables, $14.00.

This volume is oriented around the authors' belief that the ability to observe cortical events in the intact human in response to visual stimulation can serve as a "Window to the brain". Particular attention is directed towards practicality. Major chapters are devoted to the physiological significance of the visual evoked response, the effects of stimulation variables, and the effects of subject variables. Possible research and methodological challenges are summarized—current and future clinical applications are considered.

Contents. - *I: Physiological Significance of the Visual Evoked Response. - II: Stimulation Variables. - III: Subject Variables. - IV: Acquisition: Procedures and Instrumentation. - V: Signal-Averaging Computers. - VI: Analysis Techniques for the Ver. - VII: Summary and Conclusions.* - Appendices.

PÉTRACHÈNE, M. and TRIFONOV, E., **Applications de la théorie des groupes à la mécanique quantique,** Masson & C.ie, Paris, 1970, 252 pp., figs., 65 F.

Il s'agit d'un exposé tiré d'un cours professé par les auteurs en quatrième année de physique théorique à la Faculté des Sciences de l'Université de Leningrad. Ce livre constitue une introduction à la théorie des groupes (finis ou continus) et aux applications de la théorie des représentations à la mécanique quantique. Les applications considérées ont trait, entre autres: à la théorie de l'atome, à la chimie quantique, à la physique du solide, à la mécanique quantique relativiste.

Physique des dispositifs à semiconducteurs, Vol. I: **Electronique du silicium homogène,** by A. Vapaille, (Series: *Monographies d'Electronique*), Masson & C.ie, Paris, 1970, x+340 pp., figs. and tables, 150 F.

Cet ouvrage est le premier tome d'une série consacrée aux mécanismes physiques mis en jeu dans le fonctionnement des dispositifs à semiconducteurs. Tout dispositif semiconducteur étant élaboré à partir d'un matériau homogène, il a paru nécessaire de commencer par décrire les propriétés des semiconducteurs uniformément dopés. On s'est limité aux propriétés électriques dont dépend le fonctionnement des dispositifs. On s'est également limité aux propriétés « dans le volume », laissant de côté les propriétés de surface. Dans tout l'exposé, le silicium a été délibérément choisi comme exemple, il est en effet de beaucoup le matériau semiconducteur le plus utilisé dans la technologie des dispositifs. L'ouvrage s'adresse aux étudiants ou ingénieurs désirant se spécialiser dans le domaine des semiconducteurs. Son étude peut être abordée par tout lecteur possédant quelques connaissances générales de Physique. (On ne suppose pas que le lecteur soit familier de la Mécanique Quantique ou de la Thermodynamique Statistique).

REID, C., **Hilbert** (With an Appreciation of Hilbert's Mathematical Work by H. Weyl), Springer-Verlag, Berlin, 1970, xii+290 pp., figs., DM 32.

Contents. - *I: Youth. - II: Friends and Teachers. - III: Doctor of Philosophy. - IV: Paris. - V: Gordan's Problem. - VI: Changes. - VII: Only Number Fields. - VIII: Tables, Chairs, and Beer Mugs. - IX: Problems. - X: The Future of Mathematics. - XI: The New Century. - XII: Second Youth. - XIII: The Passionate Scientific Life. - XIV: Space, Time and Number. - XV: Friends and Students. - XVI: Physics. - XVII: War. - XVIII: The Foundations of Mathematics. - XIX: The New Order. - XX: The Infinite! - XXI: Borrowed Time. - XXII: Logic and the Understanding of Nature. - XXIII: Exodus. - XXIV: Age. - XXV: The Last Word.*

ROSSI DELL'ACQUA, A., TORNAGHI, L., PALLOTTINO, P. and CAPELLO, C. (eds.), **Schede di lavoro per il secondo ciclo** (La teoria degli insiemi nella scuola dell'obbligo), Zanichelli, Bologna, 1970, 168 pp., figs., L. 2.500.

Contents. - *I: Insiemi e sottoinsiemi. - II: Intersezione di insiemi. - III: Partizione. - IV: Prodotto cartesiano. - V: Addizione. - VI: Relazioni. - VII: Problemi. - VIII: Dalla moltiplicazione alla divisione. - IX: Divisione. - X: Divisione approssimata. - XI: Lunghezze. - XII: Ricoprimento di superfici. - XIII: Peso. - XIV: Capacità. - XV: Problemi.*

ROUBAULT, M., **Peut-on prévoir les catastrophes naturelles?,** Presses Universitaires de France, Paris, 1970, 174 pp., figs., 19 F.

CONTENTS. - *I: Les inondations. - II: Les tremblements de terre. - III: Les glissements de terrains et éboulements de rochers. - IV: Les avalanches. - V: La rupture des grands barrages. - VI: Problèmes administratifs. - VII: Problèmes juridiques.*

RUSINOV, V. S. (ed.), **Electrophysiology of the Central Nervous System,** Plenum Press, New York, 1970, XII+516 pp., figs. and tables, $42.00.

Leading international electrophysiologists present reviews of recent advances in the study of single neuronal activity, coordination between activity of different neurons, and global activity of the cerebral cortex. Results obtained by application of modern computer techniques and mathematical methods of analysis to electrophysiological data are stressed throughout. The book contains a wealth of information on electrophysiological phenomena relevant to behavior as well as excellent contributions to the analysis of more fundamental neurophysiological problems.

RUSSEL, F. S. and YONGE, M. (eds.), **Advances in Marine Biology,** VOLUME 8, Academic Press, London, 1970, XII+482 pp., figs., tables and plates, £7.00.

CONTENTS. - I. Particulate and Organic Matter in Sea Water, by G. A. RILEY. *I: Introduction. - II: Distribution of Particulate Organic Matter in the Sea. - III: Chemical Composition. - IV: Experimental Sudies of Non-living Particulate Matter. - V: General Discussion. - VI: References.* - II. Estuarine Fish Farming, by C. F. HICKLING. *I: Introduction. - II: Suitability of Estuaries as Sites for Fish Farms. - III: The Species Cultured. - IV: Sources of Young Fish and Prawns. - V: The Food of Cultivated Fish and Prawns. - VI: Management of Brackish-water Fishponds. - VII: The Rate of Fish Production. - VIII: Profitability. - IX: Is There a Future for Estuarine Fish Farming? - X: References.* - III. The Problem of Oil Pollution of the Sea, by A. NELSON-SMITH. *I: Introduction. - II: Sources and Control. - III: Properties of Petroleum Oils. - IV: Effects of Oil Pollution. - V: Removal of Spilt Oil. - VI: Conclusions and Prospects. - VII: References.* - IV. Scatological Studies of the Bivalvia (Mollusca), by K. Y. ARAKAWA. *I: Introduction. - II: Material and Techniques. - III: Morphology. - IV: Biological Significance of the Characteristic Form of Faecal Pellets. - V: Use of Faecal Pellets as a Systematic Index. - VI: Evolutionary Trends of Faecal Pellets. - VII: Bio-deposition of Suspension Feeding Bivalves. - VIII: Bibliography. - IX: Plates.*

SACHS, L., **Statistische Methoden** (Ein Soforthelfer), Springer-Verlag, Berlin, 1970, XII+104 pp., figs. and tables, DM 8.80.

CONTENTS. - *I: Grundlagen und Ziel statistischer Methoden. - II: Mittelwerte und Variabilität, unklassifizierte Beobachtungen. - III: Häufigkeitsverteilung und Summenhäufigkeitsverteilung. - IV: Normalverteilung. - V: Vertrauensbereich. - VI: Statistische Tests. - VII: Wieviel Beobachtungen werden benötigt? - VIII: Korrelation und Regression. - IX: Anhang: Schnellverfahren für den Vergleich mehrerer Mittelwerte.*

SALMON, Am., **Philippe de Beaumanoir** (Coutumes de Beauvaisis), 2 vols., Editions A. et J. Picard, Paris, 1970, XLVI+512 pp. and 552 pp., 75 F.

Cette réimpression des 2 volumes des « Coutumes de Beauvaisis » sera suivie d'un Tome III qui paraîtra en 1971. Ce Tome III sera un commentaire à la fois historique et juridique du Texte des « Coutumes ». Il comportera une introduction générale et un commentaire explicatif consacré à chacun des 70 chapitres des « Coutumes ». Ce commentaire qui manquait à la 1re édition fera ressortir l'importance exceptionnelle de BEAUMANOIR pour la connaissance du droit coutumier français public et privé, à la fin du XIIIe siècle.

SALZANO, F. M. and FREIRE-MAIA, N., **Problems in Human Biology** (A Study of Brazilian Populations), Wayne State University Press, Detroit, 1970, 200 pp., figs. and tables, $8.95.

Brazil's mixed population of Indians, Negroes, White Europeans, and Asiatic people in all degrees of interbreeding—and settled in the most extreme variety of environmental condition—soffers a fertile field of ideal conditions for population and mainly genetic studies. *Problems in Human Biology—A Study of Brazilian Populations* is a synthetic approach to the understanding of genetic variability which utilizes data from the fields of genetics, demography, and anthropology in the study of Brazil's complex population of eightyfive million.

Contents. - *I: The Origins. - II: Immigration and Admixture. - III: Birth and Death. - IV: Consanguineous Marriages. - V: Isolates and Migration. - VI: Genetic Load. - VII: Congenital Malformations, Twinning, Chromosome Aberrations, and Hereditary Diseases. - VIII: Morphology. - IX: Genetic Polymorphisms. - X: An Analysis of Some Genetic Polymorphisms. - XI: Synthesis.*

SCHULTZE, J. H. (ed.), **Aktuelle Probleme Geographischer Forschung** (Series: *Geographische Abhandlung*, No. 13), Dietrich Reimer Verlag, Berlin, 1970, 550 pp., figs. and plates, DM 60.

Contents. - *I:* E. Winkler, *Das System der Erdwissenschaften und die Geographie. - II:* G. Chabot, *Géographie régionale et psychologie dans la géographie française contemporaine. - III:* E. W. Gilbert, *English Regional Novels and Geography. - IV:* L. Aario, *Landhebung und Eisisostasie in Finnland. - V:* J. P. Bakker, F. J. P. M. Kwaad and H. J. Müller, *Experimente über rhythmische kutane Wasserdampfaufnahme in lockerem tonhaltigem Material bei abwechselnd hoher und niedriger relativer Feuchtigkeit der bodennahen Luftschicht, besonders unter ariden und semiariden Bedingungen (Eine Deszendenz- und Inversionstheorie. - VI:* H. Mensching, *Geomorphologische Beobachtungen in der Inselberglandschaft südlich des Victoria-Sees (Tanzania). - VII:* A. Gerstenhauer, *Beiträge zur Genese der Poljen in den wechselfeuchten Tropen. - VIII:* H. Schirmer, *Beitrag zur Methodik der Erfassung der regionalen Nebelstruktur. - IX:* K.-H. Kaiser, *Über Konvergenzen arider und « periglazialer » Oberflächenformung und zur Frage einer Trockengrenze solifluidaler Wirkungen am Beispiel des Tibesti-Gebietes in der zentralen Ostsahara. - X:* R. E. Dickinson, *Some Problems of Urban Geography. - XI:* F. Friedensburg, *Die multiplikatorische Wirkung von Bergbauunternehmen in unterentwickelten Gebieten. - XII:* G. Niemeier, *Salzgitter und Wolfsburg als administrative und geographische Stadteinheiten. - XIII:* W. Manshard, *Some Aspects of the Development of Market-gardening and Fruit-growing in South-East Hampshire. - XIV:* K. H. Olsen, *Das römische Stadtquartier EUR: Trabant, Neue City oder Administration District? - XV:* W. Wöhlke, *Die Entwicklung der agraren Kulturlandschaft im Tiefland Ost- und Ostmitteleuropas. - XVI:* K.-A. Boesler, *Infrastrukturpolitik und Dynamik der zentralen Orte. - XVII:* F. J. W. Bader, *Weinbau und Landschaft in den württembergischen Weinbaugebieten westlich des Neckars. - XVIII:* B. Hofmeiser, *Gegenwärtige Struktur und « Metropolitan Planning » in nordamerikanischen Grossstädten. - XIX:* K. D. Wiek, *Struktur und Gefälle ökonomischer Regionen. - XX:* W.-H. Schuch, *Abwanderungsgebiete und Neusiedlungsräume in Brasilien. - XXI:* B. Aust, *Dorfstrukturveränderung durch Aussiedlung. Dargestellt an Beispielen aus der Kalk- und Osteifel. - XXII:* W. Duckert, *Der Strukturbegriff in Landschafts- und Planungsraumforschung. - XXIII:* D. Partzsch, *Welchen Beitrag erwartet die Raumordnungspolitik von der geographischen Wissenschaft? - XXIV:* G. Mielitz, *Die italienische Landesentwicklungspolitik am Beispiel des industriellen Entwicklungsgebietes Tarent. - XXV:* H. Hecklau, *Bewässerungsfeldbau in Kenya. - XXVI:* D. O. Müller, *Die Südgrenze Ugandas. Entstehungsweise und Bewertung einer kolonialen Grenze. - XXVII:* K.-H. Hasselmann, *Kulturlandschaftsstudien in Busoga/Uganda dargestellt am Beispiel Namasagali. - XXVIII:* F. Vetter, *Zum Wert von Netzmodellen in der Verkehrsgeographie.*

SOMENZI, V. (ed.), **L'evoluzionismo** (Series: *Classici della filosofia*), Loescher Editore, Torino, 1971, xii+300 pp., L. 1.650.

Questa antologia, pubblicata nella collezione « Classici della filosofia » a cura di Vittorio Somenzi, comprende scritti di G. Montalenti, J. B. Lamarck, C. Darwin, T. H. Huxley, E. Haeckel, E. Cassirer, B. Farrington, P. Omodeo, P. B. Medawar. I testi, alcuni dei quali sono ormai dei classici, sono seguiti da un glossario.

STANCOVICI, V., **Ideeà de numar**, Editura Stiintifica, Bucaresti, 1971, 150 pp., Lei 4,25.

Contents. - *I: La condition d'un instrument logique de connaissance, d'expression et de démonstration. - II: Bref historique de l'idée du nombre. - III: Quelques définitions sur l'idée du nombre. - IV: L'idée du nombre. - V: Les fondements proprement dits logiques des mathématiques. Le contenu de l'idée du nombre. - VI: Le contenu de l'idée d'identité: A = A. - VII: Les principes logiques - principes ou axiomes communs de toutes les sciences. - VIII: Conséquences logiques de la définition de l'idée du nombre. - IX: Conséquences et propriétés des nombres. La logique des nombres. - X: Conséquences et propriétés des jugements arithmétiques. La logique de l'opération arithmétique. - XI: Conséquences et propriétés des calculs. La logique du calcul. - XII: Conséquences et propriétés des sciences mathématiques. A. La logique et l'Arithmétique. Pourquoi n'est-elle possible qu'une arithmétique unique? La noncontradiction de l'arithmétique. - XIII: Conséquences et propriétés des sciences mathématiques. B. Le rapport logique des axiomes de la géométrie. - XIV: Conséquences et propriétés des sciences mathématiques. C. Pourquoi plusieurs géométries sont-elles possibles? - XV: Conséquences et propriétés des sciences mathématiques. D. Le raisonnement mathématique. - XVI: Conséquences et propriétés de toutes les autres sciences. E. L'unité logique de la Science. La logique, les mathématiques et la science. - XVII: Implications et explications logiques. - Conclusion.*

SUTHERLAND, J. M. and TAIT, H., **The Epilepsies** (Modern Diagnosis and Treatment), E. & S. Livingstone Ltd., Edinburgh, 1969, 128 pp., figs. and tables, £1.25.

Dr Sutherland and Dr Tait have written an up-to-date and realistic account of epilepsy, based on personal interest and vast clinical experience, that goes far beyond the best of text-book presentations. Not only will it stimulate prefessional interest in the condition, but indirectly it will bring the benefits of more effective treatment to thousands of patients.

Contents. - *I: General Considerations and Classification. - II: The Clinical Features of Epilepsy. - III: A Note on Certain Varieties of Epilepsy. - IV: Conditions which may Simulate Epilepsy. - V: Ancillary Investigations in Epilepsy. - VI: The Treatment of Epilepsy. - VII: Anticonvulsant Drug Therapy. - VIII: Certain Complications of Epilepsy. - IX: The Surgical Treatment of Epilepsy. - X: Prognosis.*

TAVERNIER, J. and CALECKI, D., **Introduction aux phénomènes de transport linéaires dans les semiconducteurs** (Series: *Monographies de Physique*), Masson et C.ie, Paris, 1970, 238 pp., figs., 80 F.

Cet ouvrage est consacré d'une part au calcul des vecteurs flux et d'autre part à l'interprétation des résultats obtenus. Les développements sont présentés en vue des applications aux cristaux semiconducteurs. Les auteurs ont essentiellement adopté une approche semi-classique basée sur l'équation de Boltzmann et en ont déduit la méthode du gain moyen d'énergie qui facilite la compréhension de certains résultats. Ils ont donné à leur exposé un caractère général et intrinsèque et dégagé les grandes lois reliant les coefficients de transport; enfin ils se sont attachés à présenter des interprétations physiques — tant au niveau macroscopique que microscopique — des principaux phénomènes irréversibles.

The Creative Force of the Leninist Ideas (Series: *Bibliotheca Historica Romaniae Studies*, No. 31), Publishing House of the Academy of the Socialist Republic of Romania, Bucharest, 1970, 306 pp., Lei 12.

Contents. - *I: Leninism and the Romanian Communist Party's Experience in Leading the Revolution and the Construction of Socialism*, by I. Radulescu. - *II: The Penetration of Leninism in Romania. The Influence of the Great October Socialist Revolution on Romania's Working Movement*, by A. Deac. - *III: Vladimir Ilych Lenin's Conception of the Party of a New Type. Setting up of the Romanian Communist Party - an Embodiment of Leninism*, by T. Georgescu. - *IV: The Creative Application by the Romanian Communist Party of the Leninist Theory about the Building of Many-Sidedly*

Developed Socialist Economy, by E. Hutira. - *V: State, Democracy, Nation in Lenin's Conception (the Enlargement of the State Role and the Development of Democracy in Romania in the Present Stage)*, by I. Ceterchi and M. Colceriu-Leiss. - *VI: Internationalism, Sovereignty and Patriotism*, by C. Vlad. - *VII: International Collaboration and Co-operation in the Light of Leninist Ideas*, by C. Murgescu. - *VIII: The Co-ordination of Plans. Main Method of Developing and Completing the Relations of Economic and Technical Co-operation between the Socialist Countries*, by R. Moldovan. - *IX: The Influence of the Marxist-Leninist Ideology on the Contemporary Social Political Thought*, by S. Voicu. - *X: V. I. Lenin and Social Research*, by M. Constantinescu. - *XI: Reflections about Ideology and Science*, by A. Joja. - *XII: The Activity of the Romanian Communist Party in Promoting Science and Technology in the Light of the Leninist Ideas*, by V. Roman. - *XIII: Lenin's Presence in Contemporary Philosophical Thought*, by C. Mare. - *XIV: Leninist Criteria for Interpreting the Trends of Contemporary Thought*, by R. Pantazi. - *XV: Leninist Principles in the Philosophical Research Carried on in Romania*, by C. Ionescu Gulian. - *XVI: V.! I. Lenin and some Problems of Socialist Culture*, by A. Tanase and G. Todea. - *XVII: The Victory of Socialism in Romania Stands Proof to the Viability of Marxism-Leninism*, by I. Popescu-Puturi.

TUSQUES, J., **Initiation à la psychologie médicale**, Librairie Maloine S.A., Paris, 1970, 240 pp., 34 F.

Cette « Initiation » a été rédigée pour le jeunes étudiants en médecine. Elle peut intéresser aussi des médecins praticiens qui cherchent spontanément comment s'orienter pour remédier au malaise qu'ils éprouvent en constatant qu'ils n'ont pas été préparés à exercer la médecine « de ville » ou « de campagne », que beaucoup de leurs malades n'étaient pas du tout ceux qu'ils avaient vus ou qu'on leur avait montrés à l'hôpital, que l'approche clinique à laquelle ils avaient été entraînés était inadéquate à beaucoup de situations dans lesquelles ils se trouvaient. Enfin les infirmières, les kinésitérapeutes, les assistantes sociales pourront certainement trouver dans ce livre une introduction utile à l'apprentissage de leur relation avec le patient.

VACHET, A., **L'idéologie libérale** (L'individu et sa propriété), Editions Anthropos, Paris, 1970, 570 pp., No price stated.

Contents. - I. Les antinomies du libéralisme. Introduction: les bases sociales et l'expression idéologique. Première section: les principaux thèmes du libéralisme. *I: Le naturalisme. - II: La rationalisme. - III: L'individualisme. - IV: Les antinomies.* - Deuxième section: les principales thèses du libéralisme. *V: La liberté. - VI: L'égalité. - VII: La propriété. - VIII: La sûreté.* - II. Interprétations et conciliations. Première section: micro-conciliation, les Physiocrates. *IX: Loi naturelle, loi physique et loi morale. - X: Loi naturelle et propriété. - XI: Propriété et liberté. - XII: Propriété et égalité. - XIII: Société et individualisme économique. - XIV: Droit, politique et économie. - XV: Indépendance et priorité de la propriété dans l'ordre individuel et dans l'ordre social.* - Deuxième section: macro-conciliation, extrapolations et vérifications. *XVI: La dénaturation de la nature. - XVII: L'individuation de la nature et de la raison. - XVIII: L'individu et sa propriété.*

WATT, K. E. F., **Ecology and Resource Management** (A Quantitative Approach), (Series: *Population Biology*), McGraw-Hill Book Company, Düsseldorf, 1968, XII+450 pp., figs. and tables, DM 61.15.

Contents. - I. The Problem of Resource Management. *I: Nature of the Problem. - II: Man's Position and Future Prospects in the World.* - II. The Theory of Resource Management. *III: The Principles of Ecology. - IV: Management Theories Derived from the Principles of Ecology.* - III. The Principles of Resource Management. *V: Impact of the Environment and Man on Population. - VI: Important Ecological Processes. - VII: Conflicts of Interest.* - IV. The Methods of Resource Management. *VIII: Fortran Programming. - IX: Systems Measurement. - X: Systems Analysis. - XI: Systems Description. - XII: Systems Simulation. - XIII: Systems Optimization.*

WEGENER, A., **The Origin of Continents and Oceans,** Methuen & Co. Ltd., London, 1970, XXX+248 pp., figs., £1.00.

CONTENTS. - *I: Istorical Introduction. - II: The Nature of the Drift Theory and Its Relationship to Hitherto Prevalent Accounts of Changes in the Earth's Surface Configuration in Geological Times. - III: Geodetic Arguments. - IV: Geophysical Arguments. - V: Geological Arguments. - VI: Palaeontological and Biological Arguments. - VII: Palaeontological Arguments. - VIII: Fundamentals of Continental Drift and Polar Wandering. - IX: The Displacement Forces. - X: Supplementary Observations on the Sialsphere. - XI: Supplementary Observations on the Ocean Floor.*

WHITE, R. S., **Space Physics,** Gordon and Breach, London, 1970, XIV+318 pp., figs. and tables, £5.5.

This work condenses and unifies the previously separate disciplines of the earth's atmosphere, ionosphere, radiation belts, magnetic cavity and the sun and interplanetary space; it offers a thorough treatment of solar cycle variations and their effects on the earth's environment; and summarizes and criticizes currently accepted theories.

CONTENTS. - *I: The Earth's Radiation Belts. - II: The Earth's Atmosphere. - III: The Earth's Ionosphere. - IV: The Sun. - V: Interplanetary Space. - VI: The Earth's Magnetic Cavity (Magnetosphere).*

WHITTERIDGE, G., **William Harvey and the Circulation of the Blood,** Elsevier Publishing Co., Amsterdam, 1971, XIV+270 pp., Dfl. 47.50.

William Harvey and the Circulation of the Blood is a treatise presenting a balanced approach of the aspects of Harvey's work which are of central interest to historians. The book is divided into three parts. The first discusses the beliefs of the movement of the heart held by Harvey's predecessors. The statements of eminent physicians from Aristotle to Galen are considered in chronological order and illustrated by extracts from their writings; the climate of medical opinion prevalent at the time Harvey was a student at the University of Padua, is carefully evoked. The second part of the book is devoted to a commentary on the evolution of Harvey's own ideas. It is illustrated by excerpts of Harvey's writings from the *Praelectiones* of 1616 to the *De generatione* of 1651. The third part contains passages from the works of Jean Riolan and a consideration of how Harvey's thought developed in answer to criticism and attack.

CONTENTS. - *I: The Information. - II: The Realisation and Publication. - III: Controversy and Final Assessment.* - Appendices.

WILLIAMS, J. G., **A Field Guide to the Butterflies of Africa,** Collins, London, 1969, 238 pp., figs. and plates, £2.25.

This book is an illustrated guide to the commonest and most conspicuous butterflies of Africa. The region covered is Africa south of the Sahara, excluding Madagascar. Out of over 2400 species—many of which the tourist or amateur collector is not likely ever to see—John Williams has chosen 436 of those which the user of this book will have most chance of seeing. For every species are given the English and Latin names; measurements; characters of colour or pattern important for identification or for distinguishing the sexes or related species; and notes on distribution within Africa, habitat, flight, habits and food plants. The author also provides an admirably clear and illustrated introduction to the study and collection of butterflies, particularly in Africa; and a selected bibliography.

WILLIAMS, J. G., **Säugetiere und seltene Vögel in den Nationalparks Ostafrikas** (Ein Taschenbuch für Zoologen und Naturfreunde), Verlag Paul Parey, Hamburg, 1971, 352 pp., figs. and plates, DM 38.

CONTENTS. - I. Die Nationalparks, Wildschutzgebiete und andere Tierreservate. *I: Kenia. - II: Tansania. - III: Uganda.* - II. Die Säugetiere der ostafrikanischen Nationalparks. - III. Die seltenen Vögel der ostafrikanischen Nationalparks.

WILLIAMS, R. A., **Handbook of the Atomic Elements,** Philosophical Library, New York, 1970, 126 pp., $6.00.

This reference book is specifically designed to contain the most important, or at least the most often used, measurements and parameters pertinent to the atomic elements and their classification in periodic categories. The elements, ranging from Hydrogen at Atomic Number 1, through Lawrencium at Atomic Number 103, are arranged in alphabetical order so that any element can be located in less time than it takes to look up a word in the dictionary.

WOOLFSON, M. M., **An Introduction to X-Ray Crystallography,** Cambridge University Press, London, 1970, x+380 pp., figs. and tables, £4.25.

A textbook for the senior undergraduate or graduate student beginning a serious study of X-ray crystallography. It will be useful both to those who intend to become professional X-ray crystallographers and to those physicists, chemists, biologists, geologists, metallurgists, fibre technologists and others who will use it as a tool in their research work.

CONTENTS. - *I: The Geometry of the Crystalline State. - II: The Scattering of X-rays. - III: Diffraction from a Crystal. - IV: The Fourier Transform. - V: The Experimental Collection of Diffraction Data. - VI: The Factors Affecting X-ray Intensities. - VII: The Determination of Space Groups. - VIII: The Determination of Crystal Structures. - IX: Accuracy and Refinement Processes.*

YAPP, W. B., **The Life and Organization of Birds** (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1970, x+246 pp., figs., £3.50.

The theme of this book is that birds are interesting in their own right as animals, and that in organization and intelligence they are the equal of animals other than the primates; they are not merely glorified reptiles. The anatomy and physiology of birds and some aspects of their sexual and social life are discussed from this point of view. The book considers also the control of bird numbers, and their distribution and dispersion, with the evolutionary problems that these involve.

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HISTOIRE DE LA LOGIQUE

LES ÉTAPES DE SON ÉVOLUTION

Première Partie

Dans cette étude, divisée en deux parties, l'auteur se propose de présenter les principales phases par lesquelles a passé la science de la logique. D'après l'auteur, la logique - contrairement aux autres sciences - englobe sa propre histoire, la logique est son histoire. L'étude des diverses formes, revêtues par cette science le long de son développement, prend ainsi une importance capitale et devient indispensable pour la compréhension exacte de l'essence de la logique. Partant de ce point de vue, l'auteur commence son étude par l'examen de la structure logique de la mentalité primitive, de la logique en Chine et de la logique indienne, fixant leurs traits spécifiques. L'analyse de la pensée présocratique permet de prévoir l'apparition naturelle d'Aristote. Les époques suivantes seront examinées dans la seconde partie, qui paraîtra dans le numéro suivant de "Scientia".

I. - INTRODUCTION. — L'histoire d'une science est toujours intéressante, parce qu'elle nous montre son procès évolutif et nous explique son étape actuelle. Mais il y a des sciences qui peuvent être très bien connues sans rien savoir de leur histoire, qui reste à faire l'objet des recherches isolées, des recherches, pour ainsi dire, d'ordre archéologique. Par exemple, la physique peut être parfaitement connue sans aucune connaissance de son passé; la même chose se passe avec l'astronomie, mécanique, chimie, etc. Mais la logique a une situation totalement différente. Elle ne se réduit pas exclusivement aux dernières lois scientifiques établies dans ce domaine, mais elle englobe tout ce qui a été fait de l'antiquité jusqu'à present. *La logique est tout son devenir, elle est la sommation intégrale de tous les moments de son histoire.* De cette manière, cette science montre une unité parfaite, intégrant d'une façon critique tous les problèmes posés, et les solutions données dans le passé, et par cela même n'étant pas étrangère au concept général de science continuellement perfectible.

Ceux qui ont regardé la logique comme une science morte, l'ont ainsi considérée parce qu'ils l'ont réduite à quelques règles de manuel. L'exemple de Kant est célèbre. Selon lui, la logique n'a pas fait un seul pas en avant depuis Aristote, et elle lui semblait "finie et achevée". L'apparition de Hegel, de Husserl, et particulièrement de la logique mathématique, a montré non seulement qu'on ne peut renoncer à Aristote, aux Stoïciens, mais même aux Scolastiques. Pour donner un seul exemple, nous dirons que le calcul propositionnel, dont les logiciens mathématiciens actuels sont si fiers, était bien connu des Stoïciens et des logiciens scolastiques. Il y a aussi d'autres problèmes négligés par la logique actuelle, comme ce sont, par exemple, le problème scolastique des *syncategoremata*, ou celui de la supposition (*sup-*

positio). Nous attendons le moment quand on pourra traiter d'une façon mathématique ces problèmes, pour rendre encore une fois justice aux efforts des logiciens du Moyen Age.

Il y a encore une question qui se pose à celui qui veut écrire une histoire de la logique: l'option formaliste des logiciens mathématiciens veut que l'on traite la logique d'une façon purement symbolique, comme une syntaxe logique des signes depourvus de tout contenu. On espère ainsi obtenir une objectivité suprême, en bannissant tout idée, tout hypothèse philosophique. Ce serait une logique sans philosophie.

Mais est-il possible de faire de la logique sans aucun point de vue philosophique? Dès l'abord, cette intention est irréalisable, puisque même le point de vue formaliste implique une conception philosophique. La parole d'Aristote trouve ici une application très significative: "Celui qui philosophe philosophe, et celui qui ne philosophe pas philosophe [pour montrer pourquoi il ne faut pas philosopher], donc tous philosophent".

Mais en renonçant à la philosophie, et en se fixant seulement sur un domaine de calcul symbolique, on risque de faire une injustice profonde à des logiciens des plus grands. Une histoire de la logique qui accepterait ce point de vue, serait obliger d'effacer de la liste des logiciens les noms de Kant, de Hegel, de Wundt, de Husserl, de Goblot et de tant d'autres. Le lecteur d'un traité sur l'histoire de la logique, conçue de cette manière, se demanderait bien justement: comment est-ce possible que les auteurs de tant de traités de logique n'y soient pas mentionnés? Wilhelm Risse nous présente une bibliographie de la logique au temps de la Renaissance, qui comprend, seulement pour le XVI-ème siècle, plus de 2300 d'éditions de traités de logique (W. Risse: *Bibliographia Logica*, Hildesheim, 1965). Faut-il négliger tout ce matérial immense et tous ces livres de logique, par ce qu'ils ne traitent pas la logique d'une façon mathématique?

En dehors de l'injustice faite à ces penseurs, un travail, dirigé dans ce sens sélectif, ne serait pas une histoire objective mais un point de vue, concentré sur une époque particulière, à laquelle est accordé un crédit entier.

Voyons maintenant quelles sont les histoires générales de la logique qui sont à notre disposition à ce moment.

Nous avons en tout quatre histoires qui veulent être générales:

— C. Prantl: *Geschichte der Logik im Abendlande* (4 volumes, Leipzig, 1855-1870), mine inépuisable d'informations et de textes grecs et latins quelques-uns difficilement accessibles. Malheureusement elle va seulement jusqu'à la fin du XVI-ème siècle et est viciée par des appréciations inadmissibles.

— I. M. Bochenski: *Formale Logik*, une antologie de textes, en traduction allemande (Freiburg - München, 1956), en suivant l'ordre chronologique. Le but de l'auteur est de présenter tout les antécédents de la logique mathématique, depuis l'antiquité. La *Formale Logik* dédie un chapitre à la logique indienne.

— M. et W. Kneale: *Development of Logic* (Oxford, 1962) est conçue sur un autre plan que celle de Bochenski, mais, la conception des auteurs et la même: de suivre le développement de la logique dans sa ligne formelle qui a abouti à sa forme suprême qui est la logique mathématique.

— Anton Dumitriu: *Istoria Logicii* - "Histoire de la Logique" - en roumain (Bucarest, 1969), dans laquelle l'histoire de la logique apparaît comme la somme de tous ses moments, qui sont complémentaires et se complètent réciproquement.

L'auteur a fait l'effort de ne négliger aucun logicien, aucun traité ou même manuel digne à signaler.

En dehors de ces "Histoires" il y a encore d'autres travaux qui pourraient être mentionnés, sans toutefois constituer autres choses que des essais sur l'histoire de la logique, ou sur quelques problèmes spéciaux. Parmi ces travaux nous choisissons les suivants:

— Petrus Ramus: *Scholarum Dialecticarum* libri XX (XVI-ème siècle), première histoire de la logique, intéressante par quelques informations, mais naïve.

— B. Keckermann: *Praecognitorum Logicorum Tractatus III*, plutôt une énumération de titres et indications de matières, mais assez utile (Hanovra, 1598).

— J. F. Reimann: *Critisirender Geschichts -Calender von der Logika* (Frankfurt am Main, 1699), qui contient beaucoup de données bibliographiques.

— Gassendi: *De origine et varietate logicae* (Lugduni, 1658), de valeur incontestable.

— J. A. Fabricius: *Specimen elencticum historiae logicae*, une sorte de catalogue (Hambourg, 1699).

— J. G. Walchius: *Historia Logicae* (Leipzig, 1721), très court traité, mais avec informations très sérieuses.

— Fr. Aug. de Reifenberg: *Principes de la logique suivis de l'Histoire et de la Bibliographie de cette Science*, où l'information bibliographique est importante (Bruxelles, 1833).

— A. Frank: *Esquisse d'une histoire de la logique précédée d'une analyse étendue de l'Organum d'Aristote* (Paris, 1838).

— R. Blakey: *Historical Sketch of Logic* (Edinbourgh, 1851).

— Paul Janet et Gabriel Séailles: *Histoire de la Philosophie* (Paris, 1887), dans laquelle on trouve une exposition historique de la logique, assez ample, en poursuivant l'évolution des problèmes: concept, jugement, syllogisme, induction.

— Fr. Harms: *Die Philosophie in ihrer Geschichte* (Berlin, 1881). Le deuxième volume de ce travail a le titre *Geschichte der Logik*.

— R. Adamson: *A short History of Logic* (Edinbourgh, 1911).

— Th. Ziehen: *Lehrbuch der Logik auf positivistischer Grundlage mit Berücksichtigung der Geschichte der Logik* (Bonn, 1920).

— Oswald Külpe: *Vorlesungen über Logik* (Leipzig, 1923), qui commence avec une histoire de la logique, assez sommaire mais compétente.

— F. Enriques: *L'évolution de la logique* (Paris, 1926), parue d'abord en italien, contenant des idées intéressantes.

— H. Scholz: *Geschichte der Logik* (Berlin, 1931), un petit livre très érudit mais conçu sur la base que tout ce qui est valable dans le passé de la logique est ce qui préfigure la logique mathématique.

— T. Kotarbinski: *Leçons sur l'histoire de la Logique* (Paris, 1964), important ouvrage par les idées personnelles de l'auteur.

— A. O. Makaveleski: "Histoire de la Logique" (Moscou, 1967) travail résumatif (en russe).

Il y a, bien entendu, d'autres travaux historiques traitant des problèmes logiques particuliers, mais nous ne pouvons plus nous en occuper ici.

II. - LA LOGIQUE DANS LES CIVILISATIONS EXTRA EUROPÉENNES. — Nous ferons une courte incursion dans le problème de la structure mentale et des lois logiques qui régissent la pensée des peuples appartenant aux autres cultures qu'à celle d'Europe.

A. *La structure logique de la mentalité primitive.* — Nous possédons une littérature très riche sur les peuples dits primitifs, que les Allemands appellent plus correctement *Naturvölker.* Du point de vue qui nous intéresse ici, deux conceptions sont à citer: celle de L. Lévy - Bruhl, qui découvre dans le fonctionnement mental des non-civilisés des lois spécifiques; celle de Cl. Lévy Strauss, qui trouve des structures mentales différents chez les groupes humains.

Lévy - Bruhl est frappé par certaines affirmations des primitifs, qui visiblement ne respectent plus les lois logiques classiques. Par exemple, chaque membre de la tribu Bororó (Brésil), affirme qu'il est effectivement un perroquet Arara, mais il est en même temps lui-même, un exemplaire humain de cette tribu.

Lévy - Bruhl croit pouvoir expliquer de telles affirmations par la loi de participation (*Les fonctions mentales dans les sociétés inférieures*, Paris, 1910): "Dans les représentations collectives de la mentalité primitive, les objets, les êtres, les phénomènes, peuvent être, d'une façon incompréhensible pour nous, eux-mêmes et autre chose qu'eux-mêmes".

D'après la loi de participation, les choses et les phénomènes sont liés entre eux par leur participation à un élément commun invisible qui se réflète dans les réprésentations collectives.

Nous proposons une autre solution. On a rencontré dans le domaine microphysique des phénomènes bizarres: un même être élémentaire apparait effectivement dans les expériences tantôt comme onde, tantôt comme corpuscule! On a tenté de placer ces comportements de la particule élémentaire dans les cadres d'une logique polyvalente, comme l'ont fait Paulette Février et J. L. Destouches, etc.

On a construit, dans la logique mathématique, des logiques admettant plus de deux valeurs pour les propositions (le vrai et le faux). Une telle logique, convenablement construite, peut donner une structure logique polyvalente à un domaine de faits paradoxaux.

Il nous parraît possible de trouver un tel cadre polyvalent pour des affirmations comme celles des individus Bororó.

B. *La logique dans la Chine ancienne.* — La philosophie antique chinoise est divisée par A. Forke en 12 écoles (*Geschichte der alterchinesischen Philosophie,* Hamburg, 1922), dont les plus importantes sont le *Taoïsme,* le *Confucianisme,* le *Moisme,* l'école des légistes (l'école *Fa,* ou *Fa Kia,* où *Kia* = école) et l'école des dialecticiens et sophistes appelée *Ming Kia* (l'école de Ming). Les deux dernières "écoles" s'occupent spécialement des problèmes de logiques, mais on trouve de telles questions, et des exemples très intéressants de raisonnements, des sorites admirables, dans Confucius et d'autres penseurs.

Il y a trois choses que nous remarquons dans la pensée chinoise:

1. La manière paradoxale de penser qui lui donne un caractère incompréhensible pour la pensée européenne.

Comment comprendre en effet, les expressions (*Tao-te-King,* 63): "La non-activité agit; Ce qui est lourd pèse tant qu'il est léger"....

2. L'intérêt pour les sophismes, dont quelques-uns se trouvent aussi dans la philosophie présocratique: l'argument de la flèche, de la dichotomie des Eléates, etc.

3. Les Chinois n'ont pas achevé un Organon, dans le sens d'Aristote, mais ils ont énuméré la plupart des problèmes logiques - moins le problème du syllogisme, absence que Marcel Granet, dans sa *Pensée Chinoise* (Paris, 1934), explique par le fait que la logique des Chinois n'est pas une logique des genres et espèces, donc de l'extension, mais de l'ordre et de l'efficacité.

Les tournures bizarres, voire même incompréhensibles, de la pensée chinoise nous les expliquons par le fait que les penseurs chinois ont cherché surtout l'existence et pénétrant dans des zones de plus en plus inaccessibles à l'appareil intellectuel normal, ont exprimé les résultats de leurs investigations d'une manière inintelligible pour nous. Une logique polyvalente pourrait, peut-être, nous donner des schémas logiques de ces résultats paradoxaux.

C. *La logique indienne.* — Si l'on passe dans l'Inde on trouve une logique bien constituée.

En effet, parmi les six "écoles" philosophiques ortodoxes de l'Inde (appelées *darshanas* = points de vue), on trouve une dont le nom est *Nyaya*. Ce mot sanskrit signifie règle, méthode, norme pour le discours, et *Nyaya* est la logique indienne. Ainsi que l'on a conçu la logique dans l'antiquité et au Moyen-Age comme étant l'étude par laquelle on commence la philosophie, de même, aux Indes aussi, la logique a été considérée comme une introduction à l'étude des autres "systèmes" et a été pour cela appelée la "colonne vertébrale" de la philosophie.

La littérature logique est très vaste. On a une littérature *Nyaya* ancienne, exprimée *ne varietur* dans les aphorismes du texte *Nyaya* écrit probablement au III-ème siècle a. J. Ch., et de nombreux commentaires. Le texte de *Nyaya* est dû à Aksapada Gautamo. Ces principaux commentaires sont dûs à Vatsyayana, Uddytokara, Vacaspati Misra, Udayana, Sridhara, etc. La littérature moderne commence au XIII-ème siècle après J.Ch., avec le fondateur de l'école nouvelle de logique, Gangesa, école *Navya - Nyaya* (école néo-logique). Son traité principal est *Tattva - cinta - mani* ("La pierre magique des essences") et on a écrit des centaines de commentaires sur ce livre. L'auteur entreprend un examen particulier sur l'entière théorie de la connaissance, en apportant des corrections aux définitions courantes. Autres noms de cette école néo-logique sont: Vadindra, Raghunta Siromani, Maturanatha, Jagadira, Gadadhara etc.

Dès le XII-ème siècle apparaissent des manuels simplifiés, et après, des compendiums scolastiques, dont le plus connu et utilisé est le manuel d'Annambhatta, intitulé *Tarkasamgraha* ("Résumé des argumentations", XVII-ème siècle).

Il y a aussi une littérature logique énorme bouddhiste, avec son spécifique. La conception logique des Bouddhistes est formulée, avec un grand éclat par Dignaga (V-ème siècle après J.Ch.) et Dharmakirti (VII-ème siècle).

Quels sont les problèmes traités par la logique indienne? Ce sont:

a) *Les catégories*. On a les premières deux grandes catégories existence et non-existence (*bhava* et *abhava*). A côté de l'existence il y a encore 5 catégories (6 en tout): substance, qualité, action, généralité, spécifité, coexistence. Les six catégories ont des sous-divisions et la non-existence est aussi divisée en 4 sous-catégories.

Une fois énumerées les catégories on peut passer à la théorie de la connaissance valable (*prama*) et connaissance non-valable (*aprama*). La connaissance valable a quatre genres: 1) la perception; 2) la déduction par le syllogisme (*anumiti*); 3) l'induction par analogie; 4) la parole.

Disons quelques mots sur le syllogisme. La structure du syllogisme indien comprend cinq "membres" (*avayava*): 1) affirmation; 2) raison de l'affirmation; 3) exemple; 4) application (dans le cas donné); 5) conclusion.

Pour comprendre la différence entre le syllogisme aristotélicien et le syllogisme indien (à 5 membres), B. L. Atreya (*Elements of indian logic*, p. 83, Bombay, 1936) donne cet exemple:

Syllogisme aristotélicien:
Tous les hommes sont mortels;
Socrate est homme;
Donc Socrate est mortel.

Syllogisme indien (*Paranthanumana*):
Socrate est mortel;
Parce qu'il est homme;
Tous les hommes ont été mortels dans le passé, comme Thales, Zénon, etc.;
Socrate est un homme du même type;
Donc il est mortel.

La connaissance non-valable a conduit à l'étude des sophismes et de leur classification.

Le système moderne de logique étudie de plus près l'inférence, le témoignage, et la définition.

Pour conclure, nous disons que l'étude de la logique indienne est à son début. La plupart des textes logiques ne sont pas traduits et ceux bouddhistes n'existent plus que dans la langue thibétaine ou chinoise. Cela fait que cette discipline, qui comprend une littérature si riche, est presque inconnue dans son essence. Les traductions peu nombreuses, faites dans les langues modernes, sont généralement faites par des linguistes ou des non-spécialistes en logique, ce qui déforme le contenu de la logique indienne. Selon nous, une interprétation de la logique indienne doit tenir compte du fait que la logique indienne, ainsi que la logique des Grecs, est une logique de l'Universel, mais cet Universel dépasse celui des Grecs, puisque dans sa zone non-manifestée il est inaccessible à l'appareil intellectuel normal. La connaissance humaine tend à réaliser l'Universel, de devenir l'Universel; c'est pour cela que la logique n'est pas une collection de règles pour penser correctement, mais une doctrine de réalisation de la connaissance.

III. - LA LOGIQUE DES PHILOSOPHES PRÉSOCRATIQUES. — Si l'on examine maintenant les débuts de la pensée philosophique et scientifique en Europe, c'est-à-dire chez les Grecs, on constate que ce moment de l'histoire de la logique constitue la découverte de la pensée par elle-même.

La pensée commence à se définir dans sa propre nature seulement quand elle énoncera des principes abstraits, qui peuvent être pensés indépendemment de la réalité concrète. C'est justement ce qu'ont fait les premiers philosophes Grecs, connus sous le nom d'*Ioniens*: Thales, Anaximandre et Anaximène. Ces philosophes sont surtout des philosophes de la nature et ils découvrent dans la nature un monde de l'ordre, un cosmos - κόσμος, à la base duquel se trouve un principe, ou des principes. Pour Thales (625-550 a.J.Ch.), le principe unique, qui explique ce cosmos est l'eau; pour Anaximandre (610-540 a.J.Chr.), le principe de cosmos est l'infini - *apeiron*, dont il ne nous dit pas s'il est l'eau, l'air ou autre chose; pour Anaximène, il y a aussi un substratum physique unique - *mían ypokeiménon fysin*, déterminé et qui est l'air.

Pytagore (580-500 av.J.Chr.), et son école considérent que l'élément explicatif du monde est le nombre. Ils affirmaient, nous dit Aristote, que les principes des mathématiques sont les principes de l'Être entier. Diogène Laërce nous informe que Pytagore utilisait les définitions en mathématiques et Stobée nous communique que "Pytagore a étudié l'arithmétique en elle même, au delà des intérêts pratiques". Nous soulignons particulièrement, ce moment de le pensée grecque, qui conçoit des relations numériques abstraites, relations qui font intelligible ce monde. Tous les phénomènes appartiennent à un ordre qui n'exprime autre chose que les rapports entre les nombres et leurs propriétés.

Le Pytagoriciens concevaient aussi une série de dix oppositions, partant de l'idée que le nombre impair a quelque chose d'imparfait et illimité tandis que le nombre pair a un caractère parfait et limité ou déterminé. Voici ces dix oppositions, que Hegel caractérisait de "déterminations sèches, dépourvues de procès, non-dialectiques, immobiles": 1) limité-illimité; 2) impair-pair; 3) un-multiple; 4) droite-gauche; 5) masculin-féminin; 6) répos-mouvement; 7) droit-tors; 8) lumière-obscurité; 9) bien-mal; 10) carré-rectangle.

L'école éléate, fondée, d'après la tradition, par Xénophane de Colophon (565-475 av. J.Chr.), fait un pas en avant dans l'explicitation de la pensée.

Pour Xénophane, le principe unique est la même chose que le cosmos, ce qu'il exprimait par la formule: *En Kai pan*- L'un et Tout.

Le grand penseur de l'école est Parménide (540-450 av.J.Chr.). Les problèmes qu'il se pose sont d'ordre ontologique et concernent les conditions de l'existant. Il se demande quelle est la base commune de tout ce qui est pensé et constate qu'elle est l'existence: "car seul l'Être existe, éterne, immobile, indivisible". Mais ce qui n'existe pas ne peut être pensé, parce que la pensée doit avoir comme objet quelque chose qui existe. D'où cette différence fondamentale en comparaison de ses prédécesseurs, qui, d'une manière intuitive, pouvaient croire qu'une chose peut être impossible à penser mais pour cela elle ne cessait pas d'exister. Parménide montre la liaison indissoluble entre la pensée et l'existant: il n'existe que ce qui est pensable et seulement ce qui existe est pensable". Dans son fond dernier, la pensée est identique avec le fond ontologique de ce qui est pensé; dès lors "c'est la même chose penser et être" - *tó gàr autó nocin tè kai einai.*

Zénon de l'école d'Elée (V-ème siècle av.J.Chr.) forge ses célèbres arguments, qui n'ont cessé d'être discutés ni de notre temps. Ses arguments (*la dichotomie, la flèche, Achille et la tortue, les stades* etc) visaient à reduire à l'absurde la conception du mouvement, de la division à l'infini de l'espace, etc.

Dans ce but il utilisait la méthode de la réduction à l'absurde, c'est-à-dire de réduire la thèse contraire à une contradiction. Le principe de contradiction apparait donc avec eux comme le principe fondamental de l'être. C'est pour cela qu'Aristote disait que Zénon était le fondateur de la dialectique.

Avec Héraclite (540-470 av.J.Chr.), l'aspect du problème change: pour lui aussi le monde est un cosmos, mais son principe n'est pas l'être immobile, mais le mouvement dialectique même. Tout est le résultat d'une lutte - *polémos patèr pánton.*

Il voit un lógos - une raison - qui est humain mais auquel correspond le *lógos* objectif - la raison universelle, raison spécifiquement dialectique, en mouvement perpetuel.

Anaxagore (V-ème siècle av.J.Chr.) conçoit aussi le monde comme un cosmos, qui est organisé par un ordre rationnel, qui est dû à un élément différent du monde, la raison - l'intelligence, qui restera dans la philosophie grecque sous le nom de *Nous.*

Empédocle d'Agrigente (V-ème siècle av.J.Chr.) accepte quatre éléments primordiaux dont se développe le monde entier, quatre *stoihia*: l'eau, le feu, la terre et l'air. Par cela il réduit l'explication du monde à un nombre réduit de principes - d'éléments - comme d'ailleurs le fait la science moderne - indifféremment si leur nombre est plus grand.

L'atomisme de Léucippe et Démocrite (V-ème siècle av.J.Chr.) considère le mouvement comme réel et pour l'expliquer on a imaginé comme éléments primordiaux du monde les "atomes". Dans la recherche de la réalité, Démocrite trouve un critère et celui est la pensée. Intéressant à signaler est le fait qu'il désigne la pensée par le terme *énnoia* - concept, ce qui a déterminé Aristote a faire la remarque que Démocrite a pour la première fois défini le concept. Démocrite dit: "Le concept, le critérium de la recherche - *énnoia kritérion zetéseos*".

Si nous tirons une conclusion synthétique de ce que nous avons mentionné plus haut comme étant caractéristique pour la pensée présocratique, nous pouvons dire que dans cette période on précise quatre concepts fondamentaux pour l'entier développement futur de la philosophie grecque:

1) l'idée de l'Être, identique dans la nature à la pensée;
2) l'idée de cosmos, d'un monde ordonné par la raison;
3) l'idée de devenir;
4) l'idée de principe - *arhè.*

Indifféremment des variantes qui prendra l'idée de l'Être, il apparait comme une nécessité logique de la pensée de doter la réalité avec un substratum qui est commun à tout, dont la nature est identique à la pensée. Par cela se pose l'idée de *nous* et de *lógos*, le *nous* étant plutôt l'intellect inaffecté de détermination, ainsi que l'existence pure, tandis que le *lógos*, bien qu'il ait un caractère ontologique, a une première détermination, par exemple chez Héraclite, c'est le feu.

En résumé, la pensée présocratique découvre elle-même la pensée - qui même si elle n'arrive pas à se formuler par une auto-analyse, c'est-à-dire à expliciter ses propres lois - elle se reconnaît elle-même soit comme *nous* soit comme *lógos* - comme raison et fonction rationnelle ordonnatrice.

IV. - LES SOPHISTES. — Peu d'opinions ont eu une vie aussi longue que celle des anciens sur les Sophistes, opinion due à la critique sévère de Socrate. La perte totale des oeuvres des Sophistes a beaucoup contribué au maintien de cette célébrité mal famée de ces penseurs.

Il n'y avait pas d'"écoles" sophistiques: chaque penseur avait le droit de soutenir ce qui lui semblait bon: Protagoras d'Abdère soutenait que tout est vrai et que "l'homme est la mesure de toutes les choses"; Gorgias de Leontinoi affirmait qu'il n'y a pas de vérité, et il niait, comme nous l'affirme Sextus Empiricus, l'existence de toute règle de la connaissance.

Les arguments par lesquels ces philosophes voulaient démontrer le néant de la pensée paraissaient - même de leur temps - comme puérils.

Mais il faut leur rendre une certaine justice: par ces arguments, les sophistes ont soulevé des problèmes qui dépassent de beaucoup le problème de l'argumentation correcte. Au fond, ils ont posé le problème: comment la pensée est-elle possible? Ils posent dans toute sa gravité le problème de la pensée et de sa valeur, par eux la pensée commence à se penser elle-même, à examiner ses propres procès et les évaluer. Les résultats obtenus par ces philosophes grecs antérieurs étaient plutôt intuitifs. Les Sophistes sont les auteurs d'une critique de la connaissance, et si leur solution est innacceptable, en fin de compte, cela ne diminue pas en rien leur mérite d'avoir posé des problèmes purement logiques.

C'est pour cela que Ed. Zeller a vu dans la période des Sophistes une période d'"illuminisme" sans laquelle l'apparition de Socrate n'aurait pas été possible.

V. - SOCRATE ET PLATON — Socrate reprend le problème posé par les Sophistes et découvre, contrairement à ceux-là, que la pensée a des éléments stabiles. L'analyse des Sophistes les avait conduit à la conclusion que les idées ne sont que de simples noms et que la science n'est donc pas possible. Socrate montre, au contraire, que les notions générales ont une existence bien définie, qu'elles peuvent être construites d'une façon logique. Le problème fondamental qui a préoccupé Socrate était la formation et la nature des concepts généraux, qui sont les principes mêmes des choses. De ce point de vue, il ne dépasse pas le cercle de problèmes posés par les Sophistes, sa réponse est seulement différente. Socrate se convainc, comme il l'avoue lui-même (*Phédon*, 99,d), que la vérité ne peut être trouvée dans les choses mais dans les concepts qui la contiennent. Par conséquent il se propose non l'étude des choses - *pragmata*, mais l'étude des idées - *lógoi*. Une fois l'existence des idées étant reconnue, il pense que la première chose qu'un philosophe doit faire est de définir ses concepts. Avec cela il fonde une nouvelle science, inconnue jusqu'à lui, la science de la définition. Les sophistes prennaient les concepts tels qu'ils les trouvaient dans le langage commun, formes au hasard et sans être fixés par une définition, tandis que Socrate les reconstruit rationnellement et leur donne une existence logique bien déterminée, ce qui ne permet plus nul jeu sophistique.

Platon, disciple de Socrate, accorde un rôle prépondérant à l'esprit connaissant qui est la garantie de la connaissance. Il faut donc premièrement connaître l'esprit avant de connaître les bases même de la vérité. D'où la maxime de Socrate: connaît-toi toi-même - *gnothi seautón*. Platon désire donner un fondement métaphysique à cette conception. L'idée peut être isolée par la définition, parce qu'elle a une existence autonome, en elle-même, comme principe.

Il y a une monde des principes - le monde des idées - où les idées sont éternelles, immuables, principes de toutes les choses qui existent et principes de toute la connaissance. La science n'est que sortir du monde sensible, reflet pâle du monde des idées, et entrer dans le monde des idées intelligibles; cet acte de prise de contact avec les idées est un acte intellectuel intuitif et il est possible, car au fond c'est une prise de contact avec soi-même. Toute la réalité sensible n'est qu'une ombre de ce monde des idées, qui est la vraie réalité. Comment est-il possible de connaître le monde éternel?

Ce monde, réponde Platon, n'a jamais été étranger à l'homme, il a vecu autre fois dans ce monde, mais à cause de sa chute de ce monde céleste, à cause de son enchaînement au monde des ombres sensibles, il l'a oublié. Par conséquent, connaître n'est autre chose que se rappeler: c'est là la fameuse théorie de la réminiscence - ἀνάμνεσις.

Cette *anamnèse* signifie un "réveil" qui met de nouveau l'âme en possession de ce qu'elle a déjà connu jadis.

D'où il s'en suit que la connaissance des idées - ou des principes - est innée et existe virtuellement en chaque homme.

Entre ces idées il y a des relations et Platon appelle l'ensemble des opérations par lesquelles s'établissent les rélations entre les idées - *la dialectique*. Comme l'a bien observé Windelband, dans sa *Geschichte der Philosophie*, la dialectique de Platon n'a pas un caractère logique mais un caractère méthodologique. En d'autres termes, la dialectique platonicienne n'utilise pas les opérations dans les sens des découvertes logiques, mais elle constitue une méthode d'atteindre la vérité incorporée dans les idées.

Dans la *République* (chap. VII), nous apprenons quelles sont les étapes méthodologiques pour arriver à la contemplation des principes. Ces étapes sont: 1) la réfutation des théories fausses; 2) la comparaison des sensations, qui donnent lieu aux contradictions, car la même chose peut apparaître comme un et multiple, grande et petite, etc.; 3) cultiver les sciences qui permettent de faire un pas en avant vers le monde des idées mais à la condition que ces sciences soient cultivée non pour elles-mêmes, mais pour saisir les rapports intelligibles. Grâce à cette gymnastique intellectuelle, à l'exercise de la faculté rationnelle - νόησις, l'âme entre en contact avec ce qu'elle a autrefois connu, elle possède ce monde qu'elle est maintenant capable de contempler.

Pour arriver à cette contemplation il faut que l'idée soit définie d'une manière claire et distincte dans ses relations avec les autres idées. Cela peut être obtenu en suivant deux directions: l'une ascendante, qui mène à la formation des idées générales, l'autre descendante, qui nous permet de distinguer les concepts les uns des autres par division.

D'où les deux aspects de la dialectique:

1. *La dialectique ascendante*, qui forme les concepts - opération synthétique appelée *synagogé*.
2. *La dialectique descendante*, ou la division logique des concepts, appelée *diairesis*.

La dialectique ascendante poursuit à découvrir le général dans la multitude des individus.

Ces deux procès dialectiques sont capables, d'après la conception de Platon, de mettre le dialecticien en possession des essences des choses.

Ces essences sont l'universel. Il a eu une conception spécifique de l'universel, ainsi que les catégories, ces dernières étant *koinà perì pánton* (ce qui est) "commun entre tous". Ces genres maxima - μέγιστα γένη sont: l'existant, l'identique, le différent, le changement, la permanence. Si l'on ne peut parler d'une doctrine proprement dite des catégories chez Platon, il n'est pas moins vrai qu'il a eu une conception claire de ces genres "les plus grands".

En passant plus loin dans l'examen des textes, on peut dire qu'on trouve des traces du syllogisme dans les dialogues platoniciens.

Tout raisonnement - chez Platon est une division ou une synthèse, comme on l'a vu plus haut. Or Aristote lui-même (*Premiers Analytiques*, 1,31, 46 a) fait une critique à la division platonicienne et écrit: "Il est facile à voir que la division est une petite partie de la méthode que j'ai décrite. En effet, la division est, pour ainsi dire, un syllogisme faible - *asthenés syllogismós*; car elle postule ce qu'elle veut prouver et établit toujours quelque chose de supérieur, plus général que l'attribut en question".

Prantl aussi voit dans la division platonicienne "le germe" de la syllogistique d'Aristote (*Geschichte der Logik im Abendlande*, I, p.83).

D'autre part, les principes logiques ont été très bien connus par Platon.

1. Principe d'identité: la pensée doit être en accord avec elle-même (*Phédon*, 100, A).

2. Le principe de contradiction: des déterminations contradictoires ne peuvent convenir en même temps à une et même chose (*République*, IV, 436, B, *Sophiste*, 230, B).

3. Principe de la raison suffisante: telle connaissance a un caractère scientifique seulement si son fondement est connu (*Timée*, 28, A).

4. Les rapports des idées générales, comme on dirait aujourd'hui, inclusion, appartenance, réunion, équivalence, etc., étaient bien connus, mais il les a employés, pour ainsi dire, d'une façon implicite, sans les formuler explicitement, comme il résulte des dialogues *Protagoras* (332,C) ou *Théetète* (II,b).

Zeller explique très bien ce manque d'explicitation formelle de l'élément logique: "La logique est encore enveloppée dans la méthaphysique".

Pour conclure, nous dirons que la logique des idées, qui se développe dialectiquement par des définitions de plus en plus vastes, ne consiste que des procédés discursifs préparatoires en vue de déclancher l'acte noétique de prise de contact direct avec le monde des intelligibles. C'est cette conception qui explique le fait pourquoi Platon ne fait pas une théorie du raisonnement, parce que la vérité de la science platonique n'est pas une conclusion obtenue par raisonnement, mais le commencement et la fin de toute connaissance, car elle se justifie par elle-même, sans le support d'une démonstration.

VI. - LES DEMI-SOCRATIQUES — Parmi les disciples de Socrate, les uns sont restés fidèles à leur maître, comme Platon, Xénophon, (auteur des *Mémorables*), Eschine (l'orateur), les thébains Simmias et Kébes, etc.; d'autres ont abandonné la pensée de Socrate et se sont approchés de nouveau des Sophistes, formant des écoles, à savoir, l'école cyrénaïque, l'école mégarique, l'école érythréique et l'école cynique. Ces Néosophistes sont connus dans l'histoire de la philosophie sous le nom de *demi - socratiques*. Pour l'histoire de la logique, particulièrement intéressante est l'école de Mégare, fondée par Euclide (de Mégare). On lui attribue six dialogues. L'élève d'Euclide est Eubulide de Milet, auteur de plusieurs arguments célèbres: le menteur, le caché, Electre, le voilé, le sorite, les cornes, le chauve. D'autres Mégariens sont: Alexinos d'Elis, Euphantos d'Olynth, Apollonios et Diodor, tous les deux surnommés "Cronos", Cleinomachos de Thuriori, Stilpon de Mégare, etc.

La conception philosophique des Mégariens peut être résumée, en ligne générale, comme il suit: les idées ont une existence réelle, elles sont des essences immuables, accessibles à la pensée. Mais les idées sont absolument isolée les unes des autres, n'ont aucune communication et donc entre elles il n'y a aucun rapport. Bien qu'elles constituent la réalité dernière, n'ayant aucune liaison entre elles, il résulte que nul jugement n'est possible, donc ni le devenir, tout étant en repos. Le monde sensible devient une apparence. D'où leur préoccupation de démontrer par des arguments paradoxaux l'impossibilité de la pensée.

Parmi leur arguments nous en citerons deux.

Le menteur, qui a donné beaucoup à penser à tout les penseurs, dans l'antiquité, au Moyen - Age, et de nouveau de notre temps, aux logiciens mathématiciens.

Le dominateur, par lequel on cherchait à prouver que rien n'est possible.

Les Mégariens se sont occupés de plus du raisonnement hypothétique, dans le sens de la logique mathématique actuelle. I. M. Bochenski et W. et M. Kneale croient pouvoir affirmer que la logique des Stoïciens - dont nous parlerons plus loin - a ses sources dans la logique des Mégariens, bien entendu, si l'on considère seulement la ligne de développement du raisonnement hypothétique.

Nous disons un mot de l'école Cyrénaïque. Le fondateur est Antisthène d'Athène, qui est un nominaliste à outrance, et qui soutenait qu'il n'y a pas de concepts généraux. Chaque chose ne peut être représentée que par ce qui est propre à son individualité et de cette façon toute prédication, et par conséquent, tout jugement est impossible.

Les demi - socratiques n'ont rien de commun avec les conceptions de Socrate sur la pensée et la connaissance qu'ils détruisent.

Il y a quelques problèmes logiques posés par eux qui sont restés et qui ont fait l'objet de longs débats.

VII. - ARISTOTE — Le génie grec se réalise pleinement avec le maître de Stagire. Emile Boutroux dit que le génie grec touve en lui son expression universelle et parfaite. Malhereusement, deux difficultés sont présentes dans l'analyse du plus grand penseur de l'antiquité: la perte de certains de ses livres; le fait que lui-même - comme il résulte des plusieurs passages des auteurs anciens - donnait des leçons ésotériques (*acroamatiques*) "qui n'étaient accessibles qu'à ceux qui ont suivi ses cours" (Aulu Gelle, *Noctes Atticae*, XX,5).

1. *Les livres d'Aristote.* — Presqu'il n'y a pas de domaine dans lequel Aristote n'ait pas écrit. Les écrits sont ainsi groupés: 1) livres

de logique; 2) livres de philosophie naturelle; 3) livres de métaphysique; 4) livres relatifs aux sciences pratiques comme l'Etique, Politique et Poétique. Diogène Laërce lui attribue plus de 400 livres.

Les livres de logique ont été placés avant les autres (par Andronique de Rhodos) et ils sont au nombre de six:

Les Catégories, qui traitent des principaux classes de concepts.
De l'Interprétation (*Herméneutique*) traite de l'analyse du jugement.
Les Premiers Analytiques, qui contiennent la théorie du syllogisme.
Les Seconds Analytiques, étudient la théorie de la démonstration.
La Topique expose la dialectique, conçue comme l'art de la démonstration probable.
Des Réfutations Sophistiques exposent les règles d'après lesquelles ont peut réfuter les sophismes.

En dehors de ces livres qui contiennent le système de logique d'Aristote, on trouve des considérations sur la logique dans d'autres traités du Stagirite, dans la *Métaphysique* dans *De l'Ame*, etc.

2. *La place de la logique parmi les autres sciences.* — Aristote divise les sciences, ainsi que son maître Platon, dans trois groupes:

Sciences théoriques (dans le sens des sciences contemplatives);
Sciences pratiques (relatives à l'action);
Science poétiques (relative à la production).

On a donc le tableau suivant.

Sciences

1. THÉORIQUES	2. PRATIQUES	3. POÉTIQUES
métaphysique	éthique	musique
mathématiques	sciences économiques	poésie
physique	politique	architecture

Et ici apparaît la chose la plus surprenante dans la classification d'Aristote: *la logique n'a aucune place dans sa division des sciences.* Cette situation a frappé beaucoup d'historiens mais, selon nous, personne ne lui a donné la vraie signification.

Nous verrons plus loin que les logiciens scolastiques, qui ont gardé le trésor d'Aristote, ont connu plus de motifs que ceux que nous sont restés dans les textes des anciens. Mais nous possédons néanmoins de petits passages qui peuvent nous servir de base dans notre interprétation.

Dans sa *Métaphysique* (II, 3), Aristote nous dit: "Il est absurde de chercher en même temps la science et le mode de la science". Le même passage, avec une très petite différence, se trouve dans Alexandre d'Aphrodise (*In Aristotelis Metaphisica Commentaria, I*,24): "Il est absurde de chercher en même temps avec une science déterminée la forme d'existence de la science en général".

Mais nous avons un texte d'Aristote qui est décisif dans ce problème (*Seconds Analytiques, II*,19,100 a): ".... la démonstration n'est pas le principe de la démonstration, donc ni la science n'est pas le principe de la science". Dès lors, pour Aristote la logique n'est pas une science parmi les autres sciences. Étant une science de toutes les autres sciences - *Scientia scientiarum* dirons les Scolastiques - elle ne peut être classifiée parmi elles. La logique n'est pas un corps de propositions, les unes admises sans démonstration et les autres avec démonstration, mais un corps de propositions acceptées toutes avec le même titre: elle n'est pas une science déductive, mais une science de la déduction. En d'autres termes, si la structure de toute science est expliquée et justifiée par la logique, alors la logique elle - même devrait s'expliquer et se justifier elle seule, ce qui ne serait pas une justification et ce qui montre que la logique a bien une position tout à fait spéciale.

Nous dirons encore qu'Aulu Gelle nous dit que la doctrine du syllogisme faisait partie des leçons exotériques, mais les problèmes de logique sont cités par lui parmi les leçons *acroamatiques* (ésotériques).

Par conséquent, la logique n'est pas une science (en dehors peut-être de la théorie du syllogisme); elle est composée de connaissances purement contemplatives, c'est-à-dire obtenues par intuition intellectuelle.

3. - *La logique formelle* — Dans la conception du Stagirite la logique était formelle, et cette appelation est resté traditionnelle, mais elle a perdu son sens originaire. Toute science, dit Aristote, est la science de l'universel, mais tandis que les sciences particulaires partent des choses et établissent leurs relations générales, la logique part des concepts généraux qui dans leurs réalité ultime sont des universaux.

Mais qu'est-ce que l'universel? L'Universel est, selon Aristote, l'essence des choses, ce qui existe toujours et partout, ou encore, ce qui existe en plusieurs et est prédicable de plusieurs. Par cela le caractère formel de la logique est double: parce que son objet est constitué par les *formes intelligibles* des choses et donc non les choses; parce qu'elle fait abstraction de la *matière individualisée* dans laquelle s'incorpore l'universel.

Pour découvrir exactement la conception que se faisait Aristote de la logique il faut donc scruter de plus près la notion d'universel. L'universel - *tò Kathólou* - a, dans sa conception, plusieurs aspects:
— l'universel - forme = καθόλου - μορφή;
— l'universel - prédicat maximum = καθόλου - κατηγορία;
— l'universel - nécessité = καθόλου - ἀνάγκη;
— l'universel - généralité commune = καθόλου - κοινόν;
— l'universel - accident (erreur) = καθόλου - συμβεβηκός

Si l'Organon d'Aristote est la *logique de l'universel*, alors il doit étudier tous ces aspects de l'universel: la forme - essence, c'est-à-dire *l'in-formation* des idées générales; la prédication qui se fait dans le jugement et s'exprime dans la proposition; la nécessité, puisque ce qui est universel appartient par nécessité - d'où l'étude du syllogisme sous toutes ses formes; la généralité commune apparaît comme un universel dégénéré et donne lieu au raisonnement probable au dialectique; enfin, l'erreur de prendre l'accident pour essence fait l'objet d'une étude sur les sophismes.

Voilà donc la division de l'Organon justifiée par les modalités de l'universel.

Nous n'entrerons pas dans des détails des développements techniques des catégories, des jugements, des syllogismes, des raisonnements dialectiques, ou de la classification des sophismes. Nous dirons seulement quelques mots sur les catégories et sur le syllogisme.

4. - *Les Catégories* — Nous avons vu que même Platon avait discuté le problème des "genres maxima". Aristote, pour la première fois, fait une étude systématique des catégories, et il en découvre principalement dix: quantité, qualité, relation, lieu, temps, situation, possession, action, passion. Ces catégories sont les propriétés les plus générales des choses.

Mais leur sens proprement dit est de nature ontologique: elles expriment la manière la plus générale d'être des choses. Aristote le dit lui-même (*Métaphysique*, V,7): "L'existence a essentiellement autant de sens combien il y a des catégories, car les significations de l'existence sont aussi nombreuses que les catégories".

On voit donc par là combien complexe est le problème des catégories et qu'il ne peut être simplement réduit au problème de la prédication.

Mais pourquoi Aristote avait-il besoin des catégories? Le principe d'Aristote - il faut s'arrêter - devait être appliqué partout: dans l'ordre des vérités, où il découvre des principes, dans l'ordre cosmologique, où il découvre le premier moteur immobile, enfin dans l'ordre des concepts, où il faut avec nécessité avoir un commencement conceptuel, qui sont les catégories.

Malheureusement, la nature nécessairement logique de ce commencement conceptuel a été oublié par les logiciens modernes, qui ont renoncé aux catégories - aucun traité de logique moderne, soit de logique classique, soit de logique mathématique, ne traitant plus des catégories. Ce problème a été rélegué au domaine de la philosophie - considéré donc comme une question spéculative et non de nature purement logique. Et cela constitue, selon nous, une perte indiscutable et un appauvrissement du domaine logique.

5. - *La nature du syllogisme* — Nous avons deux *Analytiques* qui, tous les deux, traitent du raisonnement. On dit habituellement que les *Premiers Analytiques* ont comme sujet le syllogisme, et les *Seconds*

Analytiques ont comme but l'étude du raisonnement dans les sciences. La science de la démonstration est appelée, par Aristote, *Analytique* - elle est la *science analytique - analytiki epistémé.* Son nom vient du verbe grec ἀναλύειν (analyser), qui a le sens de "se retourner"; donc "l'analytique" est la science qui se retourne aux causes et aux conditions d'une chose à l'aide de la démonstration. Mais pourquoi deux analytiques? Nous allons clarifier cette question que les historiens ont généralement négligée. Voici ce qu'écrit Aristote (*Premiers Analytiques*, I,4) "*La démonstration est en effet une sorte de syllogisme, mais tout syllogisme n'est pas une démonstration*". Cette différence est soulignée par le Stagirite à plusieurs reprises, et voici comment il explique ce qui est une démonstration (*Seconds Analytiques*, I,2,71 b):

"J'appelle démonstration - ἀπόδειξις - un *syllogisme scientifique*, et j'appelle syllogisme scientifique celui qui, par le fait que nous le possédons, nous savons". Nous avons ainsi le syllogisme purement et simplement et le syllogisme scientifique, qui est la démonstration (l'autre n'est pas une démonstration).

Les conditions du syllogisme scientifique sont énumérées par Aristote: celui-ci a les premisses vraies, premières, immédiates, mieux connues que la conclusion, dont elles sont la cause. Un syllogisme peut exister, certes, sans ces conditions, continue Aristote, mais il ne sera pas une démonstration, car dans ce cas la science n'est pas réalisée.

En résumé, nous pouvons conclure: il y a deux *Analytiques* chez Aristote parce qu'il a distingué le schéma d'une démonstration (qui est le syllogisme) de ce qui est vraiment une démonstration (qui véhicule la vérité, et donc a un caractère ontologique).

On voit donc combien loin de la conception d'Aristote sont les logiciens mathématiciens actuels qui veulent constituer la science entière par des schémas démonstratifs!

6. - *La théorie de la science* — Dans la conception d'Aristote le syllogisme scientifique suit directement les démarches réelles de la réalité. La science entière n'est qu'une pénétration dans la réalité en lui explicitant le devenir logique. Quelle est donc la structure de la science? Il montre d'abord qu'il faut qu'il y ait des principes indémontrables, parce que tout démontrer c'est ne démontrer rien. Voici ce qu'écrit lui-même (*Seconds Analytiques*, I,2): "Il faut que les principes dont nous partont soient indémontrables; autrement, si nous ne possédons pas leur démonstration, ils ne pourraient être considérés comme connus, parce que connaître d'une manière non-accidentale les choses dont la démonstration est possible signifie posséder leur démonstration". Ce passage est très intéressant et montre deux choses. En effet, d'une part, il y a des vérités *indémontrables*, qui peuvent être directement connues. D'autre part, il y a des vérités démontrables dont la connaissance consiste dans leur démonstration. En d'autres termes, la con-

naissance rationnelle ou scientifique, ce qui est la même chose avec la vérité rationnelle, est la même chose que sa démonstration.

La vérité et sa démonstration sont identiques.

Cette conception apparaît clairement si nous tenons compte de la nature des idées générales, de leur caractère ontologique, qui appartient aussi à leur enchaînement par les moyens termes dans la démonstration syllogistique. Démontrer signifie ainsi lier les idées de la façon dont elles sont enchaînées en réalité, identifier la vérité dans son fond ontologique et dans son devenir, qui se fait selon le schéma syllogistique.

La structure d'une science, dans la conception d'Aristote est donc la suivante:

1. Les termes et les principes qui sont directement saisis par l'intuition intellectuelle.
2. Les termes définis et les théorèmes dérivés des ceux acceptés sans définition ou sans démonstration.
3. La méthode de démonstration qui est le syllogisme scientifique par excellence.

On voit donc quelle est la différence entre la théorie aristotélicienne de la science et celle de notre temps. Pour cette dernière l'acceptation des termes primitifs et des principes est une question de pure convention. La science moderne a renoncé a une faculté des plus importantes de l'intellect, que les penseurs de l'Antiquité et du Moyen - Age avaient acceptée comme une chose prouvée par leur propre expérience mentale. On a gagné une liberté énorme en ce qui concerne la construction des théories, mais, il faut le reconnaître, on a perdu la certitude que nos théories représentent réellement une connaissance de la réalité.

7. - *La logique perennis* — Presque chaque traité de logique nous offre, entre autres, une définition de la logique. Le logicien français Ed. Goblot commençait son célèbre *Traité de Logique* par cette phrase, qui explique pourquoi nous n'avons pas encore une définition unanimement acceptée de la logique: "C'est déjà une difficulté que de déterminer l'objet de la logique et de la définir. Est-ce un art ou une science? Une science *normative* ou une science spéculative? Son but est-il de prescrire des règles et des méthodes ou de faire simplement la théorie du raisonnement"?

De l'analyse sommaire faite plus haut, nous avons tiré la conclusion que la logique d'Aristote est la logique de l'universel. Et c'est ce caractère qui lui donne la possibilité d'être la logique de la nécessité, de la prédication, de l'essentiel, du formel, d'être une logique ontologique. C'est pour cela que la logique d'Aristote est la *logique perennis*, parce que son objet est tout objet possible de la logique.

Pour Aristote la connaissance de l'universel signifie une pénétration de l'intellect dans son état ontologique et le posséder en quelque sorte. C'est pour cela que pour lui, *connaître* signifie *être*. En d'autres termes, la connaissance effective de l'universel (qui ne se réduit pas à la connaissance des généralités abstraites) réalise dans le sujet connaissant l'universel, articule l'individuel à l'universel, et par cet acte ontologique, et seulement par cet acte, l'homme etablit sa place dans l'Être, le sens et le rôle de son existence singulière dans l'universel, qui seul peut lui donner effectivement l'explication et la signification de son inexplicable et insignificative existence individuelle.

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A. Dumitriu

TRANSLATION: J. E. HOLMSTROM - CHRISTCHURCH, HAMPSHIRE.

A HISTORY OF LOGIC

THE STAGES OF ITS DEVELOPMENT

First Part

In this paper, which is in two parts, the author sets out to describe the more important of the phases through which the science of logic has passed in the course of its development. Logic, as it appears to him, differs from other sciences in that it includes its own history. Logic *is the history of logic. Hence a study of the various forms assumed by this science in the course of its development is a matter of prime importance and an absolute necessity for the accurate understanding of its essence. Starting from this point of view the author proposes to consider first the logical structure of the primitive mind, then the logic of China and that of India, identifying the features specific to each. Following this, an analysis of pre-Socratic thought will lead up to natural appearances as viewed by Aristotle. The later periods will be examined in the second part, due to appear in the next issue of* Scientia.

I. - INTRODUCTION — The history of a science is always of interest as displaying the evolutionary process it has undergone and as explaining the stage it has reached at the present time. There are some sciences, however, that can be well known without any awareness of their history, this being by-passed as a possible subject for unconnected separate studies of what might be called an archaeological kind. Physics, for instance, can be perfectly well known without anything being known of its past and the same is true of astronomy, mechanics, chemistry, etc. But as regards logic the situation is quite otherwise. Its content is not limited to the latest established of the scientific laws falling within its scope; on the contrary it embraces everything done within that scope from ancient times up to the present. *Logic is the whole of its own becoming, the entire sum of all the moments of its history.* In this fashion it is a science exhibiting perfect unity, one which integrates in a critical manner all the problems that have ever been posed and all the solutions that have ever been reached a the any time in the past; for this very reason it is compatible with the general concept of a science continuously perfectible.

Whenever logic has been considered a dead science this has been because those who so considered it have been limiting it to a few handbook rules. Kant is a well known example. According to him, logic had made no single step forward since Aristotle, so it seemed to him "finished and done with". But the advent of Hegel and Husserl, followed in particular by mathematical logic, showed the impossibility of doing without Aristotle, the Stoicists and even the Scholastics. To give only one example, it can be said that the propositional calculus whereof the mathematical logicians are so proud was very well known to the Stoic and scholastic logicians. Apart from this there are other such problems, neglected by the logic of today, like for instance the scholastic problem of the *syncategoremata* or that of the *suppositio*. For the mediaeval logicians to be given their due we must await the time when these problems can be treated mathematically.

There is one further question that has to be decided by anyone proposing to write a history of logic: according to the formalist manner chosen by the mathematical logicians the treatment of logic should be purely symbolic, making it a logical syntax of signs devoid of any content. It is hoped thereby to achieve an overriding objectivity by the banishment of every idea or philosophical hypothesis. The result would be a logic without philosophy.

But is it in fact possible to make logic without having taken up any philosophical standpoint? Such an endeavour must be foredoomed from the start, for a philosophical conception is implicit even in the formalist point of view.

Here a saying of Aristotle applies very significantly: "Whoever philosophises philosophises, and whoever does not philosophise philosophises too [if only to show there is no point in philosophising]; hence everybody philosophises".

To eschew philosophy and concentrate exclusively on a field of symbolic calculus is to run the risk of being very unjust to the greatest logicians of the past. A history of logic viewing it in this way would entail deleting the names of Kant, Hegel, Wundt, Husserl, Goblot and many others from the list of logicians. The reader of such a history would very properly ask himself how it could be that so many authors of logical treatises were omitted from mention. Wilhelm Risse has given us a bibliography of logic at the time of the Renaissance in which more than 2300 publications of logical treatises are listed for the sixteenth century alone (W. Risse: *Bibliographia Logica*, Hildesheim, 1965). Ought this great mass of material and all these books on logic to be ignored because they do not treat logic mathematically?

Quite apart from the injustice done to these thinkers, no work carried out in this way could be an objective history; it could represent nothing more than a point of view concentrating on a particular period and awarding the whole of the credit to that.

Let us now take a look at what is available in the way of general histories of logic at the present time. There are four purporting to be such:

— C. Prantl: *Geschichte der Logik im Abendlande* (4 volumes, Leipzig, 1855-1870), an inexhaustible mine of information and of Greek and Latin texts, some of them difficult of access; but unfortunately not extending beyond the end of the sixteenth century and impaired by the inclusion of some unacceptable appreciations.

— J. M. Bochenski: *Formale Logik*, an anthology of texts translated into German (Freiburg - München, 1956), and arranged in chronological order with the object of showing all those since ancient times that are antecedent to mathematical logic, with one chapter devoted to Indian logic.

— M. and W. Kneale: *Development of Logic* (Oxford, 1962) arranged on a different plan from that of Bochenski but having the same purpose, namely to trace the development of logic along formal lines up to its culmination in mathematical logic.

— Anton Dumitriu: *Istoria Logicii* - "A history of logic" in Roumanian (Bucarest, 1969), in which the history is viewed as being the summation of all its moments of time, these all being complementary to one another reciprocally. The author has endeavoured not to omit any logician, any treatise or even any handbook worthy of mention.

Apart from these "histories" there are yet other works that might be cited although they are nothing more than attempts at the history of logic or disquisitions on a few special problems. The following may be mentioned by way of example:

— Petrus Ramus: (*Scholarum Dialecticarum* libri XX (16th century), the earliest history of logic, containing some information of interest; but ingenuous.

— B. Keckermann: *Praecognitorum Logicorum Tractatus III*, mainly a list of titles and subject annotations, but quite useful (Hanovra, 1598).

— J. F. Reimann: *Critisirender Geschichts-Calender von der Logika* (Frankfurt am Main, 1699), containing many bibliographical references.

— Gassendi: *De origine et varietate logicae* (Lugduni, 1568), of undoubted value.

— J. A. Fabricius: *Specimen elencticum historiae logicae*, a kind of catalogue (Hamburg, 1699).

— J. G. Walchius: *Historia Logicae* (Leipzig, 1721), a very brief treatise, but containing reliable information.

— Fr. Aug. de Reifenberg: *Principes de la logique suivis de l'Histoire et de la Bibliographie de cette Science*, giving extensive bibliographical information (Bruxelles, 1833).

— A. Frank: *Esquisse d'une histoire de la logique précédée d'une analyse étendue de l'Organum d'Aristote* (Paris, 1838).

— R. Blakey: *Historical Sketch of Logic* (Edinburgh, 1851).

— Paul Janet and Gabriel Séailles: *Histoire de la Philosophie*, (Paris, 1887), giving a fairly extensive historical survey of logic framed upon the development of problems relating to concepts, judgement, syllogism, induction.

— F. Harms: *Die Philosophie in ihrer Geschichte* (Berlin, 1881). The second volume of this work is entitled *Geschichte der Logik*.

— R. Adamson: *A short History of Logic* (Edinburgh, 1911).

— T. Ziehen: *Lehrbuch der Logik auf positivistischer Grundlage mit Berücksichtigung der Geschichte der Logik* (Bonn, 1920).

— Oswald Külpe: *Vorlesungen über Logik* (Leipzig, 1923), beginning with a rather short but competent history of logic.

— F. Enriques: *L'évolution de la logique* (Paris, 1926), first published in Italian, containing some interesting ideas.

— H. Scholz: *Geschichte der Logik* (Berlin, 1931), a very learned little book, but based on the belief that the only valid items in earlier logic are those anticipating mathematical logic.

— T. Kotarbinski: *Leçons sur l'histoire de la Logique* (Paris, 1964), an important work by reason of the author's own ideas.

— A. O. Makaveleski: - "History of logic" in Russian (Moscow, 1967), an epitome.

There are, of course, other historical works dealing with particular logical problems, but no more space can be given to them here.

II. - Logic in non-European civilisation. — Here brief references will be made to the problem of the mental structure and the logical laws that govern the thought of peoples whose cultures are other than European.

A. *The logical structure of primitive minds.* A very plentiful literature is available regarding peoples described as primitive, whom the Germans more correctly call *Naturvölker.* From the point of view that is of interest here, two conceptions of this may be cited: that of L. Lévy-Bruhl who finds that the mental functions of uncivilised peoples are subject to specific laws, and that of Lévy-Strauss who finds differences of mental structure among the various human groups.

Lévy-Bruhl was struck by certain kinds of affirmation made by primitives which clearly are inconsistent with the classical laws of logic. For instance, each member of the Bororó tribe in Brazil maintains that in point of fact he is the parrot Arara but that at the same time he is himself, as a human specimen of that tribe.

Lévy-Bruhl believes that such affirmations as these may be explicable by the law of participation ((*Les fonctions mentales dans les sociétés inférieures,* Paris, 1910) which states that "in the collective representations of primitive peoples it is possible, in some way which we do not understand, for objects, beings [*êtres*] and phenomena to be themselves as well something other than themselves". According to this law, things and phenomena are interconnected by their participation in an invisible common element which is reflected in collective representations.

Here another solution to this problem will be suggested. In the microphysical field, strange phenomena have been encountered, such as the fact that one and the same elementary entity appears in experiments sometimes to be a wave and sometimes a corpuscule! Attempts have been made, therefore, to find a place for these behaviours of the elementary particle in frameworks of a polyvalent logic, as conceived by Paulette Février, J. L. Destouches and others. Thus systems have been constructed in mathematical logic whereby propositions can be assigned either of two alternative values, the true and the false. By such means, suitably constructed, a polyvalent logical structure can be provided for explaining paradoxical facts. It would appear possible to discover such a polyvalent frame for explaining affirmations like those of the Bororó individuals.

B. *Logic in ancient China.* A. Forke (*Geschichte der alten chinesischen Philosophie,* Hamburg, 1922) has divided ancient Chinese philosophy into twelve schools, the most important of these being Taoism, Confucianism, Moism, the legal school (*Fa kia*) and that of the Ming dialecticians and sophists known as *Ming kia.* The last two of these "schools" (*kia*) are more especially concerned with logical problems, but similar questions with very interesting examples of reasoning and excellent writing are to be found in the works of Confucius and some other thinkers.

Three features of Chinese thought should be noticed:

1. The paradoxical cast of thought which makes some of it incomprehensible to the European thinker. For instance, what is to be understood from such expressions as "Non-activity acts; heavy objects weigh as much as they are light"... (*Tao-te-king*).

2. The interest in sophisms, some of which are also to be found in pre-Socratic philosophy, such as the arrow argument, the Eleatic dichotomy, etc.

3. Although the Chinese have never created an Organon in the Aristotelian sense they have in fact listed most of the logical problems except

that of syllogism. The absence of the latter was explained by Marcel Granet, in his, *Pensée Chinoise* (Paris, 1934), as due to the fact that Chinese logic is not a logic of genera and species and therefore of extension, but is a logic of order and effectiveness.

The odd and even incomprehensible twists of Chinese thought can be explained by the fact that the principal object of enquiry for Chinese thinkers has been existence, and that their delving into depths ever more inaccessible to the normal intellectual apparatus has caused them to express the results of their investigations in a way which is unintelligible to us. A polyvalent logic might, perhaps, equip us with logical schemata for these paradoxical results.

C. *Indian logic.* Going over to India we find a well constituted logic.

Among the six orthodox philosophical "schools" of India (known as *darshanas*, points of view) there is one called *Nyaya*, this being a Sanskrit word meaning rule, method, norm of discourse. *Nyaya* is the Indian logic. Just as among us in antiquity and in the Middle Ages logic was accepted as being the study wherewith to begin philosophy, so also in India logic is considered as an introduction to the study of other "systems" and for that reason it is called the "spinal column" of philosophy.

The literature of logic is very extensive. There is an ancient *Nyaya* literature expressed *ne varietur* in the aphorisms of the *Nyaya* text probably written in the third century B.C., and numerous commentaries. This *Nyaya* text is credited to Aksapada Gautamo and the principal commentaries to Vatsyayana, Uddytokara, Vacaspati Misra, Udayana, Sridhara, etc. The modern literature was begun in the 13th century A. D. Gangesa, by the founder of the *Navya-Nyaya* or neo-logical school. Its main treatise is *Tattva - cinta - mani* ("the magic stone of essence") and some hundreds of commentaries on this book have been written. Its author undertakes in it a detailed examination of the whole theory of knowledge and adds corrections of the definitions then current. Other names associated with this neo-logical school include Vadindra, Raghunta, Siromani, Maturanatha, Jagadira, Gadadhara, etc.

In the 12th century, simplified manuals made their appearance, followed later by scholastic compendia, the best known and most used of which is the handbook by Annambhatta entitled *Tarkasamgraha* ("Summary of the arguments", 17th century).

There is also an enormous Buddhist literature with specific features of its own. The logical concepts of the Buddhists were brilliantly formulated by Dignaga (5th century A.D.) and Dharmakirti (7th century).

The problems considered by Indian logic are the following:

(a) *Categories.* The first two major categories are existence and non-existence (*bhava* and *abhava*). Along with that of existence there are five more categories (making six in all), namely: substance, quality, action, generality, specificity, coexistence. The six categories have divisions and subdivisions, while non-existence too is divided into four sub-categories.

The categories, once they are enumerated, open the way to the theory of valid knowledge (*prama*) and non-valid knowledge (*aprama*), the first mentioned being of three genera according as it is gained from (1) perception, (2) deduction by way of syllogism (*anumiti*), (3) induction by analogy, or (4) words.

Here a little should be said about Indian syllogism. This has five "members" (*avayava*), namely: (1) affirmation, (2) reason for the affirmation, (3) example, (4) application (to the given case) and (5) conclusion. To show the difference between Aristotelian and Indian syllogisms, B. L. Atreya (*Elements of Indian logic*, p. 83, Bombay, 1936) gives the following example:

Aristotelian syllogism:
All men are mortal;
Socrates is a man;
Therefore Socrates is mortal

Indian syllogism (*Paranthanumana*):
Socrates is mortal;
Because he is a man;
All men have been mortal in the past, such as Thales, Zeno, etc.;
Socrates is a man of the same type;
Therefore he is mortal.

Non-valid knowledge has led on to the study of sophisms and their classification. The modern system of logic is being used to make deeper studies of inference, evidence and definition. In conclusion, be it observed that the study of Indian logic is only a beginning. The greater part of the logical texts have not yet been translated and the Buddhist ones no longer exist save in the Tibetan or Chinese languages, in consequence of which this branch of learning with its vast literature is essentially almost unknown. The translations into modern languages are few in number and have mostly been made by linguists or others unspecialised in logic, giving us a distorted picture of what Indian logic contains. As the present writer sees it, a proper interpretation of Indian logic must take account of the fact that, like the logic of the Greeks, this is a logic of the Universal; but its Universal is more extensive than that of the Greeks because the non-manifest part of it is not accessible to the normal intellectual apparatus. Human knowledge tends towards realising the Universal, to becoming the Universal. That is why logic is not just a collection of rules for correct thinking but is a doctrine for the realisation of knowledge.

III. THE LOGIC OF THE PRE-SOCRATIC PHILOSOPHERS. — If we now examine the beginnings of philosophical and scientific thought in Europe, that is to say among the Greeks, we find that what characterises this moment in the history of logic is the disclosure, by means of thought, of what constitutes thought.

Thought cannot begin to define its own nature until it has enunciated abstract principles capables of being thought independently of concrete reality. This was exactly the achievement of the earliest Greek philosophers: Thales, Anaximander and Anaximenes, collectively known as the *Ionians*. They were chiefly philosophers of nature and they discovered in nature an ordered world, a cosmos κόσμος, based upon a principle or principles. For Thales (625-550 B.C.) the unique principle explaining the cosmos was water. For Anaximander (610-540 B.C.) it was the infinite, the *apeiron*, but he does not tell us whether he equates this with water, air or something else. For Anaximenes there was also a unique and definite substratum, *mían ypokeiménon fysin*, this being air.

Pythagoras (580-500 B.C.) and his school considered the explanatory element for the world to be number. They held, according to Aristotle, that the principles of mathematics are the principles of all Being. Diogenes Laertius tells us that Pythagoras made use of definitions in mathematics and Stobaeus wrote that "Pythagoras has studied arithmetic as such, apart from its practical uses". Particular notice should be taken of this moment in Greek thought when abstract numerical relations were conceived as rendering our world intelligible.

The Pythagoreans also thought out a set of ten "oppositions" starting from the idea that there is something imperfect and unlimited about odd numbers whereas even numbers are characterised as perfect and limited, or determinate. The ten oppositions - which Hegel considered to be "insipid, pointless, undialectical and static" - are as follows: (1) limited-unlimited, (2) odd-even, (3) single-multiple, (4) right-left, (5) masculine-feminine, (6) rest-movement, (7) straight-crooked, (8) light-darkness, (9) good-evil, (10) square-rectangle.

The Eleatic school, founded according to tradition by Xenophan of Colophon (565-475 B.C.), made a step forward in the explanation of thought. For Xenophan the unique principle was the same thing as the cosmos, and he expressed this by the formula *en kai pan*, "the one and the all".

The great thinker belonging to this school was Parmenides (540-450 B.C.) who stated problems of an ontological kind, having to do with the conditions of the existent. He asked himself what was the common basis of everything thought about, and the answer he arrived at was "existence". Nothing, he said, exists but "Being: the eternal, the immobile, the indivisible". But, according to him what does not exist cannot be thought, since thought must have as its object something which exists. This is where he differs fundamentally from his predecessors who, intuitively, were able to believe that something impossible to think need not on that account be denied existing. Parmenides pointed to an indissoluble connection between thought and existent: "nothing exists save what is thinkable, and nothing is thinkable save what exists". In its deepest foundation, thinking is identical with the ontological foundation of whatever is thought about; "to think and to exist mean the same". *tò gàr autò noeĩn tè kaì eĩnai.*

It was Zeno of the Eleatic school (5th century B.C.) who forged these chains of reasoning which became famous, and have never ceased to be discussed even in our own day. His paradoxes dichotomy, the arrow, Achilles and the tortoise, the stadia, etc. were directed to reducing the concept of motion, the infinite division of space, etc. to absurdities. In so using the *reductio ad absurdum* method, which consists in reducing a contrary argument to an absurdity, he erected the principle of contradiction into a fundamental principle of existence. It was for this reason that Aristotle referred to Zeno as the founder of dialectic.

With Heraclitus (540-470 B.C.) the problem changes its aspect: for him, too, the world is a cosmos but the principle underlying it instead of being immobile existence is dialectic itself: everything is the outcome of conflict - *polémos patèr pánton*. He recognisesa *lógos* - a reason - which is indeed human, but which is matched by a corresponding objective *lógos* or universal reason, a specifically dialectic reason, a perpetual movement.

Anaxagoras (5th century B.C.), likewise, conceived the world as a cosmos, organised in accordance with a rational order due to an element which is apart from the world and which in Greek philosophy continued to be called *noũs* (translatable as reason or intelligence).

Empedocles of Agrigente (5th century B.C.) accepted the notion of the world as a whole having developed from four *stoihia* or primordial elements, these being water, fire, earth and air. In so referring the explanation of the world to a limited number of principles or elements he was anticipating modern science, although in the latter the number of elements is greater.

In the atomism of Leucippes and Democritus (5th century B.C.) motion is considered to be real and the primordial elements imagined for explaining the world are given the name of "atoms". In his quest for reality, Democritus arrived at a criterion, which is thought. It is interesting to notice that he designated thought by the term *énnois* which we render as "concept" and which led Aristotle to observe that Democritus had for the first time given a meaning to "concept" by having described "concept" as being the criterion of inquiry - *énnoia kritérion zetéseos.*

What has been said above in pointing out the characteristics of pre-Socratic thought can be summarised, in conclusion, by saying that during this period four concepts were defined which proved to be fundamental for the whole future development of Greek philosophy, namely:

(1) The idea of existence being of the same nature as thought;

(2) The idea of cosmos, meaning a world ordered by reason;

(3) The idea of becoming;

(4) The idea of principle - *arhè.*

Disregarding the variations which the idea of existence was to undergo, it was seen to be a logical necessity of thought that reality should possess a substratum common to everything that is of the same nature as thought. This substratum was discovered in the idea of *noüs* and *lógos* - the *noüs* having rather the sense of intellect unaffected by determination whereas the *lógos* (despite being ontological) has a primary determination - for instance, fire, according to Heraclitus.

To sum up, the achievement of thought before the time of Socrates was the discovery of thought itself, and although this did not extend to formulating the nature of thought by means of self-analysis - in the sense of defining its own laws - thought identified itself by the expressions *noüs* or *lógos* conveying the idea of reason and of the world being ordered rationally.

IV. - THE SOPHISTS. — Few opinions have had so long a life as that held among the ancients regarding the Sophists, an opinion due to the severe criticism of them by Socrates, but the total loss of their works has largely contributed to their abiding ill fame.

There were no "schools" of Sophists and each of these thinkers was free to maintain whatever arguments he thought fit. Thus Protagoras of Abdera argued that everything is true and "man is the measure of all things", whereas Gorgias of Leontini held that there is no such thing as truth and denied (according to Sextus Empiricus) the existence of any rules of knowledge.

The arguments whereby these philosophers sought to prove the non-existence of thought seemed puerile even at the time. Yet, in fairness, a certain credit is due to them, for these arguments of the Sophists did raise problems far wider than the problem of correct reasoning. Fundamentally, they posed the question of how thought is possible at all. They stated, in all its seriousness, the problem of thought and of its validity; they took the view that the beginning of thought consists in thinking about itself, in scrutinising and evaluating its own processes. The Sophists were the authors of a critique of knowledge; and even if on final balance their solution is unacceptable this in no way detracts from their merit of having stated problems which are purely logical.

It is on these grounds that the Sophist period has been described by E. Zeller as one of "illumination", without which the advent of Socrates would have been impossible.

V. - SOCRATES AND PLATO. — Socrates made a fresh attack upon the problem which had been stated by the Sophists and showed, contrary to them, that thought has elements which are stable. The Sophists' analysis had led them to conclude that ideas are nothing more than names, so that knowledge properly speaking is impossible. Socrates proved on the contrary that general ideas have a clearly defined existence and can be constructed in a logical manner. The fundamental problem that occupied Socrates was that of the formation and nature of the general concepts which are the underlying principles of everything we know. From this point of view he did not by-pass the problems raised by the Sophists but his answer to them was different. Socrates became convinced, as he himself expresses it (Phaedo 99, d), that truth is not to be found in things but in the concepts which condition them. He addressed himself, therefore, to the study not of things (*pragmata*) but of ideas (*lógoi*). He believed that once the existence of ideas had been recognised the first things a philosopher should do was to define his concepts. Thereby he founded a new science unknown until then, the science of definition. The sophists had taken their concepts as they found them in the common language, formed by chance and undefined, whereas Socrates reconstructed them rationally and gave them a determinate logical existence leaving no looseness for sophistical play.

Plato, a disciple of Socrates, cast the informed mind in a leading role as being the guarantor of knowledge. The mind needs to be understood before it is possible to understand the basis upon which truth rests; hence Socrates' maxim *gnŏthi seautón* - "know thyself". Plato undertook to give this conception a metaphysical foundation, seeing that ideas which exist autonomously, as principles in themselves, can be isolated by defining them.

There is in fact a world made up of principles - the ideal world - in which the ideas are eternal, immutable principles underlying all the objects that exist as well as all our knowledge. Science or knowledge is nothing else than emergence from the world of the senses into the world of intelligible ideas whereof the sensual world is a pale reflection. This act of making contact with the ideas is an intuitive intellectual one, possible because fundamentally it is an act of making contact with oneself. No sensible reality is more than a shadow of the true reality which exists in the world of ideas.

To the question how it is possible to know the eternal world, Plato answers that the eternal world has never been foreign to man. At one time he lived in it, but because of his fall from it and because he is now chained to the sensible world of shadows he has forgotten it. Hence "to know" is simply "to remember". This is the famous doctrine of recollection (or reminiscence) - *anamnesis* - which means reawakening, whereby the soul is again given possession of what it once knew.

It follows that knowledge of ideas, or of principles, is innate and virtually present in every human being. Relationships exist between these ideas, and the name given by Plato to the totality of the operations whereby such relations become established is *dialectics*. To cite a good observation by Windelband in his *Geschichte der Philosophie*, the dialectics of Plato is not of a logical but of a methodological kind; in other words it does not use operations in the sense of making logical discoveries but it constitutes a method of reaching the truth which is embodied in ideas.

We learn from the *Republic* (Chapter VII) what methodological stages need to be gone through in order to attain to the contemplation of principles, these being as follows: (1) refutation of wrong ideas; (2) comparison of sensations, which give rise to contradictions in so far as the same thing may appear either single or multiple, either large or small, etc.; (3) cultivation of the sciences which make it possible to advance step by step towards the ideal world - provided, however, that they are studied not just for their intrinsic interest but with the aim in view of discerning rational relationships. By virtue of this intellectual gymnastics and by the exercise of its rational faculty (νὸησις) the soul is enabled to establish contact with what it formerly knew, to assume possession of that (hitherto latent) world and to become able to contemplate it.

For an idea to be brought within reach of contemplation it needs to have been clearly and distinctly defined in its relations to other ideas. This can be achieved by proceeding in either of two directions: ascending, so as to be led to form general ideas, or descending so that we are enabled to distinguish concepts from one another by division. Hence dialectics has two aspects:

1. *Ascending dialectics*, forming concepts by a synthetic process called *synagogé*.

2. *Descending dialectics* or logical division of concepts, called *diaíresis*.

By means of ascending dialectics, generality is discovered covering a multiplicity of individuals.

According to Plato the dialectician can, by these two dialectical processes, be given possession of the essence of things. Such essences constitute the Universal. Plato had a specific conception of the Universal and of the categories, the latter being *koinà perì pánton*, which means "(whatever is) common to all". These most general of generalities - the *mégista géni* - are existence, identity, difference, change, permanence. Although it cannot be said that the categories of maximal generality constituted a "doctrine", properly so called, of Plato, it is none the less true that he clearly conceived them.

After examining the further related texts it can be said that traces of syllogism appear in the dialogues of Platonists.

According to Plato, all reasoning is performed either by division or by synthesis as explained above. Aristotle himself (in Prior Analytics, I, 46a) criticises Plato's divisions, commenting as follows: "It is easy to see that division is only a small part of the method I have described. In fact, division is what might be called a weak kind of syllogism - *asthenés syllogismós* - for what it does is to postulate that which is to be proved and then always establish something at a higher level, more general, than the attribute in question".

Prantl, too, saw in the Platonic division "the germ of Aristotle's syllogistics" (*Geschichte der Logik im Abendlande*, I, p. 83).

On the other hand, logical principles were very well understood by Plato:

1. The identity principle: thought must be self-consistent (Phaedo, 100,A);

2. The contradiction principle: contradictory conclusions cannot simul-

taneously be valid in respect of one and the same thing (*Republic*, IV, 436,B; *Sophist*, 230, B).

3. The sufficiency of reason principle: an item of knowledge cannot be characterised as scientific unless its basis is known (Timaeus, 28, A).

4. The interrelations between general ideas such as would nowadays be called inclusion, relevance, unity, equivalence, etc. were well known to Plato but he used them implicitly, so to speak, without formulating them explicitly, as may be gathered from the dialogues *Protagoras* (332, C) or Theaetetus (II, b). Zeller suggests a good explanation for this lack of formal explicitness in regard to the logical element: "Logic was still surrounded by metaphysics"

In conclusion it may be remarked that the dialectical development of a logic of ideas by means of ever widening definitions consists merely of using preparatory discursive processes directed to triggering off the cognitive which establishes direct contact with the intelligible world. This conception explains why Plato did not construct a theory of reasoning: it was because truth, as seen by him, is not a conclusion reached by reasoning but is both the beginning and the end of all knowledge, its own justification having no need for proof.

VI. - THE SEMI-SOCRATICS. — Among the disciples of Socrates there were some who remained faithful to their teacher, such as Plato, Xenophon (author of the Memorabilia) Eschine (the orator) and the Thebians such as Simmias, Kebes, etc., whereas others broke away from Socrates' thought and were attracted back towards that of the Sophists, forming the schools called Cyrenaic, Megarian, Eritrean and Cynical. In the history of philosophy these neo-Sophists are known under the designation "semi-Socratic". For the history of logic the Megaric school is especially interesting. This was founded by Euclid (of Megara) to whom six dialogues are ascribed. One of Euclid's pupils was Eubulides of Miletus who became the author of several famous paradoxes known respectively as those of the liar, the hidden, Electra, the veiled, Sorites, the horns, the bald man. Other Megarians were Alexinos of Elis, Euphantos of Olynth, Apollonios and Diodor both surnamed Cronos, Cleinomachus of Thuriori, Stilpon of Megara, etc.

The philosophical thought of the Megarians can be summarised in a general way as follows: ideas have a real existence, in essence they are immutable, they are accessible to thinking. Each idea, however, is completely isolated from the others, there is no communication and therefore no relationship between them. Because of this, and although they constitute ultimate reality, it follows that no judgement is possible on them or on "becoming" the whole being static. The sensible world is mere appearance. Hence the Megarians, preoccupation with proving, by paradoxical arguments, the impossibility of thought.

Among these arguments, two will be cited here:

— "The liar", which has provided much food for thought to thinkers in ancient times, in the Middle Ages, and again in our own day among mathematical logicians.

— "The master", whereby it was sought to prove that nothing at all is possible.

The Megarians were concerned chiefly with hypothetical reasoning in the same sense as modern mathematical logic. J. M. Bochenski as well as W. and

M. Kneale believe it possible that the logic of the Stoicists - to which reference will be made later - derived from that of the Megarians, though of course only as regards the line of development followed by hypothetical reasoning.

A word should be said on the Cyrenaic school. Its founder was Antisthene of Athens, an extreme nominalist who maintained that there are no general concepts. It is impossible to represent anything except by what appertains to its own individuality, whence it follows that all predication and all judgement is impossible.

The Semi-Socratics held nothing in common with the conceptions of Socrates regarding thought and knowledge, which they repudiated. Some of the logical problems they posed have survived and been debated at length.

VII. - ARISTOTLE. — The Greek genius came to full flower in the master mind from Stageira, in whom Émile Boutroux has said it reached its universal and perfect expression. Unfortunately, there are two circumstances that make it difficult to give an account of what is owed to the greatest thinker of antiquity: the loss of some of his books and the fact, made apparent by several passages in the works of ancient authors, that the lectures he himself delivered were esoteric (*acroamatic*) or, in other words, “not open except to those who had followed his courses”.

1. *The books of Aristotle*

There is scarcely any field in which Aristotle did not write. His writings, therefore, are grouped as follows: (1) books on logic; (2) books on natural philosophy; (3) books on metaphysics; (4) books relating to the “practical sciences” such as ethics, politics and poetry. Diogenes Laërtes credits him with more than 400 books.

The books on logic, six in number, have been listed by Andronicus of Rhodes before the other books, as follows:

The Categories, dealing with the main classes of concepts.

Of Interpretation (Hermeneutics), dealing with the analysis of judgements.

The Prior Analytics, containing the theory of syllogism.

The Posterior Analytics, studying the theory of proof.

The Topics, which explains dialectics as being the art of demonstrating the probable.

On Sophistical Refutations, setting out rules whereby sophisms can be refuted.

Apart from these books containing Aristotle's system of logic, considerations of logic are to be found in other treatises by the Stagirite, in his *Metaphysics*, in *Of the Soul*, etc.

2. *The place of logic among the other sciences*

Aristotle, like his teacher Plato, divided the sciences into three groups:
Theoretical sciences (in the sense of “contemplative” sciences)
Practical sciences (relating to action)
“Poetical” sciences (relating to “production”)

1. THEORETICAL	2. PRACTICAL	3. POETICAL
metaphysics	ethics	music
mathematics	economics	poetry
physics	politics	architecture

Here we encounter the most surprising feature of Aristotle's classification: namely that *logic has no place in his division of the sciences.* Though many historians have been struck by this, nobody seems to have pointed out its true significance.

It will be seen later that the scholastic logicians who kept the treasures of Aristotle knew about treatments of more subjects than have come down to us from antiquity. However, we are still in possession of a few short passages which can be used as a basis of interpretation.

Aristotle tells us in his *Metaphysics* (II, 3) that "it is absurd to inquire at the same time into science and the scientific method", and but for a very small difference the same passage is found in Alexander of Aphrodisia (In *Aristotelis Metaphisica Commentaria, I*, 24) who says: "it is absurd to inquire, with a particular science, into the form of science in general at the same time". But we also have a text of Aristotle which is decisive on this matter (Posterior Analytics, II, 19, 100a): "... a particular proof is not the principle of proof; hence knowledge[1] is not the principle of knowledge. Accordingly, logic was not in Aristotle's view a science among other sciences. Being the science *of* all the other sciences - *scientia scientiarum* as the scholastics later said - it could not be classed among those. Logic is not a body of propositions some of which are accepted without proof and others because proved: it is a body of propositions all of which have the same title to acceptance. It is not a deductive science but the science of deduction. In other words, if the structure of science as a whole is explained and justified by logic this implies that logic contains its own explanation and its own justification within itself - which cannot be a justification and which shows that logic does indeed occupy a very special position.

It can also be added that Aulus Gellius tells us the doctrine of syllogism formed part of the exoteric lessons, yet he cites problems in logic among the "acroamatic" (esoteric) lessons.

Logic, then, is not a science (except perhaps for the theory of syllogism) but is made up from departments of knowledge that are purely contemplative or, in other words, constitued by means of intellectual intuition.

3. *Formal logic.* Logic as seen by the Stagirite was formal and this description of it has traditionally been maintained although the word has lost its original meaning. All science, said Aristotle, is knowledge of the Universal; but, whereas the particular sciences start from objects and aim at establishing the general relations between these, logic starts from general concepts which in their ultimate reality are Universals.

But what is meant by Universal? According to Aristotle the Universal is the essence of all things. It is that which exists eternally and everywhere, or it is that which exists in many and can be predicated of many. Consequently the formal character of logic is double, for it deals with *intelligible* forms of things, not with the things themselves, and with abstractions of the *individualised* matter wherein the Universal is embodied.

Therefore, to reveal exactly how Aristotle conceived of logic his notion of the Universal needs to be scrutinised more closely. The Universal - *tò kathólou* - as he saw it had several aspects:

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[1] As the French word "science" tends to be used in a wider sense than the English it has been rendered by "knowledge" where this seems more appropriate. - *Translator.*

— The Universal as form = ϰαθόλου-μορὴ;
— The Universal as maximal predicate = ϰαθόλου-ϰατηγορία;
— The Universal as necessity = ϰαθόλου-ἀνάγϰη;
— The Universal as generality in common = ϰαθόλου-ϰοινόν;
— The Universal as accident or error = ϰαθόλου-συμβεβηϰός.

As Aristotle's Organon is the *logic of the Universal* he is obliged to study all these aspects of it, i.e. the *in-formation* of general ideas, the predication effected in judgement and expressed in proposition, the necessity because whatever is universal appertains necessarily (whence the study of syllogism in all its forms), the "generality in common" which makes its appearance as a degenerated Universal and gives rise to probabilistic reasoning or dialectics, and finally the error of mistaking the accident for the essence (investigated in a study of sophisms).

Such are the divisions of the Organon, resting upon the respective modes in which the Universal is manifest. While there is no need to enter into detail here on the technical developments that have taken place concerning categories, judgements, syllogisms, dialectical reasoning or the classification of sophisms, a few words may be usefully be said on categories and syllogisms.

4. *The categories.* The problem of maximal genera had already been discussed by Plato, as we have seen. Aristotle who was the first to make a systematic study of categories discovered ten main ones: quantity, quality, relation, place, time, situation, possession, action, sufferance. These categories are the most general of the properties that things can have, but more correctly their meaning is ontological: they express the most general of the modes in which things can exist. Aristotle himself put it (in *Metaphysics*, V, 7) that "Existence essentially has as many senses as there are categories, because the meanings of existence are as many as the categories". This shows the complexity of the problem of categories, and that it cannot simply be reduced to the problem of predication.

But *why* did Aristotle need categories? To answer this it must be remembered that Aristotle's principle - one "must stop,, - was to be applicable everywhere: in the order of truths, where he discovered principles; in the cosmological order, where he discovered the immovable prime mover; finally, in the order of concepts which must necessarily have a base to start from. The categories supply that base.

Unfortunately, modern logicians have forgotten that conceptualisation began on a basis which was necessarily a logical one and have altogether abandoned categories. No modern treatise on logic, whether classical or mathematical, any longer deals with them. The problem has been relegated to the philosophical field, being regarded as speculative and not of a purely logical character. In the present author's opinion, this is an indisputable loss and impoverishment of the logical field.

5. *The nature of syllogism.* We possess two Analytics both of which deal with reasoning. The subject of the first is commonly said to be syllogism and the aim of the second to be a study of reasoning in the sciences. Aristotle used the term *Analytic-* to denote the science of proof *analytiki epistémé.* This name comes from the Greek which carries the sense of "to go back to"; hence analytics is "to analyse", the name given to that which takes one back, by means of demonstration, to the causes and conditions of something.

But why are there two Analytics? Let us throw some light on this question, which the historians have most often ignored. In the *Prior Analytics*, I, 4 Aristotle wrote in that "proof is in fact a kind of syllogism, but not every syllogism is a proof". He emphasises this difference several times and he explains what is meant by a demonstration as follows (*Posterior Analytics*, I, 2, 71b):

"I apply the term 'proof' - ἀπόδειξις - to a *scientific syllogism*, here using the word 'scientific' to qualify a syllogism which we know because it is stated". Thus a distinction is drawn between syllogisms pure and simple on the one hand, scientific syllogisms on the other, the latter being demonstrations but the former not.

Aristotle stated conditions for a syllogism to be scientific as being that its premisses are true, antecedent, immediate and better known than the conclusion to which they lead. A syllogism can indeed exist without satisfying these conditions, Aristotle continues, but it will not then be a demonstration for in that case the knowledge [science] of it is not realised.

To summarise, it can be concluded that the reason why there are two Analytics of Aristotle is that he differentiates between the schemata of a demonstration (which is the syllogism) and what is properly a demonstration (a vehicle for the truth, having therefore an ontological character).

It is clear how far removed from Aristotle's conceptions are the mathematical logicians of today who set out to frame the whole of knowledge upon demonstrative schemata.

6. *The theory of knowledge.* According to Aristotle a syllogism which is scientific penetrates into reality by directly following approach roads which are real. Knowledge as a whole is formed by such penetrations whereby the logic of its formation is rendered explicit. What kind of structure, then, does knowledge have? First of all, Aristotle shows that there must be principles which cannot be proved, because to prove everything would be to prove nothing. In his own words this point is explained as follows: "The principles from which we start must be unprovable. Otherwise, if they were provable but if we did not have their proof they could not be considered as known, because to know provable things otherwise than accidentally means being in possession of their proof". This is a very interesting passage which states two things: namely, that on the one hand there are certain *unprovable* truths, which can be known directly, and on the other hand there are provable truths, knowledge of which consists in having proof of them. In other words, rational or scientific knowledge, which is the same thing as rational truth, is the same thing as its proof.

Truth and the proof thereof are identical. This conception becomes clear if regard is had to the ontological character of general ideas, which appertains also to their catenation by the middle terms in a syllogistic proof. To prove, therefore, means to connect ideas with one another in such a way that they are tied into reality; it means identifiying truth by reference to the ontological foundation of truth and the manner of its formation. This is achieved by way of syllogistic schemata.

The structure of a science as seen by Aristotle may, then, be depicted as having the following components:

1. Terms and principles which the intellect apprehends directly by intuition.
2. Defined terms, and theorems derived from these by definition or proof.

3. The method of proof which consists of scientific syllogism, in preference to any other.

This brings out the difference between the Aristotelian theory of knowledge and that of our own times according to which the acceptance of primary terms and of principles is purely a matter of convention. Modern science has relinquished one of the most important of intellectual powers, one which the thinkers of antiquity and the Middle Ages accepted as being proved by their own mental experiences. Thereby it has immensely expanded its freedom to theorise; but we ought also to recognise that thereby science has destroyed the assurence that our theories actually reflect our knowledge of reality.

7. *Perennial logic.* Nearly every treatise on logic puts forward, among other things, a definition of logic. The French logician E. Goblot began his famous *Traité de Logique* with the following sentence which goes to explain why we do not yet have a definition acceptable to all: "A first difficulty is to state the purpose of logic and to define it. Is it in fact an art or a science? Is it a *normative* science or a speculative one? Does it aim at laying down rules and methods or simply at constructing a theory of reasoning?".

From the brief analysis given above it was concluded that Aristotle's logic is the logic of the Universal, and that this characteristic enables it to be the logic of what is necessary, of what can be predicated, of what is formal and of what is ontological. For these reasons Aristotelian logic is the *perennial logic,* in that its subject matter covers every possible field of logic.

For Aristotle, knowledge of the Universal meant penetration of the Universal by the intellect in its ontological state so that, in a manner of speaking, the intellect become possessed of the Universal. For him, "to know" meant "to be". In other words, an effective knowledge of the Universal (not confined to a knowledge of abstract generalities) realises the Universal within the knower. It is by means of this ontological act, and only by this means, that man establishes his place in the scheme of Existence and may in fact aspire to explaining significance of his otherwise inexplicable and insignificant individual existence.

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RELATION ORGANE-FONCTION ET SYSTÈME NERVEUX CENTRAL

Les études morphologiques, physiologiques et éthologiques sur le cerveau concourent à montrer que, plus on s'élève dans l'échelle taxonomique, plus la liaison entre organe (aire encéphalique) et fonction se relâche. La finalisation du système nerveux central, dans ses zones supérieures, diminue au profit d'une « liberté » et d'une adaptabilité accrues. Deux positions excessives, le déterminisme rigide et l'indéterminisme extrême, sont à éviter. Organe et fonction se trouvent reliés, dans l'encéphale, non par des rapports constamment univoques mais par un spectre complexe de relations effectivement utilisées.

A - Le sens profond de la relation organe-fonction constitue un problème réel, issu d'une part d'observations de faits d'anatomie et de physiologie, présentant d'autre part des aspects épistémologiques et historiques. Il fera l'objet d'une étude conjointe plus générale et plus exhaustive que la présente note, étude dans laquelle les auteurs discuteront, en particulier, de la valeur d'une vue finaliste de la relation en cause[1]. Car c'est un fait qu'un certain finalisme, dont les degrés et les nuances ont varié selon les époques, a souvent sous-tendu les opinions exprimées sur ce sujet. Sans scruter cette situation de manière analytique, je crois pouvoir dire que cette intrusion de l'idée de finalité a souvent été directement proportionnelle à la non-ambiguïté de la relation organe-fonction ou, en d'autres termes, que la définition de l'organe était d'autant plus liée à une finalisation qu'elle était plus restrictive et plus précise. Par exemple, un rein ou un oeil ont toujours été très aisément « définis » non pas tellement par leur description anatomique (qui demeure inévitablement complexe), mais surtout par leur finalisation, c'est-à-dire que, dans un langage scientifique admis par beaucoup, ce qui révèle le mieux la « nature » de ces organes, c'est que leur fonction peut être indiquée facilement au moyen de l'expression « fait pour ». C'est pourquoi un langage finaliste, à propos de cette relation, a longtemps paru *naturel*, comme allant de soi, indépendamment de tout esprit de système philosophique explicite.

Mais beaucoup d'autres situations sont moins simples, de ce point de vue. Je voudrais le montrer ici dans quelques remarques (dont certaines implications sont méthodologiques au sens large) sur un cas particulier de relation organe-fonction, à savoir celui qui se présente dans le système nerveux central. Je crois pouvoir illustrer par cet exemple la thèse que l'adaptation morphologique d'un organe à une situation ainsi que la pertinence de sa réponse fonctionnelle peuvent être suffisamment variables pour rendre délicates l'interprétation et la simple description de la relation organe-fonction et exigent des nuances. Pour être plus précis encore, *l'expression* de cette interprétation ou de cette description m'apparaît pleine d'embûches.

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[1] BERNIER, R. et P. PIRLOT. *La relation organe-fonction: essai historico-critique.* En préparation.

B - Il n'est pas nécessaire de rappeler ici en détail l'anatomie de l'organe ou des organes dont il sera question: c'est le névraxe et presque exclusivement l'encéphale. On sait combien ce dernier est morphologiquement complexe, qu'il est constitué de structures elles-mêmes sous-structurées à l'extrême. Point n'est davantage besoin de discuter des nombreuses particularités fonctionnelles de chacune de ces structure. Il suffira de considérer une propriété fondamentale qui leur est commune: l'excitabilité ou, du moins, l'irritabilité dont n'importe quel traité élémentaire donne les caractéristiques[1]. Rappelons simplement que dans l'ontogenèse tout comme dans la phylogenèse approximativement représentée par les cinq classes de vertébrés, des régions évidemment homologues de l'encéphale, telles que le télencéphale, laissent apparaître des projections réceptrices variées tandis qu'une certaine régionalisation motrice s'y laisse aussi déceler.

Pourtant, même si l'on se limite aux mammifères, voire aux primates, chez qui le pallium a pris la forme du néocortex pluristratifié typique de cet ordre, la « destination » des aires et sous-aires homologues ne peut être décrite qu'avec prudence. En effet, les recherches contemporaines ont mis en évidence le *caractère diffus*, à des degrés variés, des réceptions encéphaliques. Et, parallèlement, les localisations des motricités ne sont pas dépourvues d'*équivoques*. Les « comptes-rendus »[2], tant morphologiques que physiologiques, de l'encéphale des diverses espèces n'apparaissent pas aussi rigoureux qu'on le souhaiterait. Par exemple, motrice dans son ensemble, la circonvolution précentrale offre cependant une mosaïque de fonctions que l'on peut croire variables d'une espèce à l'autre dans les conditions concrètes de l'existence de chacune. Et, en général, la numérotation, chez l'homme, d'une cinquantaine d'aires (de Brodmann) désignées approximativement plus que délimitées avec exactitude topographique par un rôle percepteur ou effecteur donné, ne doit pas faire illusion sur le caractère définitif de cette détermination fonctionnelle: nous allons y revenir.

Au moins deux répliques peuvent être opposées immédiatement à ces assertions. La première sera que, d'une part, certaines aires réceptrices corticales[3] sont « passablement bien » localisées: par exemple, celle de la réception du langage articulé chez l'homme; que, d'autre part, plus impressionnant encore, de ce point de vue, est un noyau oculo-moteur, constitué d'un nombre relativement très petit de cellules[1]. A cette remarque, on peut répondre: a) qu'en ce qui concerne les aires réceptrices corticales, chacune de celles-ci est reliée morphologiquement et physiologiquement à d'autres zones dont il serait fort hasardeux de prétendre donner une détermination précise et complète et nous en verrons plus loin de bonnes raisons; b) que les nucléi neuroniques moteurs les mieux circonscrits sont subcorticaux et ne représentent généralement pas non plus le centre *total* de contrôle de l'activité envisagée, qu'ils n'en sont qu'un élément et que leur intervention n'est qu'une partie d'un système fonctionnel vaste et complexe. C'est ce qui ressort de toute description classique de l'encéphale du mammifère.

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[1] Hermann, H. et J. F. Cier. 1968. *Précis de Physiologie.* Tome II, Fasc. I. *Système nerveux central.* Paris. Masson.

[2] C'est le moins mauvais mot français que j'ai trouvé pour exprimer le concept du terme anglais « account » qui me paraît le plus approprié.

[3] Je laisse de côté les relais subcorticaux (qui pourraient alimenter la même objection) puisqu'ils ne sont pas fonctionnellement terminaux chez les primates supérieurs, et probablement chez beaucoup d'autres vertébrés, dans un nombre considérable de cas. Il va de soi que la délimitation des noyaux auditifs du lemnisque latéral, par exemple, est justement précise.

Une seconde remarque suggérerait qu'on ne doit voir là qu'un simple problème technique dont le temps apportera la solution: un peu plus de raffinement dans l'expérimentation électro-physiologique, par exemple, conduirait à des délimitations plus nettes des « organes » (ceux-ci n'étant éventuellement, on doit le répéter, qu'un ou plusieurs groupes discrets de neurones). A ceci, on répondra aussi que les publications récentes suggèrent au contraire que plus on expérimente, plus réceptions et motricités se révèlent comme intéressant d'ordinaire des ensembles complexes, rarement des noyaux isolés: et c'est justement sur cette idée que prend appui notre réflexion sur la relation fonction-organe.

C - Abordons le problème, non point par la morphologie mais par la physiologie. L'aspect fondamental de l'activité nerveuse animale de base est représentée communément par le schème S - R (stimulus-réaction) ou encore par l'expression améliorée S - A - R (stimulus-association-réaction) qui correspond à l'arc réflexe au sens le plus large, dans le langage de la plupart des auteurs. Elargissons d'emblée la signification de ce schéma en suivant un usage adopté dans des manuels élémentaires et des ouvrages de vulgarisation[2] lesquels couplent, un peu indistinctement mais raisonnablement, fonction avec *comportement*: le lien entre les deux peut apparaître à première vue quelque peu vague, le comportement impliquant un ensemble hétérogène ou mal défini de fonctions, mais personne ne doutera de sa réalité lorsqu'on se place au point de vue de la totalité organique. C'est ainsi que le système nerveux *entier*, super-organe si l'on veut, présente, dans le panorama phylétique des animaux au moins cinq étages fonctionnels nettement tranchés, devenus classiques.

Considérons d'abord les trois niveaux inférieurs. Le premier est celui des *taxies* qui déclenchent chez les invertébrés et certains vertébrés inférieurs des comportements stéréotypés, pratiquement invariables, impliquant l'organisme total (phototaxies chez divers insectes, géotaxies chez les poissons). Ce sont des sortes d'orientations forcées dont la valeur adaptative est généralement claire. On remarquera facilement que, chez divers animaux (protozoaires, platyhelminthes, etc...), tout l'organisme, dans de tels cas, constitue en fait un organe entraîné dans un processus d'irritabilité globale et unique[1]. Viennent ensuite, un degré plus haut, les *réflexes non conditionnés*, parfois difficiles à distinguer des taxies, mais n'intéressant typiquement que des parties de l'organisme, un membre par exemple. Leur automatisme (ou leur nécessité) semble également accentué, voire extrême. Un exemple est celui du maintien du tonus musculaire postural. Il peut s'agir, en ce cas, d'un membre ou de la totalité du système locomoteur (squelette, muscle et réseau nerveux). Sur un troisième plan, les *instincts*, conçus généralement comme strictement spécifiques et rigoureusement adaptatifs, ont longtemps suggéré une finalisation merveilleuse: l'oiseau qui construit son nid dans des conditions optimales, qui y soigne et défend ses petits avec des astuces infinies; l'araignée qui tisse sa toile dans le meilleur site de capture puis se montre parfaite chirurgienne lorsqu'elle doit paralyser par puncturation un insecte pris au piège, etc... Les parts respectives des facteurs externes (plus ou moins variables) et des fonctions internes (plus ou moins fixes), dans le déclenchement de l'acte instinctif, sont discutées par les spécialistes. Migration, territorialité, lutte intraspécifique pour l'accouplement sont soumises, pour la plupart des auteurs, à ces deux catégories de facteurs. Mais où est l'organe de la fonction instinctive? On ne peut le désigner de façon simple: il est constitué par un complexe dont on n'est pas sûr de pouvoir énumérer tous les éléments. En quelque sorte, la notion d'organe s'atténue. En outre, les comportementistes usent souvent dans tout cela d'un langage finaliste: mais où situer cette finalité? La verrait-on dans ce que qu'ils décrivent sous la rubrique de *motivation* comme une *poussée* ou une *attraction* vers un *but* dont la possession annule la motivation même[2]? Ces termes inquiètent plus d'un biologiste: à tort ou à raison? Sont-ils biologiques ou métabiologiques? Un autre thème de discussion est la possibilité que l'instinct, totalement inné aux yeux des uns, puisse quand même être modulé, sinon modifié profondément, par un certain apprentissage. En tout cas, et c'est un point d'importance à mettre en lumière, dans toutes les situations indiquées ci-dessus, on a affaire à un système dont la base morphologique, c'est-à-dire le neurone sous un nombre très restreint de variantes structurales, est, en somme, relativement uniforme; mais, en contraste, les actions ou réactions de ce système sont suffisamment diverses pour présenter des significations adaptatives cruciales du point de vue de la sélection et de l'évolution. Et encore avons-nous jusqu'ici réduit très conventionnellement le problème à la considération de trois niveaux d'activité globale seulement: personne ne doutera que ce soit là une simplification presque excessive de la réalité.

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[1] Baron, G. 1969. *Etude comparative de quelques noyaux moteurs encéphaliques chez les Chiroptères néo-tropicaux.* I. Rev. Can. Biol. *28* (4): 241. - 1970. id. II. Ibid. *29* (2): 115. - 1970. id. III. Ibid. *29* (3): 233.

[2] Dethier, V. and E. Stellar. 1961. *Animal Behavior*. Prentice-Hall.

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[1] Il est bon d'insister en passant sur ce fait que des fonctions caractéristiques d'un système nerveux existent déjà chez des animaux dépourvus d'un tel système au sens courant, tels que les amibes: même un certain apprentissage aurait été réussi chez ces organismes.

[2] Ici, entre autres occasions, s'insère la question de savoir si cela signifie l'annulation de la fonction ou seulement du fonctionnement. Sur la validité de cette distinction, voir Bernier et Pirlot, o.c.

D - Les deux étages neuro-comportementaux supérieurs commencent avec l'apprentissage incontestable: on dépasse donc dès lors le groupe des activités innées, stéréotypées. On sait que l'apprentissage animal, à son plus bas niveau, est principalement représenté par les *réflexes conditionnés* ainsi que par les séquences d'essais-erreurs. Le conditionnement comme la correction représentent, à des degrés variés, des modifications dans la fonction d'un organe: celui-ci n'est plus, de fait, tout à fait rigoureusement lié à celle-là dans l'organisme relativement évolué, sinon l'on ne comprendrait plus l'existence de cette modifiabilité: il existe un certain indéterminisme qui empêche de définir l'un par l'autre de manière univoque exhaustive. Les glandes salivaires ne sont pas faites pour sécréter au son d'une cloche même si elles sont constituées de telle sorte qu'elles produisent leur salive devant un rôti fumant[1]. Les associations qui s'établissent entre des perceptions (signal sonore et présentation d'un aliment) déclenchent les fonctions (réponses) correctes d'une série d'organes coordonnés entre eux après une période plus ou moins longue d'entraînement. Dans ces processus, il y a place pour beaucoup de variabilité qualitative ou quantitative de la liaison stimulus-réaction et de la relation organe-fonction. Dans tous les cas envisagés jusqu'ici, il semble pourtant que l'organe soit essentiellement défini par sa fonction même: ne tiendrait on pas le même langage si l'on voulait dire qu'il est caractérisé fondamentalement par sa cause finale[2]? Il apparaît en outre, quand on réfléchit sur le réflexe conditionné, qu'un organe effecteur est d'autant mieux défini par sa fonction que celle-ci s'exerce entre des limites plus étroites, qu'elle est moins modulée ou moins variable.

Sur le plan humain, qui correspond au cinquième et plus haut étage de notre hiérarchie fonctionnelle nerveuse et comportementale, c'est par la voie du *raisonnement* utilisant des symboles ou des abstractions, avec des possibilités accrues d'associations, que s'opère le progrès de la connaissance et de l'action. Ce qui semble commun à ce palier et au précédent, c'est que les fonctions y sont essentiellement acquises, étant innées en puissance et dans l'attente de leur actuation, et que cette acquisition s'étale dans le temps. Et l'on ne peut éviter de se poser ensuite la question: les tâtonnements et erreurs que laissent voir les processus soit cognitifs soit effectifs n'impliquent-ils point, par rapport aux niveaux inférieurs, une *diminution* de la finalisation propre du système nerveux central en ce sens que le déterminisme, l'automatisme, la spécialisation quasi-infaillible des perceptions — réactions du schème S-R sont remplacés par une bonne mesure d'indétermination, par une généralisation ou une sorte de « liberté » des fonctions? A ce niveau, la seule réhabilitation de la finalité ne consisterait-elle pas à situer celle-ci dans une conscience choisissant librement ses objets de connaissance et ses voies d'action? D'aucuns n'admettront peut-être même la finalité que sous cette forme et le terme n'évoquera pour eux qu'un acte de volition éclairée. Mais nous quittons ici le terrain de la morpho-physiologie auquel nous devons nous confiner.

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[1] Quelques mots d'explication peuvent être utiles à propos de l'idée que le conditionnement modifie la fonction. Ce dernier terme ne désigne pas simplement ici la sécrétion de la glande qui, probablement, ne diffère pas dans le cas de la cloche et dans le cas du rôti, sauf par l'intensité peut-être, ce qui n'a en l'occurence pas d'intérêt. L'important est de noter que, dans la notion de fonction nerveuse, est impliqué le trajet aller-retour complet, incluant l'afférence aussi bien que l'efférence. Dans un cas, le cas normal, il se produit une afférence sensorielle par le nerf olfactif et une efférence motrice par des fibres composant les nerfs glossopharyngien et facial; dans l'autre cas, celui du conditionnement, l'afférence utilise le nerf auditif et l'efférence ne change sans doute pas de chemin. Mais l'association interne diffère d'un cas à l'autre et cette association est, comme l'afférence, une partie de la fonction globale.

[2] *Dé-finir*, c'est énoncer la *finis*, et le sens de ce terme latin est double: il signifie d'une part limite, frontière, contour, ce qui peut être objectivement descriptif, sans plus; d'autre part, fin ou but d'une activité, ce qui n'échappe que malaisément à une implication psychologique ou intentionnelle (cf. *Bernier* et *Pirlot*, o.c.).

J'ai parlé ci-dessus du système nerveux central en général, en pensant surtout à l'encéphale. Dans la perspective classique, les sous-structures de ce dernier peuvent être appelées des organes dans la mesure où l'on peut faire correspondre une physiologie définie à leur morphologie non moins définie. Mais ce qu'on a dit plus haut montre assez que cette notion d'organe appliquée au cerveau évolué ou à ses grandes subdivisions est bien floue si on la compare à celle que connote ce terme lorsqu'on parle d'un oeil ou d'un noyau moteur.

Divers autres exemples orientant vers la même conclusion peuvent être donnés. On connaît le cas des transferts de fonction après lésion d'une région encéphalique. Certaines compensations ou vicariances sont alors possibles, par lesquelles une aire donnée, jusqu'alors inactive d'un certain point de vue, devient active en se substituant à une autre aire paralysée[1]. Dans une perspective fort voisine, Goldstein écrit: « Le caractère des opérations qui les rapporte à la totalité s'exprime encore par l'indépendance que ces opérations possèdent vis-à-vis des régions déterminées avec lesquelles elles sont normalement en rapport » (o.c. p. 185). De plus, une espèce de disponibilité générale, libre du contexte pathologique du cas précédent, s'observe dans une masse centre-encéphalique importante, la formation réticulaire. Celle-ci se caractérise précisément par une polyvalence diffuse ou un indéterminisme fondamental qu'interrompent, de manière apparemment discontinue, des interventions de facilitation et d'éveil (et sans doute indirectement d'inhibition) opérées par ce réseau à l'égard d'autres fonctions encéphaliques.

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[1] Voir à ce sujet *Goldstein, K.* 1951. *La structure de l'organisme. Introduction à la biologie à partir de la pathologie humaine.* Paris, Gallimard, pp 181-186 sqq.

E - Dans tout ce qui précède, je n'ai certes pas prétendu « éclairer » toutes les ombres qui obscurcissent la relation organe-fonction. Au contraire, reconnaissant que cette relation demeure en grande partie mystérieuse dans le cas du système nerveux central tout spécialement, je n'ai voulu que délimiter ces zones obscures.

Les exemples qui sont de nature à accroître la perplexité à ce sujet sont nombreux. Si l'on se place dans une perspective phylogénique, on pourrait réfléchir, entre autres, sur la différence fondamentale qui sépare le système nerveux central de l'insecte de celui du vertébré supérieur et qui est bien plus qu'une différence de complexité structurale. Il n'est pas aisé de l'exprimer de manière satisfaisante. Un ganglion céphalique donné, chez un insecte, donne l'impression de posséder une fonction fixe, probablement exclusive de toute autre. Les mécanorécepteurs, les organes campaniformes proprioceptifs, les enregistreurs cordotonaux, les chémorécepteurs, les photorécepteurs composés sont tous très spécialisés, comme diverses structures sensorielles des animaux supérieurs. L'évolution dite progressive des centres cérébraux des insectes présentent, parmi les invertébrés, des traits bien distincts[1]:

1) spécialisation progressive continue du cerveau et apparition de noyaux spécifiques et de centres de relais;

2) centralisation du système nerveux par migration antérieure et coalescence de ganglions sur la chaîne nerveuse centrale;

3) allongement des nerfs périphériques pour compenser le déplacement des ganglions.

Cette évolution est illustrée, par exemple, par la comparaison des systèmes globaux du type *Chironomus* à *Empis* et à *Tabanus*. Les deux premiers points s'appliquent parfaitement à l'évolution jusqu'aux vertébrés supérieurs; le troisième est de moindre intérêt à ce niveau, le déplacement relatif de centres nerveux étant peu important (apparemment) dans l'ensemble du système post-céphalique. En ce qui concerne les associations, celles-ci existent au moins au niveau du procérébrum de l'insecte et il serait imprudent de croire qu'elles sont fonctionnellement moins efficaces, pour une vie d'insecte, que celles qui unissent les noyaux encéphaliques du mammifère pour l'équilibre et la survie de ce dernier.

On connaît d'autre part, et je ne vais pas en reprendre ici le détail, les *déplacements phylétiques* qui ont affecté, sinon totalement du moins de manière importante, les projections sensorielles spéciales (somatiques) chez les vertébrés tout au long de leur histoire. On en trouvera des résumés dans les manuels courants de morphologie[2]. Je rappelle brièvement les quelques faits pertinents suivants:

a) Pour l'olfaction (nerf I), des interventions diverses du bulbe et, éventuellement, d'un bulbe accessoire; un aboutissement variable au cortex antérieur, au striatum, au paléopallium, à un ensemble mal déterminé incluant le lobe piriforme et peut-être, selon certains auteurs, l'amygdale: le tout représentant des transferts peu clairs mais certains. b) Pour la vue, des déplacements quantitativement importants du toit optique au cortex occipital ou au striatum, avec interventions variables des corps genouillés. c) Pour le système stato-acoustique, des relais d'une complexité extrême mais suggérant un glissement historique et très variable selon les groupes, vers l'avant et vers le haut, par référence à la disposition linéaire de l'encéphale primitif. d) Pour la gustation, un déplacement des relais dans le rhombencéphale et des variations mal connues des projections antérieures. e) Pour la sensibilité somatique cutanée, des transferts non encore élucidés entre le mésencéphale et le télencéphale. Toutes ces observations, qui se complètent et se précisent dans des travaux récents dont la seule mention dépasserait le cadre de cet article, confirment ce qui a été dit plus haut.

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[1] Voir MEGLITSCH, P. A., 1961. *Invertebrate Zoology*. Oxford Univ. Press, p. 899, sqq.

[2] PIRLOT, P., 1969. *Morphologie évolutive des Chordés*. Presses de l'Université de Montréal, p. 796, sqq.

Enfin, il existe probablement un phénomène analogue de dissociation et de transfert entre organe et fonction dans les variantes phylétiques des nerfs crâniens et spinaux, surtout des premiers. On ne connaît pas le détail des événements évolutionnaires impliqués dans ce cas. On peut toutefois l'inférer partiellement d'après l'anatomie comparée des émergences crâno-spinales, des protochordés jusqu'à l'homme. Il est frappant: a) que les racines nerveuses dorsales aient « mis du temps » (évolutivement) à se spécialiser dans les afférences; b) que certaines d'entre elles (nerfs spinaux des vertébrés inférieurs ou certains nerfs crâniens, à savoir les facial, glossopharyngien et vague) soient encore mixtes; c) que des proportions diverses du nerf vague aient eu, au cours du temps, des affectations variables somatiques et viscérales d'une part, cérébro-spinales et autonomes d'autre part. On peut m'objecter qu'il ne s'agit, dans tous ces cas, que de réarrangements de *trajets* nerveux, que les fibres individuelles impliquées ont pu avoir et conserver depuis leur origine chez les vertébrés une seule et même fonction et qu'elles seules représentent des *organes* proprement dits. C'est discutable et l'anatomiste ne peut faire autrement que de considérer chaque racine nerveuse comme une unité morphologique discrète qu'il peut dès lors nommer organe. Et, sur la dimension phylétique, l'incertitude et la diversité des fonctions observées dans ces unités sont notables; c'est justement ce caractère incertain de la délimitation de l'organe et de l'attribution de la fonction qu'il s'agit de souligner ici.

F - Il faut donc se garder de jugements absolus quant à la relation organe-fonction dans le système nerveux central, comportement compris: ni un langage rigidement déterministe (soit mécaniste, soit finaliste), ni l'exaltation d'un indéterminisme source d'une sorte de spontanéité libre ne correspondent à une vision vraiment objective du réel. Chaque cas est à traiter séparément. Cependant, pour le biologiste préoccupé de déceler les directions phylétiques, il apparaît en fin de compte et paradoxalement que, depuis les formes les plus rudimentaires de l'organe nerveux majeur (encéphale) jusqu'à ses développements les plus complexes, si la délimitation d'aires (*organes*) de projections ou d'initiatives organiques demeure possible jusqu'à un certain point, par ailleurs la répartition spatiale des *fonctions* se fait de plus en plus floue à mesure que ces fonctions deviennent de plus en plus multiformes et flexibles. Chaque moment de chaque couplage organe-fonction apparaît de plus en plus unique ou incommensurables à tout autre, car les combinaisons qu'il inclut n'ont qu'une probabilité négligeable d'être exactement répétibles. Parallèlement, si les *adaptations* les plus contraignantes s'observent aux niveaux de vie les plus inférieurs, les *adaptabilités* les plus souples se rencontrent au plan de l'organe cérébral du primate, de l'homme surtout bien entendu[1]. De part et d'autre, la relation organe-fonction apparaît très différente. On notera, incidemment, que la flexibilité du rapport organe-fonction aux niveaux biologiques supérieurs s'accorde parfaitement avec l'existence de rétroactions (« feedbacks ») cybernétiques diversifiés.

Enfin, il est opportun de remarquer aussi que les résultats des travaux électrophysiologiques ou neuro-chimiques sur les synapses, publiés dans les ouvrages généraux du moins, n'élucident pas encore de manière satisfaisante l'extrême diversité des fonctions. Les transferts inter-neuroniques sont encore ramenés par les physiologistes à un petit nombre de mécanismes dont il appartiendra à une théorie des communications probabiliste et combinatoire (fondée sur le jeu des bits unitaires) d'explorer la vraie richesse. D'une manière générale d'ailleurs, et dans la perspective cybernétique encore, on peut, sans toutefois tomber dans l'enthousiasme excessif manifesté par certains, espérer tirer, des modèles physico-mathématiques des actions et réactions vivantes, des élucidations utiles des rapports entre organe et fonction.

Dans un travail extrêmement équilibré, Campbell et Hodos[2] ont présenté un plaidoyer très convaincant en faveur de la notion classique d'homologie opposée à une notion structuraliste superficielle. Il s'agit, pour eux, du concept de « same organ in different animals under every variety of form and function » proposé en 1843 par Owen et auquel des auteurs ultérieurs ont ajouté la précision de commune ancestralité. J'ai plaidé moi-même en faveur de cette idée[3]. De ce point de vue, les deux auteurs nommés insistent, d'une part, sur l'intérêt des connexions neuroniques entre sous-systèmes encéphaliques, d'autre part, sur le fait que, pour établir des homologies réelles, il faut recourir à un ensemble d'observations impliquant les liaisons par fibres nerveuses, la topologie et la topographie, les sulci, l'embryologie, la morphologie des neurones, l'histochimie, l'électrophysiologie et les modifications expérimentales du comportement. Tout cela indique une attitude de prudence reposant sur l'hypothèse d'une modifiabilité phylétique et actuelle des relations entre organe et fonction. Campbell et Hodos font une mise en garde, en particulier, contre des interprétations trop hâtives de similitudes histochimiques isolées de leur contexte. Or, pratiquement au même moment, paraissait un article de Parent et Olivier[1] qui considèrent la seule présence d'acétylcholine et de certaines monoamines dans les corps striés d'un reptile, d'un oiseau et de quatre mammifères comme une preuve suffisante de l'homologie entre ces structures. Le mot « phylogenetic » est employé par ces auteurs et l'implication est partout dans leur article clairement phylogénique. On doit se demander si cette implication ne serait pas rejetée par tous les tenants du concepts d'homologie à l'heure actuelle, à savoir les structuralistes d'un côté, les phylogénistes de l'autre. Il s'agit d'homologuer phylétiquement, sur une base uniquement histochimique, des parties ventrales du striatum sauropsidien avec des couches dorsales du striatum mammalien. Les auteurs ont employé le mot « homology » dans un sens qui n'est pas rigoureusement celui qu'on lui donne en biologie de l'évolution. Leurs observations, qui sont très intéressantes, suggèrent seulement une *analogie* fonctionnelle, ce qui n'est pas indépendant mais très distinct d'une homologie. En conclusion, il y a là encore l'indication qu'une distinction prudente doit être établie entre correspondance d'organes et correspondance de fonctions.

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[1] On trouve un parallèle de ceci dans la profonde divergence d'espèces animales à fonctions ultra-automatisées contrastant avec d'autres beaucoup plus flexibles et imprévisibles dans leurs réponses aux stimuli. Voir Stephan, H. et P. Pirlot. 1970. *Volumetric comparisons of brain-structure in bats.* Z.f. zool. Syst.u. Evolutionsf. Bd. *8* (H.3): 200.

[2] Campbell, C. B. G. and W. Hodos, 1970. *The Concept of Homology and the Evolution of the Nervous System.* Brain, Behavior and Evolution, 3 : 353.

[3] Pirlot, P. 1970. *Réflexions sur les concepts d'homologie et d'analogie.* Cah. Et. Biol. 18-19 : 31.

G - Il existe, d'autre part, dans l'organisation neurale, une similitude avec celle que sous-entend la théorie de la « Gestalt » en psychologie. Ce terme de « Gestalt » n'a guère pour avantage que son vague, et un langage quelque peu plus précis, quoique moins à la mode, lui substituerait, selon les cas, soit organisation ou intégration ou systématisation ou structuration sur le plan des processus, soit champ ou forme ou ensemble ou structure s'il s'agit des résultats des processus. On pourrait discuter longuement sur le choix à opérer ici parmi ce vocabulaire. On peut dire, en tout cas, que, comme les états mentaux, les systèmes neuraux encéphaliques sont organisés en interrelations complexes qui suggèrent que la notion de *champ* est probablement préférable à celle d'*aire* parce qu'elle est moins compromettante du point de vue topographique, implique plus clairement le fonctionnel et fait place à une connotation secondaire de gradient: tous ces éléments conceptuels nuancent utilement ce mélange d'indéterminisme, d'originalité, de polyvalence que les descriptions reconnaissent à diverses régions encéphaliques.

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[1] Parent, A. et A. Olivier. 1970. *Comparative histochemical study of the corpus straitum.* J. für Hirnf. 12 (1-2) : 73.

Par ailleurs, si, à côté de larges capacités de régulations, une adaptativité étendue et en relation directe avec la hiérarchie systématique se révèle dans le système nerveux central, je me sens assez en accord avec l'idée d'une logique des neurones (de type booléen, en réseau) qui s'ajuste à chaque situation. D'où aussi, me semble-t-il, doit dériver une attitude de prudence particulière devant la conception rigide des champs de perception, auxquels correspondraient, point pour point, des champs neuraux, mise en avant par une psychologie gestaltiste trop stricte. Comme le remarque Piaget[1], « le structuralisme gestaltiste pose les structures comme antérieures à tout fonctionnement et finit par anéantir celui-ci en l'absorbant dans des structures sans histoire ». Comme Piaget, mais d'un autre point de vue, je crois que les structures du système nerveux central ont une histoire ontogénique et phylogénique et qu'on ne peut éluder ce fait si l'on veut comprendre leur fonction. De cette histoire, les adaptations et les régulations constituent des événements marquants qui contribuent à l'incertitude de la relation organe-fonction dans la mesure même où adaptations et régulations apportent, chronologiquement, quelque chose de neuf par rapport à un état immédiatement antérieur.

Enfin, dans la même ligne de pensée, on remarquera qu'un autre concept d'origine relativement récente, celui de *chréode*[2], doit être pris aussi avec nuance, comme d'ailleurs Waddington le souligne lui-même (o.c., p. 64). La « nécessité » des canalisations dans le développement est tempérée partout de flexibilité, ce qui réconcilie déterminisme génique et effets ambiants plus ou moins imprévisibles. Il me semble que, dans le cas du système nerveux central, la part de la flexibilité demeure spécialement grande, même à l'état d'achèvement homéostatique du système.

H - La neurologie comparative nous a suggéré depuis longtemps, quoique trop vaguement, que la fonction générale d'intégration se perfectionne avec l'accroissement volumétrique et la complexification des masses ganglionnaires céphaliques et ceci se confirme dans la pathologie des accidents dé-intégrateurs (agnosie, aphasie, apraxie, alexie, agraphie). La capacité adaptative des organes augmente et diminue respectivement de la même manière. Il est au total impossible de parler de *la* relation organe-fonction *in abstracto* dans le cas du système nerveux central. Il existe *tout un spectre de relations* entre ces deux termes. Ce thème sera développé dans l'autre étude déjà annoncée[1]. Il y aurait aussi beaucoup à dire d'un autre type d'incertitude qui affecte très probablement le système nerveux central: il s'agit d'une certaine « surabondance » de neurones encéphaliques qu'on a depuis longtemps soupçonnée dans certaines lignées de vertébrés et dont des observations diverses confirment la probabilité[2]. Cette surabondance *provisoire* offre à l'évolution du système nerveux central des possibilités extraordinaires.

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[1] Piaget, J., 1967. *Biologie et connaissance.* Paris, Gallimard, p. 286.

[2] Waddington, C. H. 1962. *The nature of life.* New-York, Atheneum. Allen and Unwin, p. 64 sqq.

On notera, pour terminer, que ces considérations sur la variabilité et la flexibilité des relations entre organe nerveux et fonctions nerveuses, tout au long de l'échelle systématique animale, n'entrent pas en conflit avec l'idée émise ailleurs[3], que l'évolution a impliqué une « cybernétisation ». Il s'agit de modifications évolutives affectant les deux niveaux, cortical et subcortical, en concomitance. Un accroissement de quelques degrés de liberté d'un côté peut fort bien se manifester parallèlement à l'intensification de certains automatismes d'un autre côté. Cette question sera reprise dans un travail en préparation.

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P. Pirlot

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[1] Bernier, R. et P. Pirlot, o.c. Le thème et de nombreux points qui font l'objet du présent article ont apparu à la faveur des discussions plus générales dont le travail en collaboration mentionné est le résultat.

[2] Pirlot, P. 1970. *Relations pondérales entre l'encéphale et le corps chez les Chiroptères. II. Espèces paléo-tropicales et paléarctiques. Discussion et conclusion.* Bijd. Dierk. 40 (2): 103.

[3] Pirlot, P. 1969. o.c. p. 839.

TRANSLATION: J. LUBBOCK - ROME

ORGAN-FUNCTION RELATIONS IN THE CENTRAL NERVOUS SYSTEM

Morphological, physiological and ethological studies on the brain combine to show that, the higher one goes in the taxonomic scale, the greater freedom there is between organ (encephalic area) and function. The finalisation of the central nervous system, in its higher regions, diminishes to the advantage of increased « freedom » and adaptability. Two extreme attitudes — strict determinism and extreme indeterminism — are to be avoided. In the encephalon, organ and function are connected not by constantly univocal relationships but by a complex range of effectively utilised relationships.

A - The deeper meaning of the organ-function relationship constitutes a real problem, arising partly from observation of the facts of anatomy and physiology, but also presenting epistemological and historical aspects. It will form the subject of a more general and more exhaustive study than this present note, a study in which the authors will discuss, in particular, the value of a finalistic view of the relationship in question[1]. For it is a fact that the opinions expressed on this subject have often involved a certain finalism, the degrees and nuances of which have varied according to the different periods. Without going into an analysis of this situation, I think I can say that this intrusion of the idea of finality has often been directly proportional to the non-ambiguity of the organ-function relationship or, in other words, that the more restrictive and precise the definition of the organ, the more it was associated with a finalisation. For example, a kidney or an eye have always been very easily « defined » not so much by their anatomical description (which inevitably remains complex) but chiefly by their finalisation: that is to say, in a scientific language accepted by many, what best reveals the « nature » of these organs is that their function can easily be indicated by means of the expression « made for.... ». This is why a finalist language, with respect to this relationship, has for a long time seemed *natural*, as standing to reason, independently of any spirit of an explicit philosophical system.

But many other situations are less simple, from this point of view. I should like to show this here in certain observations (certain implications of which are methodological in the broad sense) on a particular case of organ-function relationship: namely, that which is found in the central nervous system. With this example, I think I can illustrate the thesis that the morphological adaptation of an organ to a situation, as well as the relevance of its functional response, may be sufficiently variable to make it tricky to interpret and even describe the organ-function relationship. To be still more precise, the *expression* of this interpretation or description seems to me to be full of snares.

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[1] BERNIER, R. and P. PIRLOT. - *La relation organe-fonction: essai historico-critique.* In preparation.

B - It is not necessary to recall here in detail the anatomy of the organ or organs to be discussed: it is the neuraxis and almost exclusively the encephalon. The morphological complexity of the latter is well known, consisting of structures that are themselves substructured to an extreme. Nor is there any further need to discuss the numerous functional details of each of these structures. It will suffice to consider a fundamental property that they have in common: their excitability or, at least, their irritability, the characteristics of which are given in any elementary text-book[1]. Let it merely be mentioned that in the ontogenesis, just as in the phylogenesis - approximately represented by the five classes of vertebrates - certain obviously homologous regions of the encephalon, such as the telencephalon, reveal various receptor projections, while a certain motor regionalisation is also apparent.

However, even if we confine ourselves to the mammals - and even to the primates, in which the pallium has taken the form of the pluristratified neocortex typical of this order - the « destination » of the homologous areas and sub-areas can only be described with circumspection. Actually, contemporary studies have revealed the *diffuse character* of the encephalic receptions, to varying degrees. In a parallel manner, there are also *ambiguities* attached to the localisations of the motricities. The morphological and physiological « accounts » of the encephalon of the various species do not seem as strict as one would wish. For example, though having on the whole a motor function, the precentral convolution nevertheless presents a mosaic of functions that may be considered as varying from one species to another in the concrete living conditions of each. And in general, the numbering, in man, of about fifty areas (Brodmann's areas) approximately designated rather than defined with topographical exactitude by a given perceptor or effector role, should create no illusion about the definitive character of this functional determination: we shall return to this later.

At least two objections to these assertions can immediately be made. The first will be that, on the one hand, certain cortical receptor areas[2] are « fairly well » localised: for example, that of the reception of articulate language in man; on the other hand, even more striking, from this point of view, is an oculo-motor nucleus, consisting of a relatively small number of cells[3]. To this remark, it may be replied that: a) as regards the cortical receptor areas, each of them is connected morphologically and physiologically to other areas of which it would be very risky to pretend to give a precise and complete definition, and we shall see good reasons for this later on; b) the best circumscribed motor neurone nuclei are subcortical and generally no longer represent the *total* centre of control of the activity envisaged; they are only an element of it and their intervention is only a part of a vast and complex functional system. This is what emerges from every classical description of the encephalon in mammals.

A second observation would suggest that what is involved here is merely a simple technical problem that will be solved in time: for example, a little more refinement in electro-physiological experimentation would lead to finer definitions of the « organs » (these, as must be repeated, being possibly nothing but one or more discrete groups of neurons). The answer to this will be that recent publications suggest, on the contrary, that the more experiments are carried out, the more receptions and motricities are revealed as ordinarily involving complex ensembles, rarely isolated nuclei: and it is this very idea that forms the basis of our reflection on the organ-function relationship.

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[1] HERMANN, H. and J. F. CIER. 1968. *Précis de Physiologie.* Vol. II, Part I. *Système nerveux central.* Paris. Masson.

[2] I leave out of account the subcortical relays (which might give rise to the same objection) since they are not functionally terminal in the higher primates, nor probably in many other vertebrates, in a considerable number of cases. It stands to reason that the delimitation of the auditive nuclei of the lateral lemniscus, for example, is rather precise.

[3] BARON, G. 1969. *Etude comparative de quelques noyaux moteurs encéphaliques chez les Chiroptères néo-tropicaux.* I. Rev. Can. Biol. *28* (4) : 241. - 1970. id. II. Ibid. *29* (2) : 115. - 1970. id. III. Ibid. *29* (3) : 233.

C - Let us approach the problem not through morphology but through physiology. The fundamental aspect of basic animal nervous activity is commonly represented by the scheme S - R (stimulus-reaction) or also by the improved expression S - A - R (stimulus-association-reaction), which corresponds to the reflex arc, in the broadest sense, in the language of most authors. Let us at once expand the significance of this scheme by following a usage adopted in elementary manuals and works of popularisation[1], which couple - somewhat unclearly but reasonably - function with *behaviour*: at first sight, the dividing line between the two may appear somewhat vague, since behaviour implies a heterogeneous or ill-defined set of functions, but no-one will doubt its reality if it is considered from the point of view of organic totality. Thus the *entire* nervous system - or super-organ, if one prefers - presents, in the phyletic panorama of animals, at least five sharply defined and distinct functional stages, which have become traditional.

Let us first consider the three lower levels. The first is that of the *taxes*, which, in the invertebrates and in certain lower vertebrates, actuate stereotyped and practically invariable behaviours, involving the total organism (phototaxes in various insects, geotaxes in the fish). They are kinds of forced orientation, the adaptive value of which is generally clear. It may at once be remarked that, among certain animals (protozoa, platyhelminthes, etc....), the whole organism, in these cases, in fact constitutes an organ involved in an overall single process of irritability[2]. Then come, one degree higher, the *non-conditioned reflexes*, sometimes difficult to distinguish from the taxes, but typically concerning only parts of the organism: one member, for example. Their automatism (or their necessity) seems equally accentuated, indeed extreme. One example is the maintaining of postural muscular tonus. In this case, either one member or the whole locomotor system (skeleton, muscle and nerve network) may be concerned. On a third plane, the *instincts*, generally conceived as strictly specific and adaptive, have for a long time suggested a wonderful finalism: the bird that builds its nest in the very best conditions for nurturing and defending its young with infinite astuteness; the spider that spins its web in the best place for making a capture and then reveals itself a perfect surgeon when it has to paralyse a trapped insect by stinging it, etc... The respective parts played by the exterior factors (more or less variable) and by the interior functions (more or less fixed), in the execution of the instinctive act, are debated by specialists. For most authors, migration, territoriality and intra-specific struggle for pairing are subject to these two categories of factors. But where is the organ of instinctive function? It cannot be designated simply: it consists of a complex of elements of which one cannot be sure of being able to enumerate them all. In some way, the notion of an organ becomes less distinct. Furthermore, the behaviourists, in all this, often make use of a finalist language: but where is this finality situated? Could it be seen in what they describe under the heading of *motivation* as a *thrust* or an *attraction* towards an *objective*, the attainment of which annuls the motivation itself[1]? These terms disturb more than one biologist: rightly or wrongly? Are they biological or metabiological? Another debated subject is the possibility that instinct, totally innate in the eyes of some, may nevertheless be modulated - or even profoundly modified - by a certain experimental learning. In any case - and this is an important point that should be emphasised - in all the above-mentioned situations one is dealing with a system having a morphological basis: that is to say, under a very restricted number of structural variants the neuron is, on the whole, relatively uniform; in contrast, however, the actions or reactions of this system are sufficiently different to reveal crucial adaptive significances from the point of view of selection and evolution. So far we have, very conventionally, reduced the problem to the consideration of three levels of overall activity only: no-one will doubt that this is almost too much of a simplification of the real state of affairs.

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[1] DETHIER, V. and E. STELLAR. 1961. *Animal Behavior*. Prentice-Hall.

[2] It is as well to insist, in passing, on this fact that the characteristic functions of a nervous system already exist in animals lacking this system in the ordinary sense, such as the amoeba: even a certain learning would have been achieved in these organisms.

D - The two higher neuro-behavioural stages begin unquestionably with experimental learning: here one goes beyond the group of innate stereotyped activities. It is known that, at its lowest level, animal experimental learning is mainly represented by the *conditioned reflexes* as well as by sequences of trials and errors. Conditioning and correction represent, in varying degrees, modifications in the function of an organ: in fact, an organ is no longer absolutely strictly tied to one function in the relatively evolved organism, otherwise one could no longer understand the existence of this modifiability; there exists a certain indeterminism that prevents one being defined by the other in an exhaustive univocal manner. The salivary glands are not made to secrete at the sound of a bell, even if they are of such a kind that they produce their saliva in the presence of steaming roast meat[2]. The associations that are established between the perceptions (sound signal and the presentation of food) set off the correct functions (responses) of a series of co-ordinated organs after a greater or lesser period of impulse. In these processes there is room for much qualitative or quantitative variability in the stimulus-reaction link and in the organ-function relationship. In all the cases so far envisaged, however, it seems that the organ is essentially defined by its own function: would it not be the same thing as saying that it is fundamentally characterised by its final cause[3]?

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[1] Here, among other things, there arises the question of knowing whether this means the annulment of the function or only of the functioning. On the validity of this distinction, see Bernier and Pirlot, op. cit.

[2] Some words of explanation may be useful with regard to the idea that conditioning modifies function. Here, this latter term does not merely designate the secretion of the gland, which probably does not differ in the case of the bell and in the case of the roast meat, except perhaps in intensity, which is of no concern in the circumstances. The important point is to note that, in the notion of nervous function, the complete out-and-back transmission, including afference as well as efference, is involved. In one case, the normal case, a sensory afference is produced by the olfactory nerve and a motor efference by the fibres constituting the glosso-pharyngeal and facial nerves; in the other case, that of conditioning, the afference makes use of the auditory nerve and the efference probably does not change its course. But the internal association differs from one case to another and, like afference, this association is part of the overall function.

[3] The verb « define » originally means « state the *finis* », and this Latin term has a two-fold meaning: on the one hand, it means limit, frontier, contour, which may be objectively descriptive, and no more; on the other hand, it can mean the end or aim of an activity, which barely avoids having a psychological or intentional implication. (cf. Bernier and Pirlot, op. cit.).

Furthermore, when one considers the conditioned reflex, it appears that an effector organ is defined better by its function when this function is exercised within the narrowest limits: when it is least modulated or least variable.

On the human plane, which corresponds to the fifth and highest stage of our nervous and behavioural functional hierarchy, it is by way of *reasoning* using symbols or abstractions, with increased possibilities of associations, that the progress of knowledge and action operates. What seems common both to this stage and to the previous one is that the functions are essentially acquired, being innate in influence and in the anticipation of their actuation, and that this acquisition reveals itself in time. The question then inevitably arises: regarding the errors and tentative procedures that reveal both cognitive and effective processes, do not they imply, through relationship with the lower levels, a *diminution* in the finalisation proper to the central nervous system in the sense that determinism, automatism and the quasi-infallible specialisation of the perceptions — reactions of the S-R scheme are replaced by a certain measure of indetermination, by a generalisation or a sort of « freedom » of the functions? At this level, would not the only way to restore this finality be to locate it in a consciousness that freely chooses its objects of knowledge and its ways of action? Perhaps some students will admit finality only in this form, and the term will evoke for them only an act of enlightened volition. But here we leave the field of morpho-physiology to which we must confine ourselves.

I have spoken above about the central nervous system in general, considering chiefly the encephalon. In the classical perspective, the substructures of the latter may be called organs to the extent to which a definite physiology can be made to correspond to their no less definite morphology. But what has been said above shows well enough that this notion of organ applied to the evolved brain or to its larger subdivisions is very hazy if it is compared with what this term connotes when one speaks of an eye or a motor nucleus.

Various other examples may be given, leading to the same conclusion. One knows the case of transfers of function after injury to an encephalic region. Certain compensations are then possible, through which a given area, until then inactive from a certain point of view, becomes active in replacing another paralysed area[1]. Expressing a very similar point of view, Goldstein writes: « The character of the operations that relates them to the totality is again expressed by the independence possessed by these operations with respect to particular regions with which they are normally in association » (op. cit. p.185). Moreover, a kind of general availability, free from the pathological context of the previous case, is observed in an important encephalic-centre mass: the reticular formation. This is precisely characterised by a diffuse polyvalence or a fundamental indeterminism that are interrupted, in an apparently discontinuous manner, by phenomena of facilitation and awakening (and undoubtedly, indirectly of inhibition) operated by this network upon other encephalic functions.

E - In what I have said above, I have certainly not pretended to « clarify » all the doubts that obscure the organ-function relationship. On the contrary, in recognising that this relationship remains largely a mystery especially in the case of the central nervous system, I have merely attempted to outline these doubtful aspects.

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[1] On this subject, see GOLDSTEIN, K. 1951. *La structure de l'organisme. Introduction à la biologie à partir de la pathologie humaine.* Paris, Gallimard, pp. 181-186 seq.

There are numerous examples of such a kind as to increase the perplexity that attaches to this subject. If it is viewed from a phylogenetic point of view, it would be possible to ponder, among other things, upon the fundamental difference that separates the central nervous system of an insect from that of a higher vertebrate, and which is much more than a difference in structural complexity. It is not easy to express it in a satisfactory manner. A given cephalic ganglion, in an insect, gives the impression of possessing a fixed function, probably exclusive of any other. The mechanoreceptors, the proprioceptive campaniform organs, the cordo-tonal recorders, the chemo-receptors and the compound photo-receptors are all highly specialised, like various sensorial structures of the higher animals. Among the invertebrates, the so-called progressive evolution of the cerebral centres of the insects shows very distinct characteristics[1]:

1) continuous progressive specialisation of the brain and the appearance of specific nuclei and relay centres;
2) centralisation of the nervous system by anterior migration and coalescence of ganglia on the central nerve chain;
3) elongation of the peripheral nerves to compensate for the displacement of the ganglia.

This evolution is illustrated, for example, by a comparison of the overall systems from *Empis* to *Tabanus* and to *Chironomus* types. The first two points apply perfectly to evolution up to the higher vertebrates; the third is of least interest at this level, the relative displacement of the nerve centres being (apparently) unimportant in the ensemble of the post-cephalic system. As regards the associations, these exist at least at the level of the procerebrum of the insect and it would be unwise to think that they are functionally less effective, for the life of an insect, than those that unite the encephalic nuclei of a mammal for the balance and survival of the latter.

On the other hand, one knows - and I do not intend to repeat them here in detail - the *phyletic displacements* that have affected, if not totally at least to an important extent, the special (somatic) sensorial projections in the vertebrates all along their history. Summaries of them will be found in all textbook in morphology[2]. I briefly mention the following pertinent facts: a) For olfaction (nerve I), various interventions of the bulb and, possibly, of an accessory bulb; variable endings in the anterior cortex, in the striatum, in the paleopallium, in an ill-defined area including the piriform lobe and perhaps, according to certain authors, the amygdala: the whole representing unclear but definite transfers. b) For sight, quantitatively important displacements from the optic tectum to the occipital cortex or to the striatum, with variable interventions of the geniculate bodies. c) For the stato-acoustic system, relays of an extreme complexity but suggesting a historical forward and upward shift - very variable according to the different groups - with reference to the linear disposition of the primitive encephalon). d) For gustation, a displacement of the relays in the rhombencephalon and little understood variations in the anterior projections. e) For the cutaneous somatic sensitivity, transfers, so far unexplained, between the mesencephalon and the telencephalon. All these observations, which complete one another and have become more accurate in recent works - the mere mention of which would go beyond the scope of this article - confirm what has been said above.

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[1] See MEGLITSCH, P. A., 1961. *Invertebrate Zoology*. Oxford Univ. Press, p. 899 seq.

[2] PIRLOT, P., 1969. *Morphologie évolutive des Chordés*. Presses de l'Université de Montréal, 796 seq.

Finally, there probably exists a similar phenomenon of dissociation and transfer between organ and function in the phyletic variants of the cranial and spinal nerves, especially the former. The evolutionary events involved in this case are not known in detail. However, they may be partially inferred from the comparative anatomy of the cranio-spinal emergences, from the protochordates up to man. It is striking: a) that the dorsal nerve roots have « taken time » (evolutively) to become specialised in the afferences; b) that some of them (spinal nerves of the lower vertebrates or certain cranial nerves, namely, the facial, glossopharyngeal and vagus nerves) are still mixed; c) that different proportions of the vagus nerve have, in the course of time, had variable attributions, somatic and visceral on the one hand and cerebro-spinal on the other. It might be argued that, in all these cases, it is merely a question of rearrangements of nerve *courses*, that the individual fibres involved may have had and preserved since their origin in the vertebrates one single function, and that these alone represent *organs* in the proper sense. This is debatable and the anatomist cannot do better than consider each nerve root as a discrete morphological unit, which he can therefore term an organ. Moreover, from the phyletic point of view, the uncertainty and diversity of the functions observed in these units are considerable; what is to be emphasised here is just this uncertainty in the delimitation of an organ and the attribution of its function.

F - It is therefore necessary to beware of absolute judgements with regard to the organ-function relationship in the central nervous system, including behaviour: there must be neither a rigidly deterministic language (whether mechanist or finalist) nor the extolling of an indeterminism, the source of a sort of free spontaneity not corresponding to a truly objective view of reality. Each case is to be treated separately. In any case, for the biologist concerned with detecting phyletic directions, it appears in the end, and paradoxically, that, from the most rudimentary forms of the major nerve organ (encephalon) up to its most complex developments, even if the delimitation of areas (*organs*) of projections or of organic motricities is possible up to a certain point, in other respects the spatial distribution of the *functions* becomes more and more vague as these functions become more and more multiform and flexible. Each moment of each organ-function pairing appears increasingly unique or incommensurable with any other, since the combinations that it includes have only a negligible probability of being exactly repeatable. In a parallel manner, if the most constraining *adaptations* are observed at the lowest levels of life, the most flexible *adaptabilities* are found at the level of the cerebral organ of the primates: particularly, in man[1]. On both sides, the organ-function relationship appears very different. Incidentally, it will be noted that the flexibility of the organ-function relationship at the higher biological levels is perfectly in accordance with the existence of diversified cybernetic feedbacks.

Finally, it is as well to point out also that the results of electro-physiological or neuro-chemical studies on the synapses, published in the general works at any rate, do not yet satisfactorily explain the extreme diversity of the functions. The inter-neuronic transfers are still ascribed by physiologists to a small number of mechanisms, and to explore their true wealth will be the task of a theory of probabilistic and combinatory communications (founded on the interplay of the unitary bits). Moreover, in general, and still within the cybernetic framework, one may hope, without indulging in the excessive enthusiasm shown by some, to derive useful explanations of the organ-function relationships from physico-mathematical models of living actions and reactions.

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[1] A parallel of this is found in the profound divergence of animal species with ultra-automatised functions contrasting with others that are far more flexible and unpredictable in their responses to stimuli. See STEPHAN, H. and P. PIRLOT. 1970. *Volumetric comparisons of brain-structures in bats.* Z. f. zool. Syst.u. Evolutionsf. Bd *8* (H.3) : 200.

In an extremely balanced work, Campbell and Hodos[1] presented a very convincing argument in favour of the classical notion of homology, as opposed to a superficial structuralist notion. For these authors, homology is related to the concept of the « same organ in different animals under every variety of form and function », proposed by Owen in 1843, to which some later authors have also added the idea of common ancestry. I myself have argued in favour of this idea[2]. From this point of view, the above-mentioned two authors insist, on the one hand, on the interest of the neuronic connections between encephalic sub-systems and, on the other hand, on the fact that, in order to establish real homologies, it is necessary to have recourse to a number of observations involving connections by nerve fibres, topology and topography, sulci, embryology, the morphology of the neurons, histochemistry, electrophysiology and the experimental modifications of behaviour. All this indicates an attitude of carefulness, resting on the theory of an actual and phyletic alterability of the relationships between organ and function. Campbell and Hodos warn, in particular, against too hasty interpretation of histochemical similarities isolated from their context. Practically at the same moment, there appeared an article by Parent and Olivier[3], who consider the presence of acetylcholine and certain mono-amines in the *corpora striata* of a reptile, a bird and four mammals as sufficient proof of homology between these structures. The word « phylogenetic » is used by these authors and, everywhere in their article, the implication is clearly phylogenetic. It must be asked whether this implication would not be rejected by all those who accept the concept of homology at present: namely, the structuralists on the one hand and the phylogenists on the other. The problem is whether one can homologise phyletically, on a solely histochemical basis, the ventral parts of the sauropsidian *striatum* with the dorsal layers of the mammalian *striatum*. The authors have used the word «homology » in a sense that is not strictly the one commonly attached to it in the biology of evolution. Their observations, which are very interesting, suggest only a functional *analogy*, which is not independent but very distinct from a homology. In conclusion, we again have here an indication that a careful distinction must be established between correspondence of organs and correspondence of functions.

G - On the other hand, in the neural organisation there exists a similarity with that which underlies the « Gestalt » theory in psychology. This term «Gestalt» has scarcely more to commend itself than its vagueness, and a slightly more precise, though less fashionable, language would replace it, according to the case - by "organisation" or "integration" or "systemisation" or "structuration" on the processes level, or "field" or "form" or "ensemble" or "structure" - if the result of the processes are concerned. One could discuss at length the term to be chosen from this vocabulary. In any case, it may be said that, like the mental states, the encephalic neural systems are organised into complex inter-relationships suggesting that the notion of *scope* is probably preferable to that of *area*, because it is less compromising from the topographical point of view, gives a clearer implication of the functional and makes room for a secondary connotation of gradient: all these conceptual elements provide useful shades of meaning in this mixture of indeterminism, originality and polyvalence that the descriptions ascribe to various encephalic regions.

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[1] Campbell, C. B. G. and W. Hodos. 1970. *The Concept of Homology and the Evolution of the Nervous System.* Brain, Behavior and Evolution, 3 : 353.

[2] Pirlot, P. 1970. *Réflexions sur les concepts d'homologie et d'analogie.* Cah. Et. Biol. 18-19 : 31.

[3] Parent, A. and A. Olivier. 1970. *Comparative histochemical study of the corpus striatum.* J. für Hirnf. 12 (1-2) : 73.

In other respects, if, beside large regulation capacities, extensive adaptivity in direct relation to the systematic hierarchy is revealed in the central nervous system, I feel fairly in agreement with the idea of a logic of neurons (of the Boolean type, in a network) which adapts itself to each situation. From here also, it seems to me, should derive an attitude of particular prudence towards the rigid conception of fields of perception, to which would correspond, point by point, the various neural fields, as advanced by an over-strict gestalt psychology. As Piaget remarks[1]: « Gestalt structuralism poses structures as anterior to all functioning and finally destroys the latter by absorbing it into structures without history ». Like Piaget, but from another point of view, I believe that the structures of the central nervous system have an ontogenetic and phylogenetic history and that one cannot escape this fact if one wishes to understand their function. The adaptations and regulations of this history constitute conspicuous events that contribute to the uncertainty of the organ-function relationship to the very extent that adaptations and regulations provide, chronologically, something new through relationship with an immediately preceding state.

Finally, in the same line of thought, it may be remarked that another concept of relatively recent origin, that of « *chreode* »[2], must also be taken with caution, as Waddington himself emphasises (op.cit. p.64). The « necessity » of the canalisations in the development is everywhere tempered by flexibility, which reconciles genetic determinism and more or less unexpected ambient effects. It seems to me that, in the case of the central nervous system, the part played by flexibility remains particularly great, even in the state of homeostatic completion of the system.

H - Comparative neurology has long since suggested to us, albeit too vaguely, that the general function of integration is perfected through the volumetric growth and complexification of the cephalic ganglion masses and this is confirmed in the pathology of the de-integrative accidents (agnosia, aphasia, apraxia, alexia and agraphia). The adaptive capacity of the organs increases and diminishes respectively in the same manner. It is, on the whole, impossible to speak of *the* organ-function relationship *in abstracto* in the case of the central nervous system. There exists *a whole range of relationships* between these two terms. This subject will be discussed further in the other study already announced[1]. There would also be much to say about another type of uncertainty that very probably affects the central nervous system: this is a certain « super-abundance » of encephalic neurons that has long been suspected in certain lines of vertebrates, the probability of its existence being confirmed by various observations[2]. This *provisional* superabundance offers extraordinary possibilities to the evolution of the central nervous system.

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[1] PIAGET, J., 1967. *Biologie et connaissance*. Paris, Gallimard, p. 286.

[2] WADDINGTON, C. H. 1962. *The nature of life*. New York, Atheneum. Allen and Unwin p. 64 seq.

In conclusion, it will be noted that these observations on the variability and flexibility of the relationships between nervous organ and nervous functions, all along the systematic scale of the animals, do not conflict with the idea put forward elsewhere[3], that evolution has involved a « cybernisation ». It is a question of evolutionary modifications affecting the two levels, cortical and subcortical, concommitantly. An increase of a few degrees of freedom, on the one hand, may very well appear side by side with the intensification of certain automatisms on the other hand. This question will be discussed further in a work now in preparation.

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[1] Bernier, R. and P. Pirlot, op.cit. The theme and numerous points that form the subject of the present article have originated from more general discussions, the result of which will be the above work in collaboration.

[2] Pirlot, P. 1970. *Relations pondérales entre l'encéphale et le corps chez les Chiroptères. II. Espèces paléo-tropicales et paléarctiques. Discussion et conclusions.* Bijd. Dierk. 40 (2) : 103.

[3] Pirlot, P. 1969. op.cit. p. 839.

HERTZ E HELMHOLTZ SULLE ONDE ELETTROMAGNETICHE[1]

Nella prima parte dell'articolo si esamina l'esperimento iniziale di Hertz del 1886 sull'induzione elettromagnetica prodotta da correnti dielettriche, ponendo in risalto la relazione fra questi esperimenti e la teoria elettrodinamica di Helmholtz del 1870. Si afferma che fu necessario ad Hertz liberarsi dalla concezione Helmotiana dei dielettrici prima di cominciare le sue ricerche sulla rivelazione in aria di onde elettromagnetiche. Nella seconda parte l'autore sottolinea il rapporto fra queste concezioni iniziali e la teorizzazione finale di Hertz dei principi dell'elettromagnetismo e dei fondamenti della meccanica. Questi risultati contribuiscono, secondo l'autore, ad una comprensione della posizione di Hertz nella scuola di Helmholtz e, in generale, del suo pensiero.

INTRODUZIONE. — Questo articolo si propone uno studio storico-critico di alcuni aspetti della scoperta di Hertz delle onde elettromagnetiche, attraverso un esame degli scritti di Hertz raccolti nel secondo volume degli « Untersuchungen Ueber die Ausbreitung der Elektrischen Kraft » (I,a) [1].

Mi propongo in particolare di evidenziare il ruolo che nel procedimento di scoperta ebbero le concezioni ereditate dallo Helmholtz, e quelle concezioni di Hertz che furono le premesse della scoperta e che furono poi precisate alla fine delle sue ricerche. In questo complesso gioco di interazioni, la teoria di Maxwell, o meglio quello che Hertz aveva acquisito di questa teoria, ha certamente un ruolo importante (il « duetto » è, in effetti, un « terzetto »). Ma, in questo articolo, il problema dell'influenza della teoria di Maxwell su Hertz sarà considerato solo per quel tanto che è indispensabile alla comprensione del problema principale, essendo questo un argomento che merita uno studio a parte.

È vero infatti, per ammissione esplicita dello stesso Hertz, che, per quanto egli ammirasse la teoria di Maxwell, « non era mai stato sicuro di avere afferrato il significato fisico delle sue (di Maxwell) affermazioni ». (I C, p. 20). Quello che qui importa è che questa incomprensione, spiegabile d'altra parte data la difficoltà e perfino l'oscurità dei testi di Maxwell - ebbe come conseguenza che le conoscenze iniziali di Hertz in elettrodinamica furono basate principalmente sulla teoria di Helmholtz. Queste ultime risultavano da una caratteristica mescolanza di azione a contatto nei dielettrici ed azione a distanza nel vuoto, palesamente un compromesso fra le idee del continente e le teorie del campo che si erano sviluppate in Inghilterra.

Non si può certamente ignorare la funzione avuta dalle caratteristiche concezioni Helmotziane nel guidare Hertz alla sua scoperta, tanto più che questa funzione fu a mio parere tutt'altro che generica, ma ebbe il merito unico di additare all'attenzione di Hertz un tipo di esperimento e di teoria in una zona in cui gli aspetti circuitali e gli aspetti locali (come si direbbe oggi) erano in sovrapposizione, rendendo così facile il congiungere gli uni con gli altri.

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[1] **Lavoro svolto con il contributo del C.N.R. (Contratto N. 6900606/113377, per il 1968/69).**

Dopo questo riconoscimento tuttavia si dovrà pure ammettere, come mostrerò in seguito, che la scoperta viene posta da Hertz come antagonista alla tradizione del pensiero scientifico di Helmholtz in quanto frutto di una sua ricostruzione del significato fisico della teoria di Maxwell a cui prestarono mano sia le sue originali elaborazioni teoriche del 1884 sia i suoi brillanti risultati sperimentali. Questa ricostruzione fu fatta sotto il segno della chiarezza logica e dell'amore per le astrazioni concettuali, tipici del pensiero tedesco, che in questo caso coesistevano ad una concezione fisica dell'azione a contatto nuova per quella tradizione.

L'ESPERIMENTO SULLA POLARIZZAZIONE DEI DIELETTRICI. — La ricerca in elettromagnetismo a cui Hertz si dedicò nel 1886 è quella proposta dall'Accademia della Scienze di Berlino. Il modo in cui Hertz iniziò questo lavoro, per cui l'Accademia della Scienze aveva proposto un premio nel 1879, viene descritto nella introduzione alle « Untersuchungen ». (I,c). La ricerca era stata suggerita da Helmholtz per il suo allievo Hertz, che, dall'autunno del 1878, aveva lavorato nel suo laboratorio di Berlino, « con la fiducia che Hertz vi si sarebbe interessato e l'avrebbe completata, come infatti fece, con successo ». (Prefazione di Helmholtz, 3,b).

Il tema della ricerca era così formulato:

« Trovare per via sperimentale una relazione fra forze elettromagnetiche e polarizzazione dielettrica degli isolanti - cioè, o una forza elettromagnetica esercitata dalle polarizzazioni nei corpi non conduttori, oppure la polarizzazione del non-conduttore come effetto della induzione elettromagnetica ». (1, c. p. 1).

Non è difficile capire perchè Helmholtz riteneva importante per la sua teoria un esperimento di questo tipo; la sua teoria della polarizzazione dei dielettrici era stata infatti formulata in modo che, con qualche sforzo, essa potesse ritenersi equivalente alla teoria di Maxwell. Il punto fondamentale era che le correnti di polarizzazione hanno, nei dielettrici materiali, effetti elettromagnetici come le correnti di conduzione. Helmholtz poi estrapolava questo comportamento dei dielettrici materiali allo spazio « privo di materia grossolana » (« void of gross matter »), che egli chiamava « etere », secondo l'uso tradizionale seguito anche da Maxwell. Ciò non toglieva però che accanto all'etere, Helmholtz ritenesse possibile l'esistenza di un « vacuum », in cui si potevano concepire cariche « nude » con i loro campi. In ciò egli seguiva la tradizione della teoria del potenziale, sviluppata in Germania da Neumann, Kirchhoff, etc. Era appunto questa dualità di concezioni, qui soltanto accennata, che io considero come il risultato di un compromesso fra la scuola continentale di elettrodinamica e le nuove concezioni di Faraday e Maxwell. Vi era inoltre l'aspetto metodologico, caratteristica di Kirchhoff ed Helmholtz, consistente nel loro attenersi ad una teoria fenomenologica [2].

Una delle conseguenze della carica « nuda » e della polarizzazione dell'etere alla Poisson, era, per la teoria, una velocità, di propagazione delle polarizzazioni diversa dalla velocità della luce. Poichè ciò rendeva difficile ammettere l'equivalenza fra questa teoria e quella di Maxwell, Helmholtz fu costretto ad escogitare una teoria « asintotica », che Hertz chiama « il caso limite della teoria di Helmholtz » [3]. Nella sua ricerca per l'Accademia, Hertz doveva verificare uno degli assunti fondamentali della teoria di Helmholtz, appunto la esistenza di effetti magnetici nella corrente di polarizzazione. Hertz non ritenne possibile affrontare la ricerca nel 1879, ma vi ritornò nel 1886, quando era professore di fisica alla Technischen Hochschüle di Karlsruhe: aveva allora trovato che potevano essere indotte con facilità delle scintille in una spirale metallica posta nelle vicinanze di un piccolo « rocchetto di induzione », quando la scarica del rocchetto avveniva attraverso uno « spinterogeno ». Quasi subito « comprese che il problema dell'Accademia di Berlino era risolubile » (I,c. p. 2).

Il metodo con cui Hertz affronta inizialmente nel 1886 l'esperimento ci aiuta a comprendere il tipo di dipendenza delle concezioni iniziali di Hertz in elettrodinamica da quelle di Helmholtz. Il procedimento di Hertz consisteva nel tentativo di provare che la parte di dielettrico che chiudeva un circuito, eccitato da un rocchetto Rumkorff, perturbava in qualche modo le correnti indotte in un circuito « secondario ». Ciò avrebbe dovuto essere provato dal fatto che, per rimozione del dielettrico, si doveva avere una variazione nelle scintille prodotte dal secondario. La presenza di questo effetto avrebbe significato che le correnti variabili presenti nel dielettrico, le cosidette correnti di polarizzazione, erano capaci di effetti elettromagnetici, d'accordo con Helmholtz, allo stesso modo delle correnti di conduzione [4].

Questo procedimento spiega anche il motivo del rifiuto di Hertz nel 1879 e il suo cambiamento di opinione nel 1886: egli aveva allora scoperto quella che gli sembrava una tecnica più sensibile per il rilevamento degli effetti induttivi, la maggiore sensibilità essendo dovuta, a suo parere, alla maggiore rapidità delle oscillazioni nel circuito principale. Egli aveva inoltre scoperto l'effetto della risonanza « la relazione sinfonica fra due circuiti interagenti » (« On Very Rapid Electric Oscillations », N. 2, in « Electric Waves, I,b, p. 42).

I risultati sperimentali avevano tuttavia disatteso la sua aspettativa, perchè non si aveva avuto nessun cambiamento nella scintillazione, dopo la rimozione della porzione di dielettrico. Il suo commento a questo scacco iniziale è:

« L'estate del 1887 passò in inutili tentativi di porre in evidenza l'influenza elettromagnetica degli isolatori ... L'esperimento falliva ogni volta per l'invariabile ripresentarsi di forti scintille nel secondario. Solo a poco a poco mi divenne chiaro che le legge da me ritenuta valida per interpretarlo non poteva applicarsi al suo caso: data la rapidità del moto, anche le forze derivabili da un potenziale erano capaci di indurre scintille nel circuito quasi chiuso (dell'indotto) ... » (I,c, pag. 5).

La permanenza delle scintille, sia in presenza dell'oscillatore chiuso che aperto, fu così spiegata ammettendo che, a circuito aperto, cooperavano al processo di induzione, assieme alle correnti nel filo, anche le cariche statiche alle estremità « aperte » dell'oscillatore. Si poteva così operare indifferentemente con la sola porzione metallica del circuito, e così fece Hertz. Ma fu ora sorpreso nel notare la distanza a cui avveniva l'induzione ed anche alcune sue caratteristiche: la spiegazione era però sempre la stessa, cioè la sovrapposizione degli effetti statici e dinamici, quella che egli era servita a spiegare il fallimento della sua prima esperienza. (I,b, pp. 87,89,91. Anche: I,g, pag. 137) [4]. Un esempio della difficoltà di liberarsi da un bagaglio teorico già acquisito.

Egli era ora anche in grado di dare una risposta positiva al tema dell'Accademia di Berlino: lavorando con l'oscillatore aperto aveva trovato una correlazione fra lo scintillio del circuito secondario e la presenza di un blocco di carta, materiale dielettrico, posto nelle immediate vicinanze dell'oscillatore stesso. Se al posto del blocco di carta si sostituiva un metallo, si otteneva lo stesso effetto. La spiegazione era data sempre dalla stessa teoria della sovrapposizione. Il risultato fu riferito all'Accademia il 10 novembre 1887 [5].

Si trattava ora, dopo il successo avuto nel verificare la prima parte del tema dell'Accademia, di trovare il modo migliore per affrontare la seconda parte « la polarizzazione dei non-conduttori come effetto dell'induzione elettromagnetica » e, a questo scopo, aveva cominciato a « foggiare nello stampo la paraffina a forma di anello chiuso ». Ma a questo punto, nella descrizione del procedimento, riportato nella Introduzione alla « Untersuchungen » - scritta circa cinque anni dopo - vi è come un momento di sosta, dedicato ad un ripensamento critico del rapporto fra l'esperimento stesso e la teoria di Maxwell. Si cita lo scritto di Helmholtz del 1870 (5,b), per affermare che Helmholtz aveva ivi derivato la teoria di Maxwell, « con l'aggiunta di tre ulteriori ipotesi alle leggi elettromagnetiche che, nel 1879, erano universalmente riconosciute valide », ipotesi che erano equivalenti alle seguenti:

« primo... un cambiamento nella polarizzazione dielettrica di un non-conduttore produce le stesse forze elettromagnetiche della corrente ad essa equivalente; secondo... sia le forze elettromagnetiche che le elettrostatiche possono produrre polarizzazioni dielettriche; terzo...

sotto tutti questi aspetti l'aria e lo spazio vuoto si comportano come tutti gli altri dielettrici ».

Il passo si chiude con l'affermazione:

« mettere alla prova tutte queste ipotesi e stabilire con ciò la fondatezza di tutta la teoria di Maxwell sembrava una richiesta non ragionevole e l'Accademia si era quindi contentata di una conferma di una delle prime due ». (I,b, pag. 7).

L'esperimento che egli aveva testé effettuato non era allora, secondo Hertz, una verifica di *tutta* la teoria di Maxwell, ma, al massimo, di *una parte*. Perchè, ci chiediamo ora, doveva essere irragionevole da parte di Helmholtz pretendere una verifica completa di Maxwell? Forse per ragioni di strumentazione, per mancanza cioè di sensibilità nei rivelatori tradizionali, prima della scoperta delle scintille nel secondario? Si direbbe questa la risposta più plausibile, ma ecco che, nel seguito del passaggio precedente, Hertz ci sorprende con un'osservazione inaspettata, che pone l'esperimento di Helmholtz sotto una luce diversa:

« Mentre lavoravo fui colpito dal pensiero che il centro d'interesse nella nuova teoria (cioè nella teoria di Maxwell) non consisteva nelle conseguenze delle due ipotesi. Se si fosse provata la loro correttezza per qualsiasi isolante ne seguirebbe che onde del tipo previsto da Maxwell vi si potrebbero propagare con velocità finita che potrebbe forse essere molto differente dalla velocità della luce. Mi convinsi che la terza ipotesi... » (I,c, pag. 7).

Secondo il passo precedente anche se una conferma completa delle prime due ipotesi fosse stata considerata tecnicamente possibile, tale conferma non avrebbe potuto considerarsi come una conferma « cruciale » della teoria di Maxwell, per Hertz. La ragione è, io credo, che Hertz è fermamente convinto che il « punto focale » della teoria di Maxwell è il concetto che la polarizzazione è connessa con l'etere. In questa prospettiva, il fatto, centrale per Helmholtz, che i dielettrici materiali sono polarizzati alla Poisson, non giustifica l'estrapolazione all'etere di questo tipo di polarizzazione, com'era nel procedimento di Helmholtz.

Ma soffermiamoci ancora un poco su questo aspetto che, a mio parere, sta alla base della scoperta. Questo punto cruciale per Hertz, per la consapevolezza del significato del suo esperimento nei riguardi della teoria di Maxwell, è collegato quindi, secondo la mia tesi, al significato diverso attribuito da Hertz alla terza ipotesi (l'aria e lo spazio vuoto si comportano come gli altri dielettrici), in contrasto con le concezioni elettrodinamiche di Helmholtz.

Continuiamo per ora la lettura del passo di Hertz già citato:

« Mi convinsi che la terza ipotesi conteneva il punto focale e distintivo della teoria di Faraday, e quindi di Maxwell, e che sarebbe stato più importante per me cercare di provare questo punto. Non vedevo alcuna via per mettere alla prova, nel caso dell'aria, separatamente la prima e la seconda ipotesi, ma ambedue sarebbero state simultaneamente verificate, se si fosse riuscito a dimostrare che nell'aria vi era una velocità finita di propagazione delle onde ». (I,c, pag. 7).

Per giustificare l'importanza attribuita qui da Hertz alla terza ipotesi, e, quel che più conta, le conseguenze che egli seppe trarne, si è indotti a pensare che a questo punto una concezione della polarizzazione più vicina alle intuizioni di Faraday e Maxwell sostituì la concezione alla Poisson. Tanto più che, come vedremo, solo la prima avrebbe potuto giustificare l'esistenza di onde nell'aria abbastanza intense per essere rilevate. La terza ipotesi implicava invece per Helmholtz una differente concezione: l'onda di polarizzazione era, per lui, un'onda di polarizzazione alla Poisson - Mossotti, effetto cioè della carica legata, di quella stessa carica che nello stato libero dava luogo alla corrente di conduzione.

Questo tipo di polarizzazione, com'era già noto al tempo di Hertz, è assai debole nell'aria: quest'ultima osservazione mi consente un'illuminante riscontro con un passo di Hertz del 1888, in uno scritto in cui si annuncia per la prima volta l'esistenza di « onde elettriche »:

« Quando forze elettriche variabili agiscono all'interno di isolanti le cui costanti dielettriche differiscono apprezzabilmente dall'unità, le polarizzazioni che corrispondono a queste forze esercitano effetti elettromagnetici. Ma il problema se nell'aria le forze elettriche variabili sono anch'esse accompagnate da polarizzazioni capaci di esercitare effetti magnetici è di tutt'altro tipo. Possiamo concludere che, se la risposta è affermativa, le azioni elettromagnetiche (nell'aria) si debbono propagare con velocità finita ». (I,e, pag. 107).

Il problema delle forze elettriche nell'aria è di « tutt'altro tipo », perchè, essendo la costante dielettrica relativa dell'aria quasi uguale all'unità, se esistono « forze elettriche » nell'aria, esse non possono attribuirsi ad una polarizzazione alla Poisson. Infatti, quando Hertz scoprì qualcosa che sembrava una propagazione di forze elettriche nell'aria, la teoria di Helmholtz non gli fu più di aiuto. Così si spiega il passo Hertziano dell'Introduzione, che riporto qui di seguito, e che, in mancanza di questa interpretazione, riuscirebbe veramente oscuro:

« Sono stato guidato (nei miei esperimenti) dal lavoro di Helmholtz, come risulta facilmente dal procedimento seguito negli esperimenti. Purtroppo nello speciale caso limite della teoria di Helmholtz che conduce alle equazioni di Maxwell, caso limite che sembrava adattarsi ad una interpretazione dei miei nuovi esperimenti, le basi fisiche della teoria di Helmholtz si perdono, come accade sempre quando si esce da una concezione dell'azione a distanza. Dovetti quindi cercare di formarmi in modo coerente le necessarie concezioni fisiche, partendo dalle equazioni di Maxwell ». (I,c. p. 20).

Le ombre, gli effetti di riflessione etc., che Hertz considerava giustamente effetti dovuti ai fenomeni di propagazione, sembravano richiedere in un primo tempo il sostegno della teoria « limite », o asintotica, di Helmholtz per essere correttamente interpretati. Ma

Hertz osserva che a questo tipo di interpretazione avrebbe fatto difetto una base fisica. Che cosa egli intenda con questo termine può essere desunto da svariati passi della Introduzione alle « Untersuchungen.... » (Parte B, Teoria, I, c.p. 24) ed anche da un suo scritto teorico del 1890 (I, h, p. 195, spec. Nota a p. 195). In breve, per « concezione fisica adatta a spiegare i fenomeni della propagazione », Hertz intende una *sua* concezione dell'azione a contatto come si vedrà fra poco. Nei riguardi della teoria di Helmholtz, ciò implicava che egli riteneva questa teoria fondamentalmente una teoria dell'azione a distanza, la quale veniva poi, mediante passaggi matematici, trasformata in una teoria formalmente equivalente a quella di Maxwell (vedi nota [2] e [3]).

Il differente significato che Hertz attribuiva alla terza ipotesi - significato su cui si incentra in definitiva la differenziazione fra le sue idee e quelle di Helmholtz - può essere compiutamente compreso solo alla luce delle sue conclusioni sulla natura della forza elettrica che egli raggiungerà alla fine del suo lavoro in elettromagnetismo. Di questo aspetto mi occuperò più a lungo nel capitolo successivo. Per darne qui un accenno, l'aria e il « vuoto », secondo Hertz, si comportano come gli altri dielettrici materiali non perchè essi dividono con questi la caratteristica di possedere « cariche legate » - com'era nella concezione di Poisson-Helmholtz - ma, proprio capovolgendo questa affermazione, sono i dielettrici materiali che partecipano della natura dello « spazio vuoto », perchè essi sono permeati dall'« etere ». Perchè la forza elettrica è, per Hertz, essenzialmente un effetto della polarizzazione dell'etere.

Ma, seguendo le concezioni Hertziane nel loro sviluppo storico, è solo qualche anno dopo che Hertz comincia a formulare esplicitamente le sue idee su questo punto:

« La conclusione più diretta (dell'esperimento che provava che le forze elettriche si propagavano con velocità finita) è che vengono confermate le vedute di Faraday, secondo le quali le forze elettriche sono polarizzazioni esistenti indipendentemente nello spazio » (I,e, p. 122).

Questo passo ci porterebbe a ricercare l'origine più profonda delle idee di Hertz, in uno scritto teorico di quattro anni prima (1884), in cui egli si interessa par la prima volta alla teoria di Maxwell (2,b). In questo scritto le equazioni di Maxwell vengono derivate dalle vecchie concezioni dell'elettrodinamica (di Ampère e Kirchhoff) e da ipotesi che non riguardavano affatto l'azione del dielettrico, *nel senso di Helmholtz*. Più precisamente queste equazioni vengono dedotte, in un modo assai interessante, da una combinazione della legge di induzione di Faraday, la legge di conservazione dell'energia ed il cosiddetto « principio dell'unità dei campi », per mezzo di un processo reiterativo che è matematicamente equivalente ad uno sviluppo in serie dei campi. Ciò potrebbe forse spiegarci il suo atteggiamento, quattro anni più tardi, incline ad attribuire alle « onde elettriche » un'origine non correlata alla natura dei dielettrici materiali. Ad esempio, la giustificazione del citato « principio dell'unità dei campi » è trovata nel fatto che « il campo elettrico, secondo le concezioni di Faraday, è qualcosa che esiste nello spazio per se stesso, indipendentemente dal modo in cui viene generato ». (2,b. p. 296).

Evidentemente ora, nelle ricerche del 1887, è la concezione fisica del campo che sostiene quella dell'onda elettrica e guida alla ricerca empirica di essa - assai diversamente di quello che poteva avvenire per la formulazione matematica di Helmholtz. Tornando infatti agli esperimenti conclusivi di Hertz, risulta dai suoi scritti come tale chiarificazione nell'impostazione concettuale si rifletta nella condotta sperimentale: egli comincia ora a cercare consapevolmente manifestazioni della esistenza, nell'aria, di onde elettriche; alcune osservazioni che erano rimaste marginali, quando egli cercava gli effetti del dielettrico nel senso di Helmholtz, diventono ora centrali: dapprima, com'è ricordato in un passo dell'Introduzione (I, c. p. 11), egli si era imbattuto in alcuni fenomeni che sembravano additare l'esistenza di ombre elettriche dietro alcuni blocchi di conduttori, ma non vi aveva attribuito molto peso. Un altro di questi esempi, che dimostrano l'interdipendenza fra la rilevanza del dato osservativo e il quadro teorico che lo inquadra e lo valorizza, è dato da Hertz subito dopo: lo cito specialmente per un rilievo relativo all'ultima parte:

« Mi era sembrato di notare, qualche tempo dopo, un caratteristico rinforzo dell'azione di fronte a quei blocchi in cui si produceva ombra ed anche alle pareti della stanza. Mi sembrò dapprima che questo rinforzo potesse avere origine da una specie di riflessione della forza elettrica da parte dei blocchi di conduttore; ma, sebbene i concetti della teoria di Maxwell mi fossero familiari, questa idea mi sembrò quasi inammissibile tanto essa contrastava con le concezioni allora correnti sulla natura della forza elettrica » (I,c. p. 11).

Si trattava evidentemente di riflessioni delle onde; ma, sia queste sia le ombre, furono trascurate quando Hertz si muoveva nel quadro della teoria di Helmholtz. Vi è certamente un'influenza sul pensiero di Hertz della forte concezione fisica che per Maxwell era espressa nella corrente di spostamento, anche se Hertz, ne dà qui una interpretazione sua propria [6].

L'ETERE COME DIELETTRICO. — Alla fine delle sue ricerche sull'elettromagnetismo - l'ultimo articolo delle « Untersuchungen..... » è del 1890 - il pensiero di Hertz è ormai su posizioni molto distanti da quelle di Helmholtz. Ma, per una curiosa combinazione, dovuta principalmente al fatto che Hertz, come si è visto, aveva inizialmente studiato Maxwell attraverso Helmholtz, le sue critiche sono dirette molto spesso alle concezioni di Maxwell e soltanto occasionalmente è messo in risalto il contrasto con Helmholtz. Questa circostanza richiede una attenta e circostanziata lettura dell'opera di Hertz e di Helmholtz ed ha sviato più di un lettore superficiale. È relativamente facile, tuttavia, separare nell'oggetto delle critiche di Hertz cioè che appartiene a Maxwell da ciò che è proprio della concezione di Helmholtz.

Nello scritto teorico del 1890 « Sulle Equazioni Fondamentali dell'Elettromagnetismo per i Corpi Fermi » (I,h), il punto della controversia riguarda quel concetto della polarizzabilità dell'etere, di cui, come abbiamo visto, Hertz si servì proprio per superare l'impostazione Helmotiana del suo primo esperimento sui dielettrici.

« La rappresentazione della Teoria Elettromagnetica che è propria di Maxwell non rappresenta il massimo desiderabile; vi è spesso in essa un ondeggiamento fra le concezioni che Maxwell ereditò e quelle a cui egli pervenne. Egli comincia con il postulare un'azione a distanza diretta e a ricercare le leggi con cui l'ipotetica polarizzazione dell'etere dielettrico dovrebbe variare sotto l'azione di queste forze a distanza e termina (invece) con l'affermazione che, sebbene la polarizzazione segua effettivamente questa legge, non è in realtà necessario che essa sia influenzata dall'azione a distanza. Questo procedimento lascia dietro di se una certa insoddisfazione e porta alla conclusione che vi deve essere qualcosa di errato o nel risultato finale o nel procedimento mediante cui lo si raggiunge ». (I,h, p. 195).

Per quanto Hertz faccia riferimento a Maxwell, nella sequenza della « polarizzazione dell'etere » che varia senza che sia « in realtà necessario che essa sia influenzata dall'azione a distanza » è evidente l'impronta della teoria Helmotiana del 1870 nel cosiddetto caso limite [1], [3]. Lo stesso Hertz, d'altra parte, lo ammette in una nota a piè pagina del passo riportato nel suo articolo (I,h. p. 195), ed, inoltre, implicitamente nell'Introduzione (I,c. p. 24 e seg.).

Nel resto dell'articolo egli contrappone alle « vecchie concezioni » quelle che egli ritiene siano più conformi alla teoria di Maxwell. Il nocciolo del discorso è sempre il concetto di polarizzazione: mentre le vecchie teorie cercavano di « spiegare la natura della polarizzazione attraverso quella dell'elettricità, assunta come nota », nella sua « rappresentazione », all'opposto, è la natura della carica elettrica che è definita « attraverso lo stato di polarizzazione dello spazio (considerato come noto) » (I,c. p. 26).

È a questo punto che la concezione Hertziana del dielettrico comincia a manifestarsi in tutta la sua originalità e nella sua forza innovativa rispetto alle concezioni precedenti della scuola continentale e vale quindi la pena cercare di approfondire il suo significato. Sino ad allora seguendo Poisson e l'estensione ai dielettrici, opera principalmente di William Thomson, la polarizzazione del dielettrico materiale era stata interpretata in termini di « carica legata », cioè il dielettrico era considerato sede di quelle stesse cariche libere a cui era dovuta la corrente nei conduttori. Tale comportamento dei dielettrici materiali si tendeva poi ad estrapolare all'etere (concezione ad es.

tipica di Helmholtz). Ciò comportava che lo status logico di « concetto primitivo » era attribuito alla carica, da cui il concetto di polarizzazione era « derivato ».

Hertz rovescia questa posizione; l'innovazione è per lui stretta conseguenza dell'esigenza di una concezione logicamente coerente dell'azione a contatto: nella vecchia elettrodinamica, fondata sulla teoria del potenziale per il vuoto e sulla Teoria di Poisson per i dielettrici, sia la forza elettrica (nel vuoto) che la carica, venivano entrambi considerati concetti primitivi. Ma tale concezione ha, per Hertz, significato « solo in quanto sia possibile rimuovere l'etere dallo spazio senza, con ciò, rimuovere la forza » (I,h, p. 196). Questo è, per lui, l'assunto di fondo, implicito in tutte le teorie dell'azione a distanza. Ma se si accetta come fondamento dell'azione a contatto l'ipotesi che nessuna forza può esistere in assenza di etere allora le posizioni precedenti vanno riviste in quanto contradittorie: questa in sintesi la posizione di Hertz.

Da ciò segue che non è possibile assumere come concetti primitivi in elettrodinamica la forza nel vuoto e la carica libera (nel vuoto) e poi ricavare mediante la teoria i loro adattamenti, per così dire, all'etere, come faceva Helmholtz. Il ruolo di grandezza primitiva va assegnato alla polarizzazione dell'etere: forza nell'etere e polarizzazione possono diventare, ora, per Hertz, dopo questo chiarimento, sinonimi:

« Nel caso dell'etere la forza coincide con la polarizzazione » (I,h, p. 210).

È evidente come Hertz rifiuti decisamente il compromesso che stava alla base della teoria dei dielettrici di Helmholtz. Ogni forza elettrica nei corpi materiali può essere legata al fatto fondamentale della polarizzazione dell'etere e, più decisamente ancora, ogni carica è l'effetto del fenomeno fondamentale della polarizzazione, ogni carica è carica di polarizzazione [7].

È in questa direzione che Hertz vuole riformulare una teoria dell'elettromagnetismo, fondata sull'azione a contatto, ma libera da quelle incosistenze logiche che turbavano, per lui, la teoria di Maxwell. La polarizzazione dell'etere avrebbe così un ruolo fondamentale nei fenomeni elettromagnetici ed ottici.

È ora importante sottolineare che questa innovazione non resta limitata, per Hertz all'elettromagnetismo, ma viene generalizzata, costituendo il caposaldo su cui Hertz vuole ricostruire una nuova meccanica. Secondo questa concezione altre proprietà dell'etere potrebbero offrire una chiave per la spiegazione di altre proprietà della materia, come ad es. la gravità e l'inerzia, senza fare ricorso al vecchio concetto di forza. A questo programma che rimase, per la sua prematura morte, incompiuto, Hertz dedicò il suo ultimo lavoro « Die Prinzipien der Mechanik » (3, a), una ricostruzione della meccanica basata sul principio che l'effetto dei vincoli può essere considerato in definitiva come equivalente a quello di «masse nascoste», aggiunte al sistema in uno stato di moto opportunamente scelto.

Nei «Prinzipien.....» vi sono varii passi che mostrano come questi sviluppi si ispirino alle sue antecedenti concezioni sull'elettromagnetismo; egli dichiara ad esempio che «dopo le elaborazioni di Maxwell si è quasi convinti che sia vera l'ipotesi che le forze elettromagnetiche siano dovute al moto di masse nascoste» (3,b, p. 26).

Anche se il quadro non è completato in Hertz da una interpretazione meccanica del significato della polarizzazione, è chiaro che il medesimo progetto di eliminazione di ogni forza sia in elettromagnetismo che in meccanica accomuna le due impostazioni concettuali (3,b, p. 41).

È evidente l'interesse di uno studio storico critico approfondito di quest'ultima parte dell'opera di Hertz [8], opera che si può in definitiva considerare come un primo tentativo di unificazione concettuale che precedette di dieci anni la Relatività Einsteniana. Ma quello che ci interessa qui ora è constatare quanto, in questi ultimi lavori, Hertz si sia allontanato dalle posizione fenomenologiche di Helmholtz. Un cenno indiretto a questa situazione ci viene fornito dallo stesso Helmholtz, in un passaggio di quella Prefazione ai «Prinzipien» di Hertz, che, scritto nel 1894, subito dopo la morte di Hertz, costituisce per altri aspetti un tributo di ammirazione ed anche di affetto del maestro al suo celebre allievo prematuramente scomparso:

«I fisici Inglesi sono stati evidentemente più attratti da spiegazioni come quella data da Lord Kelvin, nella sua teoria dei vortici atomici e da Maxwell nella ipotesi di celle dal contenuto rotante, su cui si basano i suoi tentativi di una spiegazione meccanica dei processi elettromagnetici.

Essi hanno preferito queste spiegazioni alle semplici rappresentazioni di leggi e fatti fisici nella forma più generale di un sistema di equazioni differenziali. Per quanto mi riguarda devo ammettere che io ho aderito al secondo modo di rappresentazione perchè, così facendo, mi sono sentito più sicuro. Ma ciò non toglie che io non abbia alcuna obiezione di fondo contro un metodo che è stato adottato da tre fisici di tanta eminenza» (3b, Prefazione di Helmholtz).

Il terzo fisico, dopo Thomson e Maxwell, è Hertz. Egli è qui considerato fra i rappresentanti della scuola inglese, di quella scuola cioè che opponeva alla fenomenologia tedesca concezioni di una rappresentazione fisica che va dal crudo modellismo meccanico di Thomson alle più sfumate e profonde considerazioni sul significato delle analogie che sono tipiche di Maxwell.

Conclusioni. — I risultati di questa ricerca suggeriscono considerazioni di carattere generale, su cui ora mi soffermerò brevemente, senza con ciò presumere di esaurire la tematica da essa proposta che suggerisce anzi linee ulteriori di verifica e di approfondimento.

Il momento della scoperta delle onde elettromagnetiche corrisponde quindi, secondo la tesi del presente articolo, alla acquisita consapevolezza, da parte di Hertz, che la concezione di un dielettrico « alla Poisson », non è sufficiente come concezione fisica delle « onde di forza elettrica » nell'aria. La provata esistenza delle onde, a sua volta, comporta e rafforza la convinzione che si tratta di qualcosa di non inquadrabile nella precedente tematica della scuola tedesca del potenziale, addirittura del concetto di polarizzazione dell'etere. Si ha qui anzitutto un altro esempio di una notevole interdipendenza fra le concezioni teoriche iniziali e finali e il procedimento stesso di scoperta, che viene così a perdere molto, almeno, del suo carattere fattuale.

L'esempio mi sembra inoltre abbastanza interessante perchè riguarda un periodo della storia della fisica ed una scoperta, che si ritengono già inquadrate in un processo lineare di sviluppo di una teoria, quella elettromagnetica, che, sviluppata da Maxwell, sembrerebbe avere aspettato soltanto chi si sarebbe presa la briga di confermarla. Mi sembra invece che, anche da questo punto di vista, viene suggerita una dinamica alquanto più complessa: l'intervento di Helmholtz con il suo concetto di polarizzazione è una componente necessaria nel processo della scoperta, qualora però venga superato quello che di limitativo vi era nelle sua concezione.

Altre considerazioni vengono suggerite al riguardo della nota accezione « formalista » del contributo di Hertz alla teoria elettromagnetica - basata principalmente sul famoso passo sul ruolo delle equazioni di Maxwell (I,b, p. 21), spesso citato da fisici e filosofi, ma molto spesso considerato al di fuori del suo contesto e del concreto momento storico in cui esso si inserisce nell'opera di Hertz. La forma che Hertz attribuisce alle equazioni di Maxwell è invece strettamente correlata, come risulta dal presente articolo, alla sua rappresentazione delle forze elettriche come polarizzazioni dello spazio: la forma delle equazioni sarebbe quindi interdipendente con la loro rappresentazione, con la concezione fisica che fa da sostrato alla teoria, e non si porrebbe, essa forma, come l'unica manifestazione della teoria, la teoria stessa per eccellenza. Ancora di più, se si riflette all'accennata correlazione fra l'elettromagnetismo e i « Prinzipien », la concezione dell'importanza dei « parametri nascosti » per una rappresentazione delle teorie fisiche, avanzata da Hertz, non è una di quelle che si presta ad essere facilmente assimilata nella tradizionale accezione « formalista » del ruolo di Hertz nello sviluppo della fisica.

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S. D'Agostino

Ringraziamenti. — *Nel completamento di questo lavoro mi sono state molto utili le conversazioni con M. B. Hesse del Dipartimento di*

Storia e Filosofia della Scienza dell'Università di Cambridge. Ringrazio anche il Prof. G. Jona, per aver letto questo scritto nella sua prima stesura nel gennaio del 1970 e per avermi aiutato con consigli e suggerimenti.

NOTE

[1] La storia delle origini del secondo volume delle « Untersuchungen..... » di Hertz, è narrata da D. E. Jones, il traduttore dell'edizione inglese dell'opera, che venne edita nel 1893 (I,b) con prefazione di William Thomson, Lord Kelvin.
Hertz volle stampare i varii contributi in cui venne prendendo forma la sua ricerca, pubblicati originariamente su *Wiedemann's Annalen* dal 1887 al 1891, nella precisa forma in cui essi erano apparsi, senza preoccuparsi del fatto che alcune delle sue idee erano considerevolmente cambiate nel corso dei quattro anni ed oltre si erano rivelate anche errate. Tali cambiamenti erano anche conseguenza delle serrare critiche a cui alcune di esse vennero assoggettate ed, in generale, dal grande interesse suscitato dal suo lavoro in tutti i laboratori e gli istituti del mondo. Egli però decise di aggiungere ulteriori note ed inoltre fece precedere la raccolta da una Introduzione in cui aggiunge ulteriori dettagli sul processo di sperimentazione e sulle concezioni via via sviluppatesi nel suo corso. Quest'ultimo fatto rappresenta una felice circostanza per lo storico, perché gli è possibile confrontare il pensiero di Hertz in due date successive del suo sviluppo.
Questa ricerca è stata condotta sulla edizione inglese (I,b), consultando l'originale tedesco (I,a) tutte le volte che ciò è stato ritenuto necessario per la comprensione del testo. Nelle note e nella bibliografia si fa riferimento sia agli originali tedeschi di Hertz e di Helmholtz, sia, tutte le volte che ciò è possibile, alle traduzioni inglesi, anche per comodità del lettore. Non mi risulta che l'opera di Hertz sia stata tradotta sino ad ora in Italiano. Di alcuni importanti aspetti dell'opera complessiva di Helmholtz, esiste una traduzione italiana (a cura di V. Cappelletti) in « *Opere di H. Von Helmholtz* », U.T.E.T. 1967. Le traduzioni in italiano dei passi di Hertz ed Helmholtz sono dell'autore dell'articolo.

[2] Un'utile rassegna di alcuni aspetti dell'elettrodinamica di Helmholtz si può trovare in un articolo di review scritto, all'epoca, da J. J. Thomson, in *British Ass. Rep.* (1885) 97. Per un'esposizione moderna delle stesse vedasi A. E. Woodruff, « The Contributions of Helmholtz to Electrodynamics ». *Isis*, 59 (1968) 300.
Nel suo scritto del 1870 Helmholtz definisce una costante dielettrica (diëlektrische Polarisations-Constante) come rapporto fra la polarizzazione P (der durch Vertheilung erzeugten elektrische Momente) ed il campo totale nei dielettrici. In notazione moderna: $P = \varepsilon\ (E - \mathrm{grad}\psi)$ (vedi 5,b, p. 612). E rappresenta la forza esterna, *agente a distanza*. Questo conduce alla relazione $P = \dfrac{\varepsilon}{1 + 4\ \pi\varepsilon_0}\ E$, ε_0, costante dielettrica dell'etere e dell'aria (5,b, p. 64), ε, costante dielettrica del dielettrico materiale. Questa relazione non corrisponde a quella oggi comunemente adoperata nella teoria elettromagnetica. In Maxwell, la « capacità induttiva specifica K », viene introdotta come $D = \dfrac{K}{4\ \pi}\ E$; D spostamento elettrico, E forza elettrica esterna. Mettere d'accordo le teorie di Helmholtz e Maxwell non è facile. J. Thomson (op.cit.) e Lorentz (Collected Papers, Vol. 2 p. 3) ne danno una diversa interpretazione. Ma lo stesso Helmholtz è consapevole che la sua teoria, concettualmente, non è quella di Maxwell:
« Le due teorie (cioè la teoria dei dielettrici di Helmholtz e quella di Maxwell) sono in un certo senso l'una il contrario dell'altro, perché secondo la teoria dell'induzione magnetica di Poisson, che può essere trasportata in modo analogo in quella della polarizzazione dielettrica degli isolanti, l'azione a distanza è diminuita dalla polarizzazione, mentre secondo la teoria di Maxwell viceversa la polarizzazione sostituisce esattamente l'azione a distanza..... ». (Helmholtz, 5,b, p. 556).

[3] Nel saggio citato del 1870 (5,b) Helmholtz deriva una equazione di propagazione per P, le cui soluzioni rappresentano onde sia trasversali che longitudinali. Nella teoria limite, già menzionata, le onde longitudinali scompaiono, mentre le onde trasversali si propagano con una velocità che tende, al limite, alla velocità della luce come nella teoria di Maxwell. Essa viene raggiunta per un valore infinito della costante dielettrica! Un'altra prova della fondamentale differenza delle concezioni di Helmholtz rispetto a quelle odierne. Le differenze di fondo fra le due teorie, riguardano la possibilità o meno di concepire un'azione delle cariche nel vuoto. In termini moderni si potrebbe dire che mentre, per Maxwell, anche in assenza di materia, (cioè nell'etere), si ha uno « spostamento elettrico », per Helmholtz, nel vuoto si ha solo una forza elettrica. Helmholtz ritiene possibile concepire « un vuoto », senza etere.

[4] Hertz credeva, a questo punto, che a causa della rapida variazione delle cariche statiche agli estremi aperti dell'oscillatore, le cariche indotte sul secondario, per la loro inerzia, non riuscissero a seguire le forze elettriche indotte, per cui la distribuzione risultante delle cariche non era più del tipo statico. Un pensiero innovatore a metà, perché la legge di induzione era sempre quella statica, e gli aspetti dinamici venivano spiegati ricorrendo all'inerzia. Questa teoria della sovrapposizione degli aspetti statici e dinamici dell'induzione fu sviluppato da Hertz, nell'articolo del 1888 « sull'Effetto di una Oscillazione Elettrica Rettilinea.... » (I,b, p. 80, 87, 93). Questo scritto è anche importante perché Hertz considera qui, per la prima volta, la possibilità di una velocità finita di propagazione della forza elettrica nell'aria. L'occasione gli è data dalla presenza di una polarizzazione cir-

colare della forza in alcune regioni vicine all'oscillatore: egli avanza l'ipotesi che questo possa essere causato da una sovrapposizione di forze elettrostatiche ed elettromagnetiche, con direzioni normali, i cui valori istantanei sono sfasati nel punto in oggetto a causa di una diversa velocità di propagazione: è sempre la tesi di Helmholtz della doppia forza!

[5] In termini tecnici odierni l'esperimento proposto da Helmholtz, attraverso l'Accademia di Berlino, riguarderebbe un effetto di induzione, (il rivelatore è nel cosidetto « campo vicino ») complicato della presenza di un dielettrico nelle immediate vicinanze. La trattazione di Hertz è qualitativa ed è dubbia, oggi, la riproducibilità dell'effetto. Ad ogni modo gli effetti di irraggiamento di onde non dovevano essere prevalenti.

[6] Per quanto riguarda il rapporto fra espressione matematica della teoria elettromagnetica e concezioni fisiche sottostanti, è interessante, e credo conforti la mia tesi, il seguente passo di Lenard, un collaboratore di Hertz, oltre che ben noto fisico:
« ....nei quindici anni trascorsi fra la pubblicazione del lavoro di Maxwell sull'Elettricità e il Magnetismo e la scoperta di Hertz, molte pagine erano state scritte a proposito della teoria di Maxwell.... senza che si vedesse neanche un indizio del procedimento per ottenere delle onde elettriche, perché la gente si divertiva solo a giocare con le equazioni, senza pensare alle idee di Faraday e Maxwell...... Hertz fu il primo che, oltre a comprendere le equazioni e a saperne ricavare gli sviluppi matematici, quando ciò era necessario, vide anche la struttura delle idee su cui si erano basati i suoi fondatori e comprese come occorreva servirsene ». (P. Lenard, Great Men of Science, London 1933).

[7] Hertz scrive le equazioni di Maxwell per uno spazio isolante in una forma equivalente a quella odierna, nel sistema di unità di Gauss. Questo sistema, che Hertz aveva adottato dietro suggerimenti di Helmholtz, si presta bene ad esprimere il concetto fisico Hertziano che, nell'etere, forza elettrica e polarizzazione coincidono, perché, in assenza di materia, la costante dielettrica è unitaria nel sistema di Gauss.

8] vedi anche: Robert S. Cohen « Hertz's Philosophy of Science: An Introductory Essay », in: Hertz, Principles of Mechanics (3,b).

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HERTZ AND HELMHOLTZ ON ELECTROMAGNETIC WAVES[1]

Hertz's initial experiment of 1886 on magnetic induction from dielectric currents is examined in the first part of the article and its connections with Helmholtz's theory of electrodynamics of 1870 are underlined. Evidence is given to the fact that Hertz, in order to be able to initiate his researches for detecting « electric waves » in air, had to abandon the Helmholtian concept of the relation between material dielectrics and ether. In the second part the author emphasizes the relation between the conceptions which led Hertz to the discovery of electric waves and his final rationalization of the principles of electrodynamics and the foundations of mechanics. These results have some relevance, according to the author, for an understanding of Hertz's position in Helmholtz's school and an inquiry into Hertz's philosophy of physical science.

INTRODUCTION. — The purpose of this article is to consider some aspects of Hertz's discovery of electromagnetic waves as they appear from the description that Hertz himself has given in the second volume of his collected papers, *Untersuchungen Ueber die Ausbreitung der Elrktrischen Kraft.* (I,a) [1].

I am mainly concerned here with the role, in the process of discovery, of the conceptions which Hertz inherited from Helmholtz and with those changes in Hertz's conceptions which preceded and followed the discovery. In this connection Maxwell's theory, or better Hertz's acquisition of Maxwell's theory, plays, of course, an important role. The « duetto » is in reality a « terzetto ». Those aspects however which concern the influence of Maxwell's theory on Hertz will be considered in this article only in so far as they represent essential ingredients for the arguments of the article.

The fact is that, in spite of the greatest admiration for Maxwell's theories, Hertz admits that he had « not always felt quite certain of having grasped the physical significance of his (i.e. Maxwell's) statements » (I,C, p. 20). This was partially due to intrinsic obscurities and difficulties in Maxwell's papers; for what concerns us here, however, it has the consequence that what Hertz initially knew about electrodynamics was mainly derived from Helmholtz's theory. This last was largely a compromise between continental views on action-at-a-distance and field theories which offered a peculiar blending of action-by-contact in dielectrics and action-at-a-distance in etherless vacuum.

It cannot be denied that Helmholtz's approach to electrodynamics played an important role in stimulating Hertz towards the discovery of electromagnetic radiation, not only in the sense of calling his attention to and arousing his interest in important aspects of electromagnetic phenomena such as dielectric polarization, but, more specifically, in my opinion, by indicating an area of research where circuital and field aspects of the theory could easily overlap.

But once the above points are made, one cannot fail to realize that Hertz's discovery did not happen within the tradition of Helmholtz's scientific thought: Hertz had to rediscover Maxwell's theory, or better his own reconstruction of Maxwell's theory, by his own effiorts, both by an original theoretical approach in 1884 and in the course of his skilled experimentation. In this rediscovery

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[1] This work has been supported by a grant from C.N.R.

he brought to bear all the logical lucidity and inclination for abstract thinking, which was traditional in the German school, together with a strong physical conception of contact action which was foreign to it.

In the following pages my aim will be to develop and elucidate the preceding questions.

HERTZ'S EXPERIMENT ON THE POLARIZATION OF DIELECTRICS. — In the Introduction to his *Electric Waves*, Hertz describes (I,c) his initial tackling of the research on the prize problem set by the Berlin Academy of Science in 1879. This research was suggested by Helmholtz, « in the belief that Hertz », who had been working in his laboratory in Berlin since the autumn of 1878, « would feel an interest in it and would attack it, as he did, with success » (Helmholtz's Preface, 3,b).

The title of the prize-research is the following: « To establish experimentally any relation between electromagnetic forces and dielectric polarization of insulators - that is to say, either an electromagnetic force exerted by polarizations in non-conductors, or the polarization of a non-conductor as an effect of electromagnetic induction » (I,c; p. 1).

The reasons why an experiment of this sort was considered relevant to Helmholtz's theory are not difficult to grasp; Helmholtz had developed a theory of polarization for dielectrics which had been put by him, with some effort, on a basis which he considered equivalent to Maxwell's theory. The fundamental assumption in the theory was that polarization-currents exert, in material dielectrics, magnetic effects on the same footing as conduction currents. This behaviour of material dielectrics was then by Helmholtz extrapolated to the space « void of gross matter », which Helmholtz called the « ether » in accordance with the well known tradition and Maxwell's usage. But side by side with the « ether » and its polarization, Helmholtz considered the existence of a « vacuum » in which «bare» charges and their fields were conceivable. This was in accordance with the theory of potential and electrostatics. This duality of action was, in a certain sense, the result of a compromise between the continental school of electrodynamics and the new conceptions of Faraday and Maxwell; moreover, the rather formal treatment of the theory was congenial to Kirchhoff's and Helmholtz' phenomenological approach to physics [2].

One of the consequences of the admission of a « bare » charge and of this crude extrapolation to ether of the poisson-type behaviour of material dielectrics was that the propagation velocity of the polarization force, as derived from the theory, was not equal to the velocity of light, a relevant difficulty if it had to agree with Maxwell's electromagnetic theory of light. In order to overcome the difficulty Helmholtz devised an asymptotic theory which Hertz calls « the limiting case of Helmholtz's theory » [3]. In his research for the Berlin Academy of Science Hertz was called upon to test the fundamental assumption of the theory, namely the existence of magnetic effects from polarization currents. This research did not seem feasible to Hertz in 1879, but he went back to it seven years later in 1886, when he was a professor of physics at the Technical High School of Karlsruhe. At that time he had found that the discharge of a small induction coil was very efficient in inducing sparks in a metallic spiral, « provided it had to spring across a spark gap ». After some further steps he « felt convinced that the problem of the Berlin Academy was now capable of a solution » (I,c; p. 2).

Hertz's initial method of tackling an experiment on this problem in 1886 presents to us some features which are very helpful for understanding the type of dependence of Hertz's initial conceptions on Helmholtz's views on electrodynamics. Hertz's proposal in his attack on the theme of the Berlin Academy consisted in trying to show that the dielectric portion of a closed circuit, acted on by variable currents through a Ruhmkorff coil, was affecting in some way the currents induced in a nearby « secondary » circuit, as manifested by the sparking in the sparking-gap of this circuit, following its removal. The expected effect would have been interpreted as the proof that variable currents in a dielectric - the so-called polarization currents - were also capable, on the same footing as conduction currents, of sustaining a magnetic field according to Helmholtz's theory [4].

This type of approach explains why in 1879 Hertz had refused to tackle this experiment and which were the reasons that convinced him of its feasibility in 1886: he had then discovered what seemed to him a more sensitive technique of detecting inductive effects and had connected this higher sensitivity to the higher rapidity of oscillation in the primary circuit; besides, he had discovered the effect of resonance « the symphonic relation between the mutually reacting circuits ». (« On Very Rapid Electric Oscillations », Paper N. 2, in *Electric Waves*, I,b, p. 42).

The result of the experiments however disappointed his expectations, for no relevant change in the sparking was observed in the secondary circuit, following the removal of the dielectric portion. Hertz comments on this initial failure of his research:

> «The summer of 1877 was spent in fruitless endeavours to establish the electromagnetic influence of insulators.... The experiment was frustated by the invariable occurrence of strong sparking in the secondary conductor. It only gradually became clear to me that the law which I had assumed as the basis of my experiment did not apply here; that on account of the rapidity of the motion, even forces which possess a potential are able to induce sparks in the nearly closed conductor... » (I,c, p. 5).

The invariable occurrence of sparks in the presence of both closed and open oscillators was thus explained with the concept that, when the circuit was opened, both electrostatic charges, from the open ends of the oscillator and currents in the wire, were cooperating in the process of induction. Since no change in sparks was observed, he kept experimenting with the metallic portion of the circuit only, the dielectric being permanently removed; he was surprised now by the great distance at which the inductive effect was observed and by the peculiarity as its actions and explained this fact by the same concept: the induction produced by the currents was in this case superposed to the induction due to the static-type charges at the end of the oscillator. The same theory which holds good for explaining the failure in his expectations was put to profit for a theorization of the dynamical effects of « open oscillators » (I,b, pp. 87, 89, 91. Also 1, g, p. 137) [4].

Hertz felt now able to give a positive answer to the theme of the Berlin Academy: almost incidentally, as it seems, he found that sparking in the secondary circuit was, this time, affected by the presence of a block of dielectric material (paper) situated in the immediate neighborhood of the open oscillator, just as it was by an identical block of metal in the same place. To give an explanation of this effect he adopted the same theory of a superposition of static and electromagnetics forces. The experiment was reported to the Academy on November 10th, 1887 [5].

Now, after the successful accomplishment of the first part of the theme of the Berlin Academy, Hertz was considering the best method of attacking its second part, « the polarization of non-conductors as an effect of electromagnetic induction », and to this purpose he had begun « to cast closed rings of paraffin ». In the Introduction, (I,c, p. 6) Hertz presents at this point a reconstruction - made about four years later - of the process of his thought at this turning point in his experimentation. It is a sort of critical appraisal of the relation to Maxwell's theory of the experiment he had just performed. He starts quoting Helmholtz's paper of 1870: « Uber die Bewegungsgleichungen der Elektricitaet... » (5,b), to the effect that Helmholtz had there derived Maxwell's theory « by adding three further assumptions to the electromagnetic laws that in 1879 enjoyed universal recognition ». These assumptions were, according to Hertz, equivalent to the following hypotheses:

> «first,.... changes of dielectric polarization in non-conductors produce the same electromagnetic forces as do the currents which are equivalent to them; secondly,.... electromagnetic forces as well as electrostatic are able to produce dielectric polarizations; thirdly,.... in all these respects air and empty space behave like all other dielectrics».

Hertz concludes the passage affirming that « the problem of proving all these hypotheses and thereby establishing the correctness of the whole of Maxwell's theory appeared to be an unreasonable demand; the Academy therefore, contented itself with requiring a confirmation of one of the first two » (I,b, p. 7).

Hence the single experiment just performed was not, in Hertz's opinion, a test of the whole of Maxwell's theory, but, at best, of a part of it. On what grounds would it have been an unreasonable demand on behalf of Helmholtz to ask for such a test of the whole of Maxwell's theory? Up to this point the answer would be presumably on technical grounds, i.e. lack of sensitiveness of traditional detectors, before Hertz's discovery of the sparkings in the secondary circuit. But shortly afterwards in the same passage, Hertz comes out with an unexpected remark, which turns out to be a different evaluation of Helmholtz's experiment.

> «But while I was at work it struck me that the center of interest in the new theory (i.e. Maxwell's theory) did not lie in the consequences of the two hypotheses. If it were shown that these were correct for any given insulator, it would follow that waves of the kind expected by Maxwell could be propagated in this insulator, with a finite velocity which might perhaps differ widely from that of light. I felt that the third hypothesis....» (I,c, p. 7).

According to the latter, even if Helmholtz had considered technically possible the confirmation of the first two hypotheses taken together - not of one of the first two separately - this test could not have been considered as a crucial confirmation of Maxwell's theory. It is my convinction that Hertz maintains here that the Berlin Academy experiment cannot be considered as a pertinent test of Maxwell's theory because the « gist » of the theory is the concept that polarisation is an effect of ether and material dielectrics only modify it more or less. In this perspective the fact that material deilectrics present Poisson-type polarization does not imply any stringent argument towards the possible existence of polarisation in ether.

This is a different type of evaluation of Helmholtz's experiment; even if mildly stated - or understated - it can be considered as an admission that Helmholtz's experiment was, at best, marginal to Maxwell's theory, especially since the central point in Maxwell's theory was the propagation of electric waves in air with the velocity of light. Hertz must have realized by this time that his apparatus allowed him a direct test of the whole of Maxwell's theory; and besides, what is more important, he realized which part of Maxwell's theory was crucial to test, namely electric waves in air and their velocity.

This turning point in Hertz's consideration of his experiment - Helmholtz's experiment - is to be correlated, in my opinion, as I y said, with the meaning attributed by Hertz to the third hypothesis (that air and empty space behave like other dielectrics), somewhat in disagreement with Helmholtz's conceptions in electrodynamics, as I'11 try to show in the following lines.

Let's start with Hertz's comment on this matter in the additional part of Hertz's quoted passage:

> «I felt that the third hypothesis contained the gist and special significance of Faraday's and therefore of Maxwell's, view and that it would thus be a more worthy goal for me to aim at. I saw no way of testing separately the first and second hypothesis for air, but both hypotheses would be proved simultaneously, if one could succeed in demonstrating in air a finite rate of propagation and waves» (I,c, p. 7).

To justify the emphasis given here by Hertz to the third hypothesis, and, more important, the consequences derived from it, one is led to the assumption that Hertz shifted at this point from a conception of the polarisation in the sense of Poisson and Helmholtz to that of a polarisation closer to the intuition of Faraday and Maxwell's «displacement», which alone could have explained the existence of electromagnetic waves of detectable intensity in air. Quite differently, the dielectric wave was for Helmholtz a wave of polarization in the sense of the Poisson-Mossotti polarization theory (1,C, p. 23); polarizability in this theory was the effect of the bound charge, the same charge that in its free state was responsible for the conduction current. In this last sense, I think, we must understand Helmholtz's statement that « air and empty space behave like all other dielectrics », the statement that Hertz calls the third hypothesis (I,c, p. 6). Helmholtz meant that air is polarizable « à la Poisson », but he knew, and Hertz also knew, (I,c, p. 106) that the polarizability of this type is, in air, very low in comparison with other material dielectrics.

This interpretation can be confirmed by a Hertzian passage at the beginning of the 1888 paper, which represents the first Hertzian announcement of the existence of electric waves:

> «When variable electric forces act within insulators whose dielectric constants differ appredibly from unity, the polarizations which correspond to these forces exert electromagnetic effects. But it is quite another question whether variable electric forces in air are also accompanied by polarizations capable of exerting electromagnetic effects. We may conclude that, if this question is to be answered in the affirmative, electromagnetic actions must be propagated with a finite velocity» (I,e, p. 107).

Since the dielectric constant of air is almost unity, the second part of the above passage must be understood as an indication that Hertz knew that

« variable electric forces in air » are connected to polarizations which are not of the Poisson type (« ...it is quite another question... »). Consistent with the interpretation is also the fact that when Hertz discovered something that looked like the existence of electric waves in air, he felt at a loss with Helmholtz's theory, because, in the general theory only polarization currents of the Poisson type existed in air.

If this is the case, we have an explanation of the otherwise obscure passage in Hertz's Introduction:

> «I have rather been guided (in my experiments) by Helmholtz's work, as indeed may plainly be seen from the manner in which the experiments are set forth. But unfortunately, in the special limiting case of Helmholtz's theory which leads to Maxwell's equations, and to which the experiments pointed, the physical basis of Helmholtz's theory disappears, as indeed it always does, as soon as action-at-a-distance is disregarded. I therefore endeavoured to form for myself in a consistent manner the necessary physical conceptions, starting from Maxwell's equations» (I,c, p. 20).

The existence of shadows, reflections etc., correctly interpreted by Hertz as a conseguence of a wave propagation, was pointing to the limiting case of Helmholtz's theory as a possible interpretation. But Hertz remarks that this theory was lacking a physical basis. This point is discussed by Hertz at some length in his Introduction to Electric Waves in Part B, Theoretical (I,c, p. 24) and also in a theoretical article of 1890 (I,h, p. 195 spec. Note at p. 195). In a few words, the physical basis of Helmholtz's theory disappeared because this theory was fundamentally an action-at-a-distance theory, and propagation in a medium was considered as a second order effect, which, in the so called limiting case, could be made preminent by a mathematical passage, only to comply with some results of Maxwell's theory (see notes [2], [3]).

The different meaning attributed now by Hertz to the third hypothesis can be better understood in the light of the conclusions on the nature of electric force reached by Hertz at the end of his work in electromagnetism, conclusions which are outlined in the last paragraph of the present article. These could be properly anticipated here by saying that, according to Hertz, air and empty space behave like other material dielectrics not for the reason that they participate in the action of the supposed bound charges - according to a Poisson-Helmholtz's conception; but, just on the reverse, material dielectrics behave like empty space for the reason that they participate in the nature of empty space, -i.e. they embed « ether ». For the electric force is, essentially, a polarization of ether. At this stage this clear-cut conclusion would be an anticipation.

But one year later Hertz will express himself more clearly on this matter: « The most direct conclusion (of the experiment on the finite velocity of propagation of e.m. forces) is the confirmation of Faraday's view, according to which the electric forces are polarizations existing independently in space » (I,e, p. 122).

A deeper consideration of the process which is at the origin of this Hertzian contribution must take into account, in my opinion, Hertz's first committment to Maxwell's theory in his 1884 theoretical paper. (2,b). In this paper Hertz had derived Maxwell's equations from the old electrodynamics and from hypotheses that did not imply a dielectric action, *in Helmholtz sense.* Maxwell's equations result from a combination of Faraday's induction law, the energy conservation law and the so-called «principle of the unity of fields», by reiteration process which is mathematically equivalent to a series expansion of the field. This fact might have counted in favour of his considering electric waves as dependent on something more essential than the behaviour of material dielectrics. An important point in this paper is the so-called « principle of the unity of fields »: the electric field has the same nature, irrespective of the mechanism through which it si generated, be it by standing or moving charges or by a static or changing magnetic field. The justification is found in the fact that « ... the electric field, according to Faraday's conceptions, is something existing in itself in space independently of the way in which it is generated » (2,b. p. 296). It is this physical conception of a field as a self-sustaining entity that makes possible the conception of a self-sustaining wave of electric force as a physical entity (not a purely mathematical one).

Let is resume our main theme after this short digression: that there was a change in Hertz's conceptions at this turning point is also manifest through a modification in his outlook upon his experiments: he now becomes aware that he is looking for waves of electric force in air. As a consequence, some observations which have been disregarded in the past as meaningless became relevant from his new position: he recalls in a passage of his Introduction having come across something like a formation of shadows behind conducting masses during his past experiments without paying much attention to it (I,c, p. 11). Another example of this lack of relevance in observations, when the proper theoretical framework is still missing, is given by Hertz in another passage:

« Somewhat later on, I thought that I noticed a peculiar reinforcement of the action in front of such shadow forming masses, and of the walls of the room. At first it occurred to me that this renforcement might arise from a kind of reflection of the electric force from the conducting masses; but although I was familiar with the conceptions of Maxwell's theory, this idea appeared to me to be almost inadmissible so utterly was it at variance with the conceptions then current as to the nature of an electric force (I,c. p. 11) ».

Both shadows and reflections were outstanding hints of propagation of waves that Hertz overlooked when he was looking for the induction properties of dielectrics. So, the last sentence, in as much as it is revealing of Hertz's change of attitude, is also confirmatory of my interpretation of the above situation [6].

DIELECTRIC ETHER. — Hertz's thought is now moving further and further from Helmholtz's on matters of fundamental physical conceptions. What might mislead the occasional reader is the fact that criticism in Hertz's writings is mainly directed against Maxwell and only secondarily against Helmholtz. In the conceptions, however, which Hertz attributes in these cases to Maxwell, one can easily perceive Helmholtz's hands: Maxwell is seen through Helmholtz's glasses.

What is at stake in the theoretical paper of 1890, « On the Fundamental Equations of Electromagnetics for Bodies at Rest» (I, h), is the concept of the polarizability of ether, the very concept through which it was possible for Hertz, as we have seen, to bridge the gap between electromagnetic induction in dielectrics and waves in air and in vacuum.

> «Maxwell's own representation does not indicate the highest attainable goal; it frequently wavers between the conceptions which Maxwell found

> in existence, and those at which he arrived. Maxwell starts with the assumption of direct action-at-a-distance ;he investigates the laws according to which hypothetical polarizations of the dielectric ether vary under the influence of such distance-forces; and he ends by asserting that those polarizations do really vary thus, but without being actually caused to do so by distance-forces. This procedure leaves behind it the unsatisfactory feeling that there must be something wrong about either the final result or the way which led to it» (I,h, p. 195).

In spite of the reference to Maxwell, it is easy to recognize in the sequence of the « polarization of the ether », which vary... » without being actually caused to do so by distance-force » the unmistakable imprint of Helmholtz approach to electrodynamics in the limiting case in his essay of 1870, as, partially at least, is admitted by Hertz himself in a note of the same article (I,h. p. 195).

In the lines that follow the quotation from the 1890 paper, Hertz contrasts the « older views » against the conceptions which he believes to be more consistent with Maxwell's theory. The central point in this new position is that the older views tended « to explain the nature of the polarization through the nature of electricity (assumed to be known) », whereas in the other « mode of representation », which he defends, the nature of an electric charge is defined « through the state of polarization of the space (regarded as known) » (I,c, p. 26).

Since the Hertzian conception of dielectrics represents the locus termini of his change of conceptions in electricity, it is worth dwelling a little longer on it. It was commonly accepted in electricity, since Poisson and William Thomson, that polarization should be considered as the effect of the « bound charge ». In this theory the electric « free charge » was given the role of a primary concept in electrical science, from which the concept of polarization was derived. Hertz, as we have seen, overturns this position. Polarization is no more to be considered à la Poisson; this innovation is for him the logical consequence of a consistent adoption of action-by-contact. Hertz, in his new position, maintains that in order to regard electric force in vacuum and free charge as primary concepts, according to the « older views », one should be able to define them independently of polarization; but this requirement has a meaning « so long as we can remove the ether from the space and yet allow the force to persists in it » (I,h, p. 196). This requirements is evidently impossible if we accept as a consequence of action-by-contact that no force can exists without an ether. For the reasons given above, force and free charge cannot be for Hertz primary concepts in a consistent representation of action-by-contact, but this role must be assigned to the polarization of ether.

If polarization of ether is the primary concept - as is also proved by the existence of electromagnetic waves in air and in vacuum, this would be Hertz's trend of reasoning - force can now be defined through polarization: « In the case of ether force and polarization coincide » (I,h. p. 210) [7].

It is along these lines that Hertz wants to reconstruct an electromagnetism, founded on action-by-contact, but free from those logical inconsistencies which were affecting Maxwell's theory. In this conception polarization of ether should have the role of providing an explanation for electromagnetic and optical phenomena.

But this is not the only innovation; Hertz wants now to generalize his new conception to the whole of physics: other properties of ether could offer the key for an explanation of other properties of matter, such as gravity and inertia, without any recourse to the old concept of force. To this attempt Hertz devoted his last work « *Die Prinzipien der Mechanik* » (3,a), a logical reconstruction of Mechanics founded on the principle that the effect of a mechanical constraint can be made equivalent to the effect of adding « hidden masses » in suitably chosen states of motion. Many references in the « Prinzipien » show the connection between Hertz's dielectric conceptions and his last work in Mechanics (3,b, see for instance: Introduction, p. 26 and 41). His « hidden masses » are certainly reminiscent of Maxwell's ethereal vortices, because he thinks that « through Maxwell's labours the supposition that electromagnetic forces are due to the motion of concealed masses has become almost a conviction » (3,b, p. 26).

Hertz's theorization of action-by-contact brought a radical change in Hertz's conceptions not only in electricity but throughout the whole of physics (3,b, p. 41). After refuting the concept of an electric force acting at-a-distance, he found that the concept of force itself had no place in a consistent conception of action-by-contact and made an attempt to rebuild a mechanics free from it [8].

In this last work he had also moved a long distance from Helmholtz's fundamental conceptions. This can be grasped indirectly, for instance, from a passage inserted by Helmholtz in his introductory Preface to the « Prinzipien », an otherwise notable tribute to his illustrious student, written shortly after Hertz's premature death in 1894:

> English physicists - e.g. Lord Kelvin, in his theory of vortex atoms, and Maxwell, in his hypothesis of systems of cells with rotating contents, on which he bases his attempt at a mechanical explanation of electromagnetic processes - have evidently derived a fuller satisfaction from such explanations than from the simple representation of physical facts and laws in the most general form, as given in systems of differential equations. For my own part, I must admit that I have adhered to the latter mode of representation and felt safer in so doing; yet I have no assential objections to raise against a method which has been adopted by three physicists of such eminence (3,b, preface by H. von Helmholtz).

In this sober but firm statement Hertz is considered as the third representative of the school of Thomson and Maxwell, rather than the intellectual pupil of Helmholtz himself.

CONCLUSIONS. — Coming to considerations of a more general character, I think that this article has shown that, in the case of Hertz's discovery of electromagnetic waves, we have a remarkable interdependence between the final rationalization of the Hertzian conception of a dielectric and the procedure by which the waves were actually discovered. The discovery of the waves corresponds to the moment when Hertz realizes that the conception of a dielectric « à la Poisson » is not sufficient to support the conviction of the existence of waves of electric force in air, but that the existence of waves must be somewhat connected with the polarization of ether. The discovery was in some way a consequence of this change of conceptions and strengthened, in turn, this change of conceptions.

I consider this example more relevant for the reason that it belongs to a period and a subject in history of physics which is often considered, by conventional sources, as rather « linear » in its development and not controversial in its general outlook. The implication of the above consideration for the understanding of the role of experiment and theory in the case of Hertz's discovery are not however fully developed in this article, and they are rather suggested as a possible field of future research.

Another consideration that presents itself as a consequence of what precedes is the following: Hertz's characteristic formalization of Maxwell's equations, as presented in his theoretical papers, is strongly related to his representation of electric froce as polarization of ether. The interpretation of Hertz's position in the « formalist fashion » - founded mainly on the famous and often quoted statement on the meaning of Maxwell's equation (I,b, p. 21) - is in need of a reconsideration in the light of those characteristics of the process of discovery which are outlined in this article. The famous statement is not to be isolated from its context and from the historical process, as it has been often the case with some scholarly comments on its meaning.

The need for such a reconsideration of Hertz's formalist interpretation is also evidence by the connections between Hertz's electromagnetism and his « Principles of Mechanics ». The conception of the importance of the « hidden » parameters for a consistent representation of physical theories, stressed by Hertz, in other parts of his last work, is not one can be easily assimilated to the conventional meaning of the formalist interpretation.

But this also requires further an a more detailed analysis.

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Acknowledgements. — *For the completion of this article in the present form I am grateful to Dr. M. B. Hesse, History and Philosophy of Science Dept., Cambridge University, for the stimulating conversation and comments on some topics related to the present research.*

From Professor G. Jona, University of Rome, I received useful suggestions and expressions of encouragement when this article was in the form of an early draft, in January 1970.

NOTES

[1] The story of the origin of the *Untersuchungen*, the collection of Hertz's papers on his electromagnetic researches, is suitably told in a note that D. E. Jones, the English translator, premised to the English edition of the German work, for which William Thomson, who also wrote a preface, suggested the title « Electric Waves » (I,b).

Hertz's conceptions and experimental procedures changed considerably throughout the course of his five years' work in electromagnetism, and some sections of his contributions must by the end have appeared perhaps to have been superseded in their experimental content, as well as in their theoretical outlook; this also happened as a result of the rapidly growing interest aroused among physicists of the time by the discovery. In spite of this circumstance, Hertz wanted to reprint the separate papers in the « Untersuchungen... » « in the form in which they were originally published », but decided to supplement them by explanatory notes and to write an introduction « in which he describes the manner in which the investigations were undertaken and also discusses their bearing upon electrical theory and the criticism to which they have been subjected » (I,b, English translator's note).

These facts represent a very happy circumstance for the history of Hertz's ideas; for the opportunity is given here to the historian of physics to follow the path of Hertz's original thought, first in his effective historical development, then in his own comments and mature views in the introduction: the researches on electromagnetic waves cover, in a narrow sense, the years 1887-91, and the introduction for the first German edition was written in 1892.

References in this article are to the English translations, whenever possible, or to the German originals. The English text has been checked with the German one when this has been deemed necessary.

[2] Useful remarks and informations on Helmholtz's electrodynamics can be found in a review essay of J. J. Thomson, in *British Ass. Rep.* (1885) 97. For a modern account of Helmholtz's theory see: A. E. Woodruff, « The Contributions of Hermann von Helmholtz to Electrodynamics ». *Isis, 59,* (1968) 300.

Helmoltz in his paper of 1870 defines a dielectric constant (diëlektrische Polarisations-Constante) as the ratio between the polarization P (der durch Vertheilung erzeugten elektrish. Momente) and the total field in dielectrics:

$\overline{P} = \varepsilon\,(\overline{E} - \text{grad}\ \varphi)$ (in modern notation)

(Bewegungsglesch, 5,b, p. 612)

$\overline{E}$, external force (gegeben äusseren Kräfte). φ Polarization Potential. (Potentialfunction der vertheilten Elektrizität).

E is for Helmholtz the traditional force, acting-at-a-distance. This definition leads to the relation:

$$\overline{P} = \frac{1}{1 + 4\ \pi\varepsilon_0}\ \varepsilon\ \overline{E}$$

ε_0 dielectric constant of ether (and air) (5,b, p. 614)

ε dielectric constant of a « material dielectric ».

As it is well known - Maxwell defines a specific inductive capacity K, as $D = \frac{K}{4\ \pi}\ E$ (in modern notation).

$\overline{D}$ displacement, $\overline{E}$ external force.

J. J. Thomson (op.cit.) finds that the relation between Maxwell's K and Helmholtz's, is of the type $K = \frac{\varepsilon}{\varepsilon_0}$.

As regards this relation. Lorentz (Collect. Papers, vol. 2, p. 3) writes,

$$K = \frac{1 + 4\ \pi\varepsilon}{1 + 4\ \pi\varepsilon_0}$$

which reduces to J. J. Thomson's K, if $\varepsilon, \varepsilon_0 \gg 1$.

As regards the relationship between Helmholtz's polarization P and Maxwell's displacement, Helmholtz himself was aware of the differences.

« The two theories are opposed to each other in a certain sense, since according to the theory of magnetic induction originating with Poisson, which can be carried through in a fully corresponding way for the theory of dielectric polarization of insulators, the action-at-a-distance is diminished by the polarization, while according to Maxwell's theory, on the other hand the action-at-a-distance is exactly replaced by the polarization... » (Bewegungsgleich. 5,b, p. 556) (Quoted in the English version reported by Woodruff, op.cit. p. 307).

[3] Helmholtz derives in his essay of 1870 a wave equation for P which gives both longitudinal and transverse waves of polarization having, in aether and in air, the velocities:

for longitudinal waves $c\ (1 + 4\ \pi\varepsilon_0) \sqrt{\frac{1 + 4\ \pi\Theta_0}{4\ \pi\varepsilon_0 K}}$

for transverse waves $c \sqrt{\frac{1 + 4\ \pi\varepsilon_0}{4\ \pi\varepsilon_0}}$

C velocity of light, K a constant, Θ_0 magnetic susceptibility. (*Bewegungsgleichungen*, (5,b, p. 627).

In the limiting case, the longitudinal waves, which are related to the electrostatic field, have an infinite velocity, i.e. disappear, while the transverse waves are propagated with the velocity of light C. The fact that, in Helmholtz's theory, the velocity of transverse waves is different from C is not a direct consequence of the adoption of a Mossotti-type polarization but of the introduction of the distant force E.

To add a few remarks to this important historical problem of the relation between Maxwell's and Helmholtz's theories of electrodynamics, it can be said that Helmholtz's procedure is that of extrapolating to the « unknown » ether the behaviour of material dielectrics, as investigated in Poisson-Mossotti, Thomson, and Faraday's researches. Maxwell's conception, on the other side, is much less straightforward: recent studies have stressed the difficulty of according the mentioned theories of dielectrics with Maxwell's theories of the ether.

[4] The first explanation of the dynamical effects of open oscillators was given in terms of a reinforcement in induction produced by the static-type charges located at the open ends of the oscillator.

The theory of a dynamical field as a superposition of static and induction field was fully developped in the paper (published in 1888) « On the Action of a Rectilinear Electric Oscillation... » (I,b, p.80, 87,93). This paper is also important due to the fact that here, for the first time, Hertz mentions the possibility of explaining the « neutral points » by a finite velocity of propagation of the electric force in air. This occurs when his circular detector shows him that there are regions in space where the force presents somewhat like a circular polarization. He advances the hypotheses that this is a consequence of the superposition of an electrostatic and an electromagnetic force, with normal directions, their instantaneous values differing in phase due to a different velocity of propagation. Even if the static force propagated instantaneoulsy, the electromagnetic force ought to have a finite velocity: the splitting of the force is in accord with Helmholtz's theory of 1870.

[5] Helmholtz's experiment was concerned with a near-field effect; the near-field zone is the zone where the inductive effects are prevailing. For this reason the experiment was an induction-type experiment.
In the near field we can have polarization current effects without having radiation. See for instance R. B. Whitmer, Calculation of Magnetic and Electric Fields from Displacement Currents, *American Jour. of Physics* 1965-481. These effects could be detectable by an induction-type experiment, i.e. an experiment in which the detector operates in the near-field zone.
In our modern theory we consider it possible to have radiation without polarization currents à-la-Poisson, as in the case of radiation in vacuum.

[6] See P. Lenard's remarkable comment (in P. Lenard, Great Men of Science, London 1933): « ... in fifteen years between Maxwell's publication of his work on Electricity and Magnetism and Hertz's discovery, a great deal had been written about Maxwell's theory... yet not even the beginning of a way to the goal had been made clear, for people simply played about with Maxwell's equations, but not with the ideas of Faraday and Maxwell's... Hertz was the first who not only understood the equations and knew how to deal with them mathematically when necessary, but also saw the structure of ideas upon which they were based by their originator, and understood how to move about in it ».
Lenard's statement in his laying stress not only in the equations but also in physical concepts and iseas, is a suggestive comment to the thesis of the meaning of the turning point in the Hertz's experiments.

[7] Hertz writes the equation in the form (I,b, p. 202) for a non-conducting space:

$$A\mu \frac{d\overline{L}}{dt} = \mathrm{rot}\ \overline{x} \qquad \mathrm{div}\ \overline{x} = o$$

$$A\varepsilon \frac{d\overline{x}}{dt} = -\,\mathrm{rot}\ \overline{L} \qquad \mathrm{div}\ \overline{L} = o$$

(in modern vect. notation)

ε Dielectricitätconstante . In ether $\varepsilon = 1$.
μ Magnetisirungsconstante . In ether $\mu = 1$.
X,L electric and magnetic force.
A velocity of light.

The Gaussian System of Units which Hertz adopted on Helmholtz's suggestion, serves well, in this case, to express Hertz's physical conception of the coincidence in ether of Electric Force, x, and Polarization, for, in this system, $\varepsilon = 1$ for empty space.

[8] Su also: Robert S. Cohen, « Hertz's Philosophy of Science: An Introductory Essay », in Hertz's, Principes of Mechanics, (3,b).

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ARISTOTLE AS A BIOLOGIST

The biological studies reported by Aristotle in Historia Animalium, Generation of Animals, *and* Parts of Animals, *have a closer resemblance to their so-called modern counterparts than have his cosmological theories. As a biologist he depended on observations made by his own sense organs; perhaps under the influence of his father, an Asclepiad physician, he performed his own dissections. His advice to trust the sense organs over reason may have influenced later biologists through William Harvey. He incorporated into his biological writings observations made by fishermen, hunters, and herdsmen. These were the technicians of his day, thus the character of his work in biology was directly related to the socio-cultural milieu in which he lived.*

> There are, as it seems, two ways in which a person may be competent in respect of any study or investigation, whether it be a noble one or a humble: he may have either what can rightly be called a scientific knowledge of the subject; or he may have what is roughly described as an educated person's competence, and therefore be able to judge correctly which parts of an exposition are satisfactory and which are not. That, in fact, is the sort of person we take the « man of general education » to be; his « education » consists in the ability to do this. In this case, however, we expect to find in the one individual the ability to judge of almost all subjects (Aristotle, P.A.,* 639a1 ff).

Aristotle himself, who began his treatise on the *Parts of Animals* with the lines just quoted, *was* the one individual with the ability, it seems, to judge of almost all subjects, and one of the subjects he dealt with was what we would call biology. But he also had a scientific knowledge of biology. He was a good biologist, a better biologist than a cosmologist, and I should like to look into some of the reasons why. The reasons relate to the life people lived in his day, and this is how the subject « Aristotle as a Biologist » fits into the framework of what we are all interested in today, science and the socio-cultural milieu.

Aristotle wrote a number of works on biology. One of these, which we may call by its Latin title, *Historia Animalium*[2], is a history of animals in the old sense of the word history, an inquiry into animals. It deals with animals alive and dynamic. It incorporates observations on animals, their structure, their habits, and their reproductive habits especially. Aristotle, like every one else, was interested in the methods by which creatures perpetuate themselves, and in fact, he wrote a whole treatise on the latter subject, the *Generation of Animals* [1].

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* **Abbreviations: G.A., *Generation of Animals;* H.A., *Historia Animalium;* P.A., *Parts of Animals.* The editions from which the quotations have been borrowed are specified in the list of References below.**

The third of his major biological works is usually called the *Parts of Animals* [3]. The translator of the Loeb edition of the *Parts of Animals* has said that the title of this should really be « *Of the Causes of the Parts of Animals* » (P.A., p. 8). This is the most speculative and theoretical of the three major works; it is the most physiological of the three, and the farthest away from what we believe today. There are also other less exhaustive Aristotelian writings on subjects related to biology: the *Movement of Animals*, the *Progressions of Animals*, the *Soul*, and others. There are in the *Historia Animalium* a number of references to what some translators have called a *Treatise on Anatomy*, but D'Arcy Thompson [2] points out that in classical Greece the word « anatomy » may mean a dissection as well as a book on dissection, as it did in England, for instance, between the 16th and the 18th centuries. And Aristotle did dissect, as we shall see shortly.

We shall confine ourselves here to consideration of the *Historia Animalium*, the *Parts of Animals*, and the *Generation of Animals*. Each of these three major works is in itself encyclopedic. Nordenskiöld [9] has said that together they enumerate around 520 species recognizable today, and so much information is included in the three works that it is moot whether Aristotle himself collected it all. There is one theory (see Singer, *Greek Biology and Greek Medicine* [11] for a discussion of it) that what has survived is a group of notebooks used by Aristotle for his lectures, or sets of notes on his lectures taken down by his students. There are a number of references to diagrams in the texts, which might bear out these theories. In any event, in the ensuing discussion, we shall speak of Aristotle as the author of the books that bear his name, and as his own dissector, unless he himself specifies otherwise.

He did, of course, take advantage of information provided by others. Pliny, in his *Natural History*, has written that « Alexander the Great, fired by desire to learn of the natures of animals, entrusted the prosecution of this design to Aristotle.... For this end he placed at his disposal some thousands of men in every part of Asia and Greece, and among them hunters, fowlers, fishers, park-keepers, herdsmen, bee-wards, as well as keepers of fish-ponds and aviaries in order that no creature might escape his notice » (cited from Singer, *op. cit.*, p. 22). Pliny was closer in time to Aristotle than we are, but he was still several centuries away from him, and a notorious exaggerator, so we must wonder as to his accuracy.

D'Arcy Thompson (H.A., p. vii) has pointed out that throughout the *Historia Animalium* references to places in Greece are few, although numerous to those in Macedonia and the coast of Asia Minor. After Plato died in 347 B.C., Aristotle spent several years in Atarneus and Mytilene, then Macedonia. D'Arcy Thompson concludes that the studies incorporated in the *Historia Animalium* were carried on between Aristotle's two periods of residence in Athens, and thus that they were probably written before his more philosophical works. According to this interpretation, the philosophical works were influenced by the biological, rather than vice versa.

The evidence, however, from the text of the *Historia Animalium* confirms Pliny's remarks about the variety of Aristotle's sources of information, and he tells what he has learned from the fishermen and the hunters, for instance: « Experienced fishermen affirm, some that they have at times seen in the sea animals like sticks, black, rounded, and of the same thickness throughout; others that they have seen creatures resembling shields, red in colour, and furnished with fins packed close together » (H. A., 532b20 ff); « Hunters ... say that the panther has found out that wild animals are fond of the scent it emits » (*ibid.*, 612a110 ff), and so on and so on.

So far as what Aristotle accepted from others is concerned, he has been accused of gullibility. For instance, he repeats without questioning it Herodotus' idea, accepted in his time, that « All animals move the lower jaw, with the exception of the river-crocodile; this creature moves the upper jaw only » (H.A., 492b24). But Aristotle had no reason to doubt this; in our own times, the Arthrodires, extinct fish-like animals, have been shown to have had a movable joint located dorsally in the skull. When Aristotle speaks of the salamander, he says that « this creature, so the story goes, not only walks through the fire but puts it out in doing so » (*ibid.*, 552b16). « So the story goes. » Is this a certain skepticism? Perhaps not. « Males are generated on the left-hand side of the womb, and females on the right-hand side, » he wrote when discussing the generation of sharks, considering an old fallacy, but then he continues « and males and females on the same side together » (*ibid.*, 565b13 ff). Is this the correction of an error?

This is not to imply that Aristotle himself made no errors; he made many that seem to us egregious. They are scattered throughout the writings on animals, and some of them are not even meaningful, especially those in the *Parts of Animals* where he looked for causes on a theoretical basis. « Man has more sutures in the skull than any other animal, and males have more than females. The size of the brain is the reason for this ...; it is to secure ventilation, and the larger the brain, the more ventilation it requires » (P.A., 653a35 ff).

The last passage quoted would hardly suggest that Aristotle had examined the skulls of men and women very carefully, if at all. But in general, because of the nature of what he was studying in the biological treatises, and the way in which he studied it, he was most nearly correct in what he recorded when he used his own eyes, or when he had direct evidence from others, such as the fishermen, who did so.

There is considerable evidence suggesting that he made many observations for himself. People who live by the sea might be expected to look at fishes. Aristotle made a point, however, of the importance of studying all kinds of animals, in one of the most wonderful passages of all biological literature:

> It now remains to speak of animals and their Nature. So far as in us lies, we will not leave out any one of them, be it never so mean; for though there are animals which have no attractiveness for the senses, yet for the eye of science, for the student who is naturally of a philosophic spirit and can discern the causes of things, Nature which fashioned them provides joys which cannot be measured. If we study mere likenesses of these things and take pleasure in so doing, because then we are contemplating the painter's or the carver's Art which fashioned them, and yet fail to delight much more in studying the works of Nature themselves, though we have the ability to discern the actual causes — that would be a strange absurdity indeed. Wherefore we must not betake ourselves to the consideration of the meaner animals with a bad grace, as though we were children; since in all natural things there is somewhat of the marvellous. There is a story which tells how some visitors once wished to meet Heracleitus, and when they entered and saw him in the kitchen, warming himself at the stove, they hesitated; but Heracleitus said, « Come in; don't be afraid; there are gods even here. » In like manner, we ought not to hesitate nor to be abashed, but boldly to enter upon our researches concerning animals of every sort and kind, knowing that in not one of them is Nature or Beauty lacking (P.A., 645a4 ff).

Some evidence that Aristotle relied on observations of animals mean or grand, his own and those of others, may appropriately follow:

> As a general rule, fishes have a prickly tongue, not free in its movements; though there are some fishes that present a smooth undifferentiated surface where the tongue should be, until you open their mouths wide andmake a close inspection (H.A., 503al ff).
>
> The dolphin ... is not provided, like quadrupeds, with visible teats, but has two vents, one on each flank, from which the milk flows; and its young have to follow after it to get suckled, and this phenomenon has been actually witnessed (H.A., 504b22 ff).

He had other interests in dolphins, too, and the following passage suggests that he used his ears as well as his eyes:

> The dolphin, when taken out of the water, gives a squeak and moans in the air, but these noises do not resemble those above mentioned. For this creature has a voice ...,

> for it is furnished with a lung and a windpipe; but its tongue is not loose, nor has it lips, so as to give utterance to an articulate sound (H.A., 535b32 ff).

Some noises above mentioned are those made by fishes, and the passage is of particular interest today, since it has only recently been acknowledged by others than fish-minded zoologists what noisy creatures fishes can be:

> Fishes can produce no voice, for they have no lungs, nor windpipe and pharynx; but they emit certain inarticulate sounds and squeaks, which is what is called their 'voice', as the *lyra* or gurnard, and the sciaena (for these fishes make a grunting kind of noise) and the *caprus* or boarfish in the river Achelous, and the *chalcis* and the cuckoofish ... which makes a sound greatly like the cry of the cuckoo, and is nicknamed from the circumstance. The apparent voice in all these fishes is a sound caused in some cases by a rubbing motion of their gills, which by the way are prickly, or in other cases by internal parts about their bellies; for they all have air or wind inside them, by rubbing and moving which they produce the sounds. Some cartilaginous fish seem to squeak (H.A., 535b15 ff).

Aristotle of course did not confine his observations to aquatic animals; terrestrial ones are included too.

> If stags be mutilated [castrated] when, by reason of their age, they have as yet no horns, they never grow horns at all; if they be mutilated when they have horns, the horns remain unchanged in size, and the animal does not lose them (H.A., 632a11 ff).

But Aristotle knew that for a biologist, looking and listening is not enough; he must do, he must use his hands. Put your hand to it, Vesalius was to say later; « I put my hand to the business. » Aristotle put his hand to it:

> The sea-nettle, or sea-anemone, clings to rocks like certain of the testaceans, but at times relaxes its hold. It has no shell, but its entire body is fleshy. It is sensitive to touch, and, if you put your hand to it, it will seize and cling to it, as the cuttle-fish would do with its feelers, and in such a way as to make the flesh of your hand swell up (H.A., 531a32 ff).

Also, as we have said, Aristotle dissected. He was born into a tradition whose practice was to examine, not speculate. He was son of a physician, Nicomachus; his father was physician to King Amyntas III of Macedonia, father of Philip. Nicomachus was a member of a guild (Asclepiads) whose members practised dissections; as the son of such a physician Aristotle would early have had opportunity to learn the value of accurate observation and, hopefully, careful dissection. Again, a number of passages testify to his recognition of both the perils and the importance of dissection.

> In the dead bodies of animals the nature of the chief veins is undiscoverable, owing to the fact that they collapse at once when the blood leaves them .... In *living* animals it is impossible to inspect these parts, for of their very nature they are situated inside the body and out of sight. For this reason anatomists who have carried on their investigations on dead bodies in the dissecting-room have failed to discover the chief roots of the veins, while those who have narrowly inspected bodies of living men reduced to extreme attenuation have arrived at conclusions regarding the origin of the veins from the manifestations visible externally (H.A., 511b13 ff).

Aristotle's understanding of the origins of the veins, no matter how attained, was not very accurate, according to our standards, but he took advantage of the techniques of the anatomists to recommend methods for the anatomical study of animals. « The investigation of such a subject » [the origin of the veins], he says, « as has been remarked, is one fraught with difficulties; but, if any one be keenly interested in the matter, his best plan will be to allow his animals to starve to emaciation, then to strangle them on a sudden, and thereupon to prosecute his investigations » (H.A., 513a13 ff).

According to Lones ([8] cited from Cole, *History of Comparative Anatomy* [4]) Aristotle dissected carefully twelve mammals, nine birds, four reptiles, two amphibians, ten fishes, one ascidian, seven molluscs, three arthropods, and a sea urchin. Still according to Lones, Aristotle gives anatomical details concerning 110 dissected animals, but not all of these dissections may have been made by Aristotle himself. Perhaps one passage might be reproduced, however, to exemplify the precise nature of some of the details he seems to have adduced from dissection: « The mole ... is deprived of sight; it has no eyes visible, but if the skin — a thick one, by the way — be stripped off the head, about the place in the exterior where eyes usually are, the eyes are found inside in a stunted condition, furnished with all the parts found in ordinary eyes, ... but all those parts are smaller than the same parts in ordinary visible eyes » (H.A., 533a3 ff).

Lones states that Aristotle seems not to have dissected the human body. But he knew something of its structure, and he knew the likeness of the monkey's body to it: « The monkey, as has been observed, is furnished with a tail. In all such creatures the internal organs are found under dissection to correspond to those of man » (H.A., 502b25 ff). He recognized, however, the dangers of depending on dissections of other animals than man: « The fact is, » he admits, « that the inner parts of man are to a very great extent unknown, and the consequence is that we must have recourse to an examination of the inner parts of other animals whose nature in any way resembles that of man » (H.A., 494b22 ff). This was what made trouble for speculators on human anatomy until the time of Vesalius.

Man was, of course, of primary importance for Aristotle. For all Greeks, man was the measure of all things. « We must take animals species by species, and discuss their peculiarities separately .... To begin with, we must take into consideration the parts of Man. For, just as each nation is wont to reckon by that monetary standard with which it is most familiar, so must we do in other matters. And, of course, man is the animal with which we are all of us the most familiar » (H.A., 491a5, 491a20 ff). « Man is the animal; » he writes here; « man has more sutures in the skull than any other animal, » he writes in the passage already cited on page 651 above. It is fascinating that he states it so coolly; this is what was to make difficult acceptance of the tenets of Darwin nineteen centuries later. Man may have been the divine animal to Aristotle, but Aristotle was not sure that he did not share his divinity: « Man is the only one of the animals known to us who has something of the divine in him, or if there are others, he has most » (P.A., 656a5).

If Aristotle knew man's place in nature, he also knew his place as a scientist and as an observer of nature, at least in biology, and his emphasis on the importance of observation was to bear its fruit later, as we shall see shortly. But it was later, rather than earlier, that biologists turned and returned to Aristotle, and his biological works were not as influential, in the Middle Ages, as his more logical and cosmological studies.

This was not, however, because of their unavailability. In the 9th century, the *Historia Animalium*, the *Parts of Animals*, and the *Generation of Animals* were translated into Arabic, in Bagdad, by Ibn al-Batriq, a physician; the Nestorian Christians knew Greek, and in fact had translated some works by Aristotle and Galen into Syriac even before the Arabs came on the scene. Michael Scot translated the Arabic version into Latin in Toledo early in the 13th century; the British Museum has an Arabic translation in ms which may well be the version from which Scot made his translation (see Peck, Introduction, P.A. [3], pp. 39-44). Robert Grosseteste, Bishop of Lincoln in the early 13th century, quoted from Michael Scot's translation of *Generation of Animals*, Albertus Magnus wrote twenty-six books *De Animalibus*, really a commentary on Aristotle, but in some cases adding to Aristotle's descriptions, and St. Thomas Aquinas requested William of Moerbeke to undertake new translations of Aristotle's biological works directly from the Greek. A number of other translations were made before 1500, so that it is evident that these works were, if not influential, at least not unknown. In the Renaissance, Aristotle's influence showed itself in the work of Fabricius, and, as we shall see, especially in that of Harvey, late in the 16th and early in the 17th centuries. Later in the 17th and in the 18th centuries, Aristotle was really denigrated, and as a biologist he became rehabilitated only at the end of the 18th century by Cuvier, and by Saint-Hilaire and others in the 19th. We are all schoolboys beside Aristotle, said Darwin.

But if biology became scholastic before the late Renaissance, it was because people did not take advantage of what Aristotle had told them. Vesalius began to break down biological scholasticism; he dissected. Perhaps, however, he was more an anti-Galenist than a pro-Aristotelian. But William Harvey, who in performing his experiments [7] made physiology a truly modern science, was a true Aristotelian. Harvey demonstrated experimentally that the blood circulates in the body, and is not continually made anew to ebb and flow in the great vessels; Harvey demonstrated that the heart is a pump, a machine, but he did so as an Aristotelian [5, 10].

It was Aristotle who had first emphasized the primacy of the heart among the organs. He had had a good observational basis for this, too. He examined chick embryos, opening incubated eggs day by day, as advised in the Hippocratic treatise on generation; the heart, containing red blood on the third day of incubation, is the first organ visible to the eye unaided, as was Aristotle's, by lenses:

> With the common hen after three days and nights there is the first indication of the embryo .... The heart appears, like a speck of blood, in the white of the egg. This point beats and moves as though endowed with life, and from it two vein-ducts with blood in them trend in a convoluted course ... and a membrane carrying bloody fibres now envelops the yolk, leading off from the vein-ducts. A little afterwards the body is differentiated, at first very small and white (H.A., 561a6 ff).

Aristotle observed the heart of the three-day chick, and based his physiological speculations on this factual observation. As a non-observer, he postulated the circular movements of the heavens, circular because circular movement is perfect. Harvey, in coming towards his concept of the circulation of the blood, said, «I began to think there was a sort of motion [of the blood] as in a circle. This I afterwards found to be true, » and he goes on in the next paragraph to speak of the motion of the blood as « circular in the way that Aristotle says air and rain follow the circular movement of the stars » (*Exercitatio Anatomica de Motu Cordis* [7], p. 70; see also works by Curtis and by Pagel [5, 10], listed below, on Harvey's relationships to Aristotle with regard to theories concerning the blood).

Harvey, like Aristotle, wrote a whole book on generation, and a fascinating one, available in a wonderful 17th century translation [6] written in a style almost as sonorous as the prose of Sir Thomas Browne. Here again, Harvey makes his acknowledgments to Aristotle: « In chief, of all the *Ancients*, I follow *Aristotle*; and of the later writers *Hieronymus Fabricius ab Aquapendente*, Him as my *General*, and this as my *Guide* » (Harvey, *Generation*, preface, n.p.). Another whole essay could be written comparing Harvey's book on generation with Aristotle's, but for our purposes here, only a single passage need be quoted.

> Give me leave therefore to whisper this to thee (friendly Reader) that thou be sure to weigh all that I deliver in these *Exercitations, touching the Generation of living Creatures*, in the steady scale of experiment; and give no longer credit to it, then thou perceivest it to be securely bottomed, by the faithful testimony of thy own eyes. This very thing did *Aristotle* perswade us to; who when he has discoursed much of Bees, added at last: *That the Generation of Bees is after this manner, appears by reason, and by those things which are seen to come to pass after the maner of Bees. Yet have we not a sufficient discovery of what may fall out. Therefore when the discovery shall have been completed, then is Sense more to be trusted to, then Reason. For so far onely is Reason to be relied upon, as those things which are demonstrated, agree with those things which are perceived by sense* (Harvey, *Generation* [6], preface, n.p.; Harvey's italics. The source of the passage in Aristotle is G.A., 760b28 ff).

Sense is more to be trusted to than Reason. This is the method of the modern biologist of any historical age. Aristotle could use it because as a biologist he could look on nature in the manner of the observing physician, or fisherman, or hunter, or herdsman, or bee-keeper, and this is a part of science and society. The fishermen, the herdsmen, the hunters were the technicians of his agricultural and sea-faring civilization.

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ALGORITHMIC ASPECTS IN COMPLEX SYSTEMS ANALYSIS

The importance of algorithmic procedures in physical models and generally in the study of complex systems is stressed. An informal account of some general techniques for the analysis of such systems - in particular Watanabe's and Zadeh's approaches - is given and a new proposal is made. In conclusion, some possible relevant implications of the previously discussed proposals are remarked.

I. - INTRODUCTION. — The purpose of this paper is to give a general, even if not exhaustive, account of some general techniques in the study of complex systems that have been very fruitful in their specific fields of application, suggesting at the same time their possible extensions in fields different from the original one.

During the exposition we will bear constantly in mind the theory of algorithms even if, sometimes, this connection will not appear at first sight; we, in fact, retain that many conceptual problems can be greatly clarified if we look at them from the theoretic-computational point of view.

It seems to us that a true and genuine dialogue between the new formalisms of physics and the group of new theories generally collected under the name of Cybernetics, in which the theory of algorithms plays an essential role, will be, no doubt, useful for both. It is not casual, we retain, that von Neumann and Wiener have been interested both in the foundations of Quantum Theory and the main problems of Cybernetics [1-5]; there is, in fact, a common conceptual background and a very strong relation between the new epistemological problems raised by Cybernetics and Quantum Mechanics.

We wish to stress the informal character of our paper. Owing in fact to the complexity and vastness of the problems and concepts involved we claim more to stimulate a useful, interdisciplinary discussion of these problems, even if sometimes only at an intuitive level, than to give a formal and complete exposition.

In the next Section we stress the importance of the algorithmic procedures in physical models. This computational aspect becomes particularly important in the study of the complex systems, which Cybernetics deals with, as is shown in the third Section. In the fourth Section two kinds of new possible techniques by which to analyze very complex systems are compared and a proposal that embraces both of them is made. Finally we remark some possible implications of these new approaches.

In Appendix some elements of lattice theory which we hope may help the reader, are reported.

2. - Algorithmic Procedures in Physics. — In this Section we will try to point out some algorithmic (i.e., computational) aspects of physical models that are generally neglected but nevertheless are very important regarding not only the previsional aspects of a theory but also its foundations, as the interpretation of the mathematical formalism.

The computational and the foundational aspects, which at first sight may seem *separable*, are often strongly related. Every theory, in fact, ultimately provides, in the setting of a suitable language, computational procedures allowing one to obtain previsions about the phenomena that it aims to describe. One of the main aspects of the previsions is then the *time of computation* which is often realistically short (this happens, for instance, in the case of most linear systems) but, however, in some cases (as, for some non-linear or very complex systems) may considerably increase and even become intolerably large.

In the latter case, the model has to be replaced by simpler ones which may however produce a lack of information about the system. Furthermore let us emphasize that any discussion on the computational aspects of a physical theory is meaningful only if the *precision* required in the prevision of the phenomena is also specified. Generally the greater the precision the greater the computational time. These two factors often determine the use of either one model or another or of many models at the same time. A model works well until the required precision is comparable with the « tolerances » allowed by the use made of the theory itself.

The need for simple algorithms naturally leads one, for instance, to « linearize » problems that are highly non-linear; this often happens, however, with a loss of information about the system. Such attempts are linked, from a philosophical point of view (as has been clarified by Böhm [6]) to a « Weltanschauung » in which the systems are analyzed in separate parts, more and more simple, mutually interacting. This implies a sharp separation between observer and observed object.

Such a conception of the world works well at a macroscopic level and in some areas of microphysics but nevertheless seems to break down at different levels of organization. Quantum Mechanics, for instance, would represent at the same time the last step of a conception of this kind and the first attempt toward one in which such a separation into actually distinct parts does not take place. If, in fact, it maintains the classical division between observed object and measurement apparatus, it breaks, nevertheless, with classical conceptions for it is assumed (according to the orthodox interpretation) that there is a strong and uncontrollable interaction during the measuring act: under these conditions it seems not very meaningful to go on speaking of separate systems.

To quote Böhm [6]:

> « *The classical idea of the separability of the world into distinct but interacting parts is no longer valid or relevant. Rather, we have to regard the universe as* « an undivided and unbroken whole » ... *A centrally relevant change in descriptive order required in the quantum theory is thus the dropping of the notion of analysis of the world into relatively autonomous parts, separately existent but in interaction. Rather, the primary emphasis is now on* « undivided wholeness », *in which the observing instrument is not separable from what is observed* ».

Quantum Theory re-obtains, nevertheless, a division into separate parts even if at a more abstract level: that is the one of the quantum state (or its representative, the vector in Hilbert space). The need for such a separation arises, we presume, mainly from feasibility and computational requirements; a global analysis can generally be made only by non-linear equations (in which an analysis in independent components is not relevant) or by other mathematical devices that are highly unmanageable. Such models (as, for instance, those of General Relativity) may be very often useful in giving some global information but, generally, are too complex for providing specific previsions. Sometimes serious difficulties arise even in the physical interpretation of the mathematical formalism. One is then forced to use models that, even if they seem might not equally satisfying and complete, are nevertheless able to provide the needed quantitative previsions.

We then see how the algorithmic aspects contribute to determining the choice of a model and so, finally, a conception of the world too.

3. - Cybernetics and Computability. — The typical cybernetical systems (such as brain, social organizations and so on), with which we will briefly concern ourselves in this Section, are generally characterized by a great complexity, mainly due to a very strong interaction among their elements, which prevents a meaningful and complete description of the system itself considered in its wholeness. The need for a division of such systems into relatively autonomous parts mutually interacting then naturally arises. In these cases, however, unlike classical sciences as Thermodynamics in which a good description of the systems is made in terms of few yet known macroscopic parameters such as pressure, volume, temperature, etc., a serious preliminary difficulty generally consists in determining the meaningful parameters and relationships in terms of which to reach a useful description of the system. This often makes is still harder to find useful man-

ageable models. In these cases or whenever the knowledge about the system is at a very preliminary stage a good description may be a black-box like one. This substantially means that one limits oneself to a description of some characteristical behaviours, or « functions », performed by the systems (or some of them), i.e. one gives an algorithmic procedure realizing those functions. These situations very often happen in Cybernetics; one of the main problems of it, for instance, is to see *if and how* it is possible to design « machines », or « automata », reproducing such typical activities of the human mind as « memory », « learning », « abstraction capacity » and so on. An explanation of this problem, usually called *Artificial Intelligence* (see, for instance, [7]), can be obtained once it has been clarified what is meant by « Intelligent Machine » [8]. A satisfying formalization of the notion of *machine* is that given by A. M. Turing [9] who proposed a device, called after him *Turing machine*, that is an abstract prototype of digital computer, reproducing the characteristic features of the computing behaviour of man; let us however observe that the identification between the intuitive notion of algorithm and the formal one of Turing machine is an assumption (Church's thesis) even if no examples have been hitherto found of functions intuitively computable but non-Turing computable and any attempt at formal specification of the notion of « mathematical effectiveness » has defined classes of functions (for instance, the *general recursive functions*) equivalent to the class of Turing computable ones (see, for instance, [10-11]).

The Theory of Computation based on such a notion of machine has been of main importance for it allowed the proof of general limitations (*undecidable problems*) of the « capabilities » of any digital machine, also with a potentially infinite number of states. It is then clear that the Theory of Algorithms by itself is very important in Cybernetics.

Among many other applications of this theory we remember here the recent new precise definitions of information and entropy based on it, independent from classical probabilistic concepts, given by Kolmogorov and others [12-14].

The design, however, of machines able to perform in a realistic time some specific tasks in relation to some prefixed ends is not easily handled by means of this theory. Turing Machines, in fact, are prototypes of exact algorithms, in the setting of which such problems as « the precision of the results », « computational time », « reliability », « complexity of the algorithms », « relation between analog and digital machines » either have not yet been adequately described or have not been formulated in such a way as to get results that can be practically used. There is indeed a large number of classes of problems for which it is actually desirable to obtain some previsions in a *short time* even if at the expense of their precision. Generally, the greater the prescribed precision, the greater the complexity of computation, the latter being related to the number of states of the machine and to the time of computation[1].

The previous problems are of primary importance in the study of « artificial intelligence, » seen as « inductive inference », in which such concepts as « probability », « precision », « heuristic procedures », « complexity » and such, play an essential role [7, 15-21].

Bearing well in mind the Theory of Computation one asks then if and how it is possible to elaborate languages by means of which to describe algorithmic procedures in which the previous intuitive concepts are, in some way, formalized.

In the following, we will call these procedures « non-standard algorithms »; the word « non-standard » refers, then essentially to their use and aims rather than to the algorithms themselves: in other words, it is a different way of looking at the theory of algorithms more than an enlargement of the theory itself.

The search for such « non-standard » algorithms has been hitherto approached from different points of view and with different techniques and aims. A review of two « non-standard » algorithms that we find particularly noteworthy in relation to some situations very often appearing in the study of complex systems and a new proposal will be reported in the next Section.

4. - AN ANALYSIS OF SOME PROPOSALS.

— In this Section we will consider some proposals of non-standard techniques for the analysis of complex systems that have been made in recent years, hoping to stimulate a useful comparison among them, a comparison that in many cases has been much lacking, paying particular attention to making their specific fields of application evident.

A common feature of complex systems is that generally a detailed traditional analysis of them is not feasible. This may depend on the enormous greatness of the computational time; if the complexity is a purely combinatorial one, that is, if it is caused by the very great number of elements of the system (as, for instance, a gas) a fruitful description is in many cases obtained by means of the classical theory of probability. There are, however, many other systems - as, for instance, cybernetical and even quantum ones - a classical probabilistic analysis of which often is not entirely satisfactory.

The complexity of a system indeed can really be not a purely combinatorial one but be accomplished or just be given by other causes as, for instance, a strong and impredictable perturbation caused by the measuring act or an intrinsic difficulty in defining in an unambiguous way the meaningful variables of the system and the relationships among them. We moreover observe that these features may be present at the same time and hardly separable, thus making it even more difficult to find a useful description of the system; we will take into account this situation at the end of this Section.

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[1] In a recent work by D. Tsichritzis (see Ref. [41] p. 85) some considerations similar to the ones sustained by us are made: « *...in the framework of recursive function theory, practical problems are usually complex and often can be proved to be unsolvable. On the other hand, a heuristic approach often enables us to claim that almost every problem is « approximately » solvable. The reason for this discrepancy is that humans are not always interested in an exact and general solution; in most of the cases the practical problems are so stated that a precise solution is not even easy to define* ».

Although a great deal of work has been done, we will at present confine ourselves to considering Watanabe's [22] and Zadeh's [23] recent proposals. Our aim is to point out the differences between them and their implicit relationships (even if only at a formal stage) with other theories such as, for instance, *Quantum Theory* [24] and *Many-valued Logics* [25-26]. We will do this essentially by means of an algebraic analysis of their proposals.

Let us briefly examine Watanabe's program. The central point of his analysis is the recognition that a non-negligible and uncontrollable interaction between object and observer is not an exclusive feature of the microscopic world, but is actually the rule even in a large class of other situations. He therefore tries to adapt the formalism of Quantum Theory[1] that works so well in some fields of microphysics, to many general situations, satisfying some required prescriptions, as, for instance, *Information Theory* and *Pattern Recognition.* His program is thus essentially an attempt to extend the domain of application of the formalism of Quantum Theory outside physics.

The main algebraic aspect of Watanabe's approach is the *non-distributivity* (see Appendix) of the algebraic structure underlying it.

Watanabe makes two observations in support of his choice of non-distributive logics[2] (in particular he pays attention to modular logics[3], namely he notes that: « *unlike intuitionistic logic, modal logic or three-valued logic, modular logic has a definite domain of human experience in support of its validity and is a very natural extension of the Boolean logic which is certainly valid in a certain limited domain of experience* », and that: « *the fact that modular logic includes Boolean logic as a special case makes it consistent to use a Boolean language as the meta-language explaining the modular logic* » [27].

As far as the first observation is concerned, we express the opinion that also other logics refer to an actual and well-defined (that is identifiable) class of problems and situations that do not coincide with Watanabe's. We shall come back to this point later when we will consider Zadeh's proposal. As regards the second remark we simply note that the inclusion of Boolean structures as special case is not an exclusive feature of modular logics, but is also a possible property of other ones.

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[1] See [24], in particular chapters 5, 6, 7, 8 and references therein.

[2] Here and often in the following we will use the word « logic » as synonymous of algebraic structure.

[3] Let us note that the standard formulation of Quantum Mechanics does not make use of a modular logic but of a weakly-modular one (see Appendix) for modularity is in contrast with the « localizability » (see Ref. [24]). We however will not make this difference in the following for we are mainly interested only in the non-distributivity of the lattice.

The other class of proposals we intend to consider, as we said at the beginning, is that of *non-complemented logics* (in particular Zadeh's theory). In order to better understand Zadeh's proposal and generally non-complemented logics, we will make a brief general disgression about propositions.

In the classical interpretation by « proposition » is meant any linguistic expression considered *true* or *false*. A problem really arises when we observe that only a very small part of the sentences we are constantly confronted with satisfies the previous prescription. It is then possible to take two different positions when we consider such expressions that, owing to intrinsic *indefiniteness*, do not allow a yes-no classification. The first admits that if it is not possible to assign a value, true or false, to a proposition, this is due to the fact that not every condition has been appropriately specified. Once this has been done the « generalized propositions » will reduce to usual ones. The second points out that this assumption is not a natural one and is only an arbitrary attempt to force the world into an « a priori » scheme not supported by real situation, if we consider them free from preconceived ideas. According to the upholders of this approach, (among them, Zadeh) we have to study these « generalized propositions » without modifying them.

Let us now give a brief algebraic outline of Zadeh's proposal.

Let I be a universal class. By *generalized characteristic function* (« fuzzy set » in the terminology of Zadeh) we mean any map from I to the unit interval of the real line:

$$f : I \longrightarrow [0, 1] \tag{4.1}$$

If $\mathscr{L}(I)$ denotes the class of all maps (4.1) it is possible to give a lattice structure (see Appendix) to it by the binary operations $\vee$ and $\wedge$ that associate with any pair of elements f and g of $\mathscr{L}(I)$ the functions $f \vee g$ and $f \wedge g$ of $\mathscr{L}(I)$ defined point by point as

$$\begin{aligned} (f \vee g)\ (x) &= \text{Max}\ \ \{f(x),\ g(x)\} \\ (f \wedge g)\ (x) &= \text{Min}\ \ \{f(x),\ g(x)\} \end{aligned} \tag{4.2}$$

These operations generalize the set-theoretic *union* and *intersection* expressed in terms of characteristic functions. $\mathscr{L}(I)$ is a *distributive* but *non-complemented* lattice (see Appendix) with respect to the operations (4.2); besides it is a Brouwerian lattice [28]. The non-existence of a complementation is the algebraic counter-part of the circumstance that in Zadeh's theory the concepts *belonging to*, or *not belonging to* disappear, as also the principle of « tertium non datur »; the concept of *membership function* is substituted for the one of the characteristic function of sets theory.

In order to better understand the difference between Watanabe's and Zadeh's proposals and the classical theory of probability we observe that one may give other lattice structures to $\mathscr{L}(I)$, or to a suitable subclass of it, by means of any map w that associates with any element

A of a lattice M an element f_A of $\mathscr{L}(I)$. The lattice structure is then induced into $\mathscr{L}(I)$ in the following way:

$$\begin{aligned} (f_A \vee f_B)(x) &=_{\mathrm{Def}} f_{A \cap B}(x) \\ (f_A \wedge f_B)(x) &=_{\mathrm{Def}} f_{A \cup B}(x), \text{ for all } A, B \end{aligned} \tag{4.3}$$

where $\cup$ and $\cap$ are the lattice operations in M.

Any lattice property of M is transmitted to a convenient subset of $\mathscr{L}(I)$ (or to $\mathscr{L}(I)$ if w is onto); if M is complemented, the complementation is induced by

$$\overline{f_A}(x) =_{\mathrm{Def}} f_{\bar{A}}(x)$$

Let us now impose the further requirements:

$$\begin{aligned} f_{A \cup B}(x) &= f_A(x) + f_B(x) \\ f_I(x) &= 1, \text{ for all } A, B, \varepsilon\ M \text{ such that } A \subseteq \bar{B}. \end{aligned} \tag{4.4}$$

If M is a field F of sets, the triplet $\{I, F, f(x)\}$ is a *field* of *probability*. Some random situations may be described by this formalism interpreting, for instance, $f_A(x)$ as the *probability that x belongs to the set A*. In this case, however, contrary to Zadeh's theory, the classical concepts of *belonging to* and the *tertium non datur* remain the same as in the classical sets theory.

If M is a *non-distributive* but *complemented lattice*, $f_A(x)$ represents, if some other specific additional hypotheses are also verified, the kind of generalized probability appearing in the axiomatic formulation of Quantum Mechanics and reproposed by Watanabe in a more general context.

We presume it is possible to draw the following conclusions from the exposition given above even if, due to the different conceptual background underlying the two approaches, it is sometimes difficult to compare them in a parallel way. The main formal difference, as we observed before, between Watanabe's approach and these latter theories is that the former is characterized by the *non-distributivity* of the algebraic structure underlying it, while in the latter, apart from not negligible differences both at the interpretation and the formal stage, a complementation is not generally definable. The two approaches are not in competition and do not substantially overlap for they refer to different classes of situations not well tractable by classical methods. This fundamental difference is clearly shown, as we have seen, by an algebraic analysis; it is, however, worth noting that it is the conceptual difference that is revealed in a very clear way when the schemes are formalized and not conversely.

We therefore think that the two approaches are on the same footing as soon as we look at their possible « right of life »; differences suddenly arise when we consider their present development. We have in fact a very useful realization of modular logics, namely that of the subspaces of a linear space, that allows the development of quantitative evaluations; this realization is taken from the classical version of

Quantum Theory. On the other hand, we have not such good realizations of non-complemented logics, and the formalism of generalized characteristic functions of Zadeh's theory has not been hitherto developed in such a way as to allow the construction of a mathematical calculus usefully applicable in concrete cases (see, however, [29-30]).

Finally, let us note that, as we said at the beginning of the Section, the division of non-standard situations into two classes: one where the act of measurement is both not negligible and uncontrollable and another in which the quantities we are going to study are not well definable, is arbitrary. We then think that it is not unrealistic to take into consideration languages that make use of *a logic both non-distributive and non-complemented* [28]. This proposal is, therefore, not only a way of embracing, in a unique scheme, two different situations but it is mainly an attempt at describing in a more direct way those complex systems in which the two previous aspects are both present and not separable without strong schematizations. We note, however, that this proposal can become a useful quantitative tool only if we are able to find some concrete realizations of the considered logic that we do not have at present.

As regards the connection between the previous languages and the related algorithms we only limit ourselves to saying that there is a large literature on *probabilistic* [21, 31-32] and *non-deterministic* [33] ones and that recently some papers on fuzzy algorithms [34-35] have also appeared. However, it seems to us that algorithms based on the generalized probability used in the axiomatic of Quantum Mechanics or on more general languages have not yet been developed. A final problem we mention here is to see whether and how, as in the case of the theory of probability, these languages may be ultimately reduced to boolean ones even if by means of sophisticated procedures.[1] The question is worthwile for the ties with sets theory or the recognition of the points of departure from it may, no doubt, help in understanding the potentialities and implications of the proposals themselves. A brief discussion on this subject may be found in [28].

5. - CONCLUDING REMARKS.

— We shall try to provide here, though briefly and in an intuitive and incomplete manner, some general ideas arising from the previous informal discussion.

Every model of a system essentially consists of a suitable choice of the parameters considered as meaningful and of some relationships among them. However, it may happen that a description made in terms of certain variables is not the best possible and may in some cases also lose meaning when it prevents a practically feasible quantitative description of the system itself. As pointed out in the second

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[1] This problem has been considered in the case of the non distributive lattice of Quantum Mechanics in [36, 37]; see also [28].

Section the complexity and manageability of the algorithms lead to analyzing the system in separate simpler parts mutually interacting. The previous separation may be done in different ways and the corresponding models may greatly overlap in the sense that the answers provided by their mathematical formalisms to certain classes of questions about the system are the same. The previous remarks clearly apply in their general form only if none of the questions to which different answers are provided by the two theories can be tested experimentally. For instance, there are, as is well known, two alternative interpretations of Quantum Mechanics, which are the « orthodox » and « by hidden variables » ones; no crucial experiment has yet been performed, allowing to reject one of them. Without entering into the controversy[1] we only mention here that the « bone of contention » consists in the claim, made by the hidden variables sustainers, of giving a *complete theory* in contrast with the orthodox assumption that the description by means of the wave-function is the best possible even if it gives rise to an *incomplete theory*. Let us, however, explicity observe that the word « complete » as used before is, of course, referred to the « classical concepts » of dynamical variables; it is with respect to *these* classical concepts that the orthodox interpretation seems to give rise to an incomplete theory; such a theory is not able at the same time to be complete and contain all the significant variables of the classical Physics. The concept of completeness is relative to the fixed set of meaningful variables used in a theory and a theory which is incomplete with respect to a certain set may become complete with respect to another set of suitable variables. This generally changes the interpretation of the the theory without necessarily modifying substantially the mathematical formalism.

In the analysis of complex systems we may be forced to give up some concepts used in previous models; the attempt to adapt them to new situations may not be relevant. As pointed out in the fourth Section, there are systems such that their descriptions in terms of classical variables are too complex to be usefully handled. In these cases an alternative convenient description may be obtained by means of a suitable language very much related to the empirical data even if it does not ultimately make use of Boolean logics. We therefore presume that a careful examination of the algebraic structures which are the mathematical representatives of classes of such complex situations is worthwhile and may, perhaps, give some contributions both in the direction of a conceptual clarification and in the finding of new concrete mathematical techniques.

Finally we want to note a recent paper by Robin Giles [38] of which we had notice only after the completion of this work. In this paper, as consequence of an operational critique to the physical quantities on which a theory is based, the concept of « mixed text », opposed to the « pure text » (or « proposition » in the terminology of Jauch [24] and « question » in Mackey's one [39]) is introduced. The main feature of a mixed text is that, taking as examples classical physics, every physical quantity, defined in strictly operational terms, has to make use of functions assuming values in [0, 1] related to the probability of response of the instruments instead of the characteristic functions of the Borel subsets of the phase space, representing pure texts. This is easily extended in Quantum Theory by considering hermitian operators with eigenvalues in [0, 1] instead of projectors.

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[1] See, for instance, [24] chapter 7 and quoted references.

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ACKNOWLEDGEMENTS. — *It is a pleasure to thank Prof. E. R. Caianiello for many stimulating discussions and Drs. F. Esposito and R. D. Parmentier for their critical comments.*

APPENDIX

A lattice L is a partly ordered set, with respect to a binary relation $\leq$ (to be read « less or equal to »), such that for any pair x and y of elements of L there exist a *least upper bound* (or *join*) and a *greatest lower bound* (or *meet*), denoted respectively by $x \vee y$ and $x \wedge y$.

The binary operations of meet and join satisfy for any x, y, z of L the following properties:

$$\begin{aligned} &x \wedge y = y \wedge x && x \vee y = y \vee x && \text{(Commutative)} \\ &x \wedge (y \wedge z) = (x \wedge y) \wedge z && x \vee (y \vee z) = (x \vee y) \vee z && \text{(Associative)} \\ &x \wedge x = x && x \vee x = x && \text{(Idempotent)} \\ &x \wedge (x \vee y) = x \vee (x \wedge y) = x && && \text{(Absorption)} \end{aligned} \tag{A.1}$$

Furthermore $x \leq y$ is equivalent to each of the relations

$$x \wedge y = x \quad \text{and} \quad x \vee y = y.$$

Conversely any set L with two binary operations satisfying the axioms (A.1) is a lattice.

A lattice is called *modular* when it satisfies the property:

$$x \vee (y \wedge z) = (x \vee y) \wedge z, \quad \text{if} \quad x \leq z; \tag{A.2}$$

a lattice is *distributive* if for all x, y, z of L

$$x \vee (y \wedge z) = (x \vee y) \wedge (x \vee z) \tag{A.3}$$

It is easy to verify that a distributive lattice is modular and that (A.3) implies the distributivity of meet on join:

$$x \wedge (y \vee z) = (x \wedge y) \vee (x \wedge z)$$

Let us now consider a lattice L with a *greatest* element 1 and a *least* element O; L is called *complemented* if for any $x \in L$ there exists at least an element $\bar{x} \in L$ such that:

$$x \vee \bar{x} = 1$$
$$x \wedge \bar{x} = 0.$$

A complemented distributive lattice is called *Boolean lattice* (or algebra); in this case the complement of any element is *unique*.

A lattice with universal bounds (1 and 0) and a unary operation (′) satisfying the properties

$$x \wedge x' = 0 \qquad\qquad x \vee x' = 1$$
$$(x \wedge y)' = x' \vee y' \qquad (x \vee y)' = x' \wedge y' \qquad \textit{De-Morgan laws}$$
$$(x')' = x \qquad \textit{Involution law}$$

is called *ortholattice.* Any ortholattice is complemented and a distributive ortholattice is a Boolean algebra.

An ortholattice which satisfies the property

$$x \vee (x' \wedge y) = y, \qquad \text{if} \qquad x \leq y$$

is called *weakly modular lattice* (or *orthomodular* lattice). Any modular ortholattice is weakly modular but the converse is not generally true.

If in a Boolean algebra B for any pair a and b of elements of B we consider the relations:

$$a \wedge x \leq b$$

and (A.4)

$$a \vee y \geq b,$$

the sets X and Y of elements x and y of B satisfying (A.4) contain a *greatest* and a *least* element respectively, given by

$$b \vee \bar{a}$$

and

$$b \wedge \bar{a}$$

Now we consider a lattice L more general than a Boolean one such that for any pair a and b of elements of L the set X, previously defined, contains a greatest element called *pseudo-complement* of a in b. Such a lattice is called *Brouwerian.*

A lattice for which the set Y contains a least element for any pair a and b of elements of L is called of *Heyting.*

These lattices are both *distributive,* but generally *not-complemented.*

An exhaustive and complete exposition of lattice theory may be found in [40].

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TELESIO - BRUNO - CAMPANELLA

Considerazioni generali introduttive. - *La 'rinascenza', periodo aureo del pensiero umano nelle arti figurative e nelle lettere. Inizio di una fase nuova nella filosofia e nella scienza, come superamento dell'aristotelismo.* B. Telesio, *fondatore dell'Accademia cosentina e « novorum hominum primus ». Nella sua opera « De rerum natura juxta propria principia »* (1586) *si leggono concetti nuovi dal punto di vista epistemologico, quali, fra gli altri, i 'principia agentia' e sopratutto quelli sullo spazio e sul tempo, che anticipano originalmente le moderne teorie relativistiche.* G. Bruno, *geniale continuatore del pensiero telesiano. Di notevole interesse le sue idee sull'Universo e sulla relatività dei fenomeni non solo nell'ambito terrestre, ma anche extraterrestre, e in virtù della quale il peso viene ad assumere un significato relativo.* T. Campanella, *sostenitore convinto della moderna instaurazione scientifica di Galileo, nonchè autore di importanti opere con le quali contribuì a rinnovare la filosofia in Italia e in Europa, iniziando nelle dottrine speculative il metodo sperimentale, e introducendo una nuova classificazione delle scienze nonchè nuovi canoni per indagare sulle leggi della natura.*

CONSIDERAZIONI GENERALI INTRODUTTIVE. — Nessun altro periodo storico manifestò in modo così rilevante come la 'rinascenza' la continuità ideale dello sviluppo del pensiero occidentale, nessun'altra epoca della storia, forse, sentì mai, per bocca dei suoi più significativi autori, tanto intensamente l'esigenza del nuovo, il bisogno di una trasformazione totale e radicale non solo del mondo intellettuale, ma anche del mondo sociale. Il motivo principale si può rilevare nel contrasto netto fra la filosofia medioevale, già verso il tramonto, e il pensiero antico classico, che solo nel corso del sec. XV si venne a conoscere, dopo secoli perduti, nelle sue opere originali. Dallo studio profondo dei testi greci e latini e dalla diffusione della cultura umanistica nacque dapprima in Italia, e poi anche in Germania, in Francia, in Inghilterra e in altre nazioni, il pensiero antiscolastico, caratterizzato, quanto al metodo, dall'aver liberato la deduzione apodittica da presupposti di qualsiasi autorità, e, quanto alla forma, dalla durezza e ineleganza del latino monastico. In questo clima di ammirazione per il *risorto* pensiero antico, di sensibilità edonistica, per così dire, di acutezza di osservazione dell'uomo e della natura, di profondo senso estetico, sorsero capolavori nel campo delle lettere e delle arti. Un interesse sempre crescente si manifestò altresì per le scienze, sopratutto rivolto alla astronomia, alla matematica e alla meccanica. Ciò forse fu dovuto al fatto che astronomia, matematica e meccanica erano già formate da secoli, il che consentiva di passarle criticamente in rassegna alla luce delle nuove concezioni.

Le ardite dottrine di Copernico e di Kepler, che resero di pubblico dominio le prime vedute nettamente opposte a quelle dell'astronomia tolemaica, e le ardimentose imprese di Colombo, di Vespucci e di Magellano ebbero conseguenze rivoluzionarie: la nostra Terra non può più essere considerata il centro dell'Universo, ma un semplice pianeta ruotante attorno al Sole, così come la Luna ruota attorno alla Terra. Le discipline matematiche vantano in Germania i nomi di M. Stiefel (1486-1567), C. Rudolff (?-?), A. Riese (1492-1559), in Italia quelli di S. Dal Ferro (1465-1526), N. Tartaglia (1500-1557), G. Cardano (1501-1576), L. Ferrari (1522-1565), R. Bombelli (?-?), le cui scoperte algebriche saranno destinate ad orientare in gran parte i progressi successivi dell'analisi matematica, e in Francia il nome di F. Viète (1540-1603), l'iniziatore dell'algebra «simbolica».

È in gestazione, dunque, un mondo nuovo del pensiero: l'aristotelismo, che dominava incontrastato in tutti i campi, comincia a subìre i primi efficaci colpi da parte di studiosi e ricercatori europei: A. Alciato (1492-1550) sgombrava la giurisprudenza dalle sottigliezze scolastiche e rinnovava lo studio delle leggi romane; A. Cesalpino (1519-1603) ideava la prima classificazione naturale delle piante e scopriva la circolazione del sangue; Berengario da Carpi (?-1550) dimostrava sperimentalmente che, in opposto a quanto sostenevano gli scolastici con i loro sillogismi, i nervi partono dal cervello e dal midollo spinale e non dal cuore; Leonardo da Vinci (1452-1519), i già citati Tartaglia e Cardano, con i loro discepoli, iniziano decisamente la serie delle argomentazioni contrarie all'aristotelismo nel campo della meccanica. Tutte queste ed altre conquiste, dal punto di vista gnoseologico, si possono considerare come i risultati concreti di quel movimento iniziato da Duns Scoto (1274-1308) in piena filosofia medioevale, movimento basato sulla indipendenza della filosofia dalla teologia. Quanto più nell'epoca umanistica, la filosofia si viene costituendo come sapere autonomo, tanto più il suo compito *precipuo* si viene delineando come conoscenza della natura. A questo scopo concorrono tutte le correnti della rinascenza. La filosofia è scienza dell'uomo, è scienza della natura: ecco il centro motore del nuovo pensiero. Ma lo sviluppo di un pensiero siffatto non potè, nei primi tempi, uscire dagli schemi tradizionali, tutti fondati sul concetto di considerare la Terra, e l'uomo con essa, come centro dell'Universo. Sicchè l'indirizzo naturalistico in tutte le sue forme, le scoperte scientifiche, le invenzioni tecniche, le imprese geografiche e le conseguenti trasformazioni sociali, determinarono l'orientamento del pensiero occidentale, liberato dall'aristotelismo, verso la visione di un mondo nuovo. E questo mondo nuovo verrà annunciato agli uomini dalla magnifica poesia cosmica di Telesio, Bruno e Campanella, tanto feconda di sviluppi nei secoli successivi.

Bernardino Telesio, nato a Cosenza nel 1509, dopo aver compiuto profondi studi umanistici, particolarmente a Milano e a Padova, insegnò a Napoli e in seguito fondò nella sua città natale un'Accademia, tuttora operante.

Il soggiorno a Padova, roccaforte dell'aristotelismo italiano, segnò per il T. una tappa fondamentale del suo pensiero, rappresentata dalla rottura definitiva con l'Aristotele scolastico dell'epoca, si badi bene, e non con l'Aristotele antico, per il quale non è neppure spenta l'ammirazione dei moderni. Alla tendenza dei suoi predecessori e contemporanei, i quali attraverso le sole sottigliezze della logica sillogistica, pretendevano di dedurre 'a priori' tutta la scienza umana, il T. contrappose il metodo 'a posteriori' ossia deduttivo, basato saldamente sull'osservazione e sull'esperienza, e in virtù di questa sua decisa posizione fu appunto celebrato dai suoi contemporanei come il più ardito assertore della necessità per la scienza della natura di una base di esperienze accessibili alla sua costruzione, reintegrando così la natura stessa fuori da ogni concezione magica o teologica, tanto che il celebre filosofo inglese Fr. Bacon (1561-1626) ebbe a definire il T. « novorum hominum primus ». La sua esistenza si chiuse nel 1588.

Il capolavoro del T. reca il titolo « De rerum natura juxta propria principia » (Napoli, 1586), col quale - poichè alla sua epoca « l'Universo, egli dice, era creato a seconda del proprio capriccio e della propria immaginazione » - si propose lo scopo di sostituire alle opinioni soggettive « alcuni principi dedotti dai fatti positivi e reali e dall'osservazione della natura ». Il T. assume come uniche e vere guide i nostri sensi e la natura, la quale, sempre in accordo con se stessa, agendo secondo leggi eterne e necessarie, produce i medesimi risultati per tutti e ci presenta uno spettacolo così regolare e imponente che siamo portati a immaginarla come un essere organizzato vivente, animato. Già nel ricordato titolo, l'opera telesiana richiama alla mente il « De rerum natura » del poeta romano Lucrezio (95-55 a.C.ca.), rivelando così le simpatie del filosofo cosentino per il pensiero naturalistico classico. Per es. egli riprende e sviluppa in profondità il concetto di supporre la materia inerte e soggetta all'azione di due forze opposte, l'una attrattiva e l'altra repulsiva, ossia il caldo, che ha prodotto e domina il cielo, e il freddo, che ha prodotto e domina la Terra. La lotta eterna fra questi due principi antitetici crea l'Universo e tutta l'infinita varietà dei fenomeni. A dire il vero però questa ipotesi del caldo-freddo assunta dal T. è ben poco fondata, giacchè si allontana dal criterio metodico da lui stesso posto. Malgrado ciò, si può dire in complesso che la visione filosofica del T. assuma un ruolo di importanza storica, in quanto al finalismo aristotelico viene a sostituire le azioni prodotte da cause naturali. Infatti ove, per esempio, gli aristotelici parlano di 'materia' e di 'forma', il T. introduce il concetto totalmente nuovo delle 'forze attive' ('principia agentia'). Di grande interesse altresì sono ancora oggi le sue vedute sui concetti fondamentali di spazio e di tempo, cui egli attribuisce una realtà autonoma, per metterli allo stesso livello della materia concreta. Lo spazio non è più da lui considerato una semplice qualità, secondo Aristotele, ma una entità reale autonoma come la materia o « moles », concetto questo molto vicino a quello di massa secondo il Newton. Lo spazio telesiano è il grande ricettacolo di tutti gli esseri, animati e inanimati. Se un qualsiasi corpo abbandona il proprio luogo o viene da esso espulso, il luogo in parola non si allontana affatto nè viene espulso, ma resta il medesimo, divenendo prontamente il ricettacolo di un altro corpo. «...Pertanto il luogo può divenire ricettacolo di qualsiasi essere e se gli esseri che lo occupano si ritirano o vengono espulsi, esso non si ritira nè viene espulso, ma rimane perfettamente identico, pronto ad accogliere tutti gli esseri che si sostituiscono agli altri, ed assume prontamente le dimensioni di tutti gli esseri in esso luogo collocati, e sempre è uguale alle cose in esso collocate, e tuttavia non è identico con alcuna di esse, ma sempre è completamente diverso da tutte le cose » (D.r.nat).

Così lo spazio, sebbene uguale alle cose che lo occupano, non è identico ad alcuna di queste cose. Innanzitutto lo spazio è incorporeo, e, poichè null'altro è che pura attitudine a ricevere la materia, è libero da ogni azione ed operazione. Lo spazio non mostra ai nostri sensi alcuna diversità qualitativa, ma è completamente omogeneo nella sua struttura in ogni direzione, così che non possono esistere i « luoghi naturali » aristotelici. Tutte le parti dello spazio mostrano uguale attitudine a ricevere ogni specie di materia. Il moto dei corpi nello spazio non è causato da qualche differenza qualitativa inerente allo spazio stesso, ma è l'effetto di forze fisiche. Lo spazio come un tutto è immobile, esso è accessibile alla percezione dei nostri sensi, come dimostrano chiaramente gli esperimenti con il vuoto. Basandosi sui fondamenti fisici, il T. attacca la tesi aristotelica contro la possibilità dello spazio vuoto, tesi che si rifiutava ostinatamente di accettare le dimostrazioni date dall'esperienza. Queste ed altre originali vedute del T. mostravano chiaramente lo spirito della sua nuova filosofia, vedute che naturalmente non potevano non avere come risultato la formulazione di una nuova fisica, nettamente antiaristotelica. Il che non mancò di sollevare grande rumore in tutta Italia: si organizzarono ovunque dibattiti, in cui i 'principia agentia' telesiani venivano contrapposti alle 'forme' aristoteliche e gli argomenti filosofici spesso cedevano il campo ad altri ancor più ponderosi, quale per es. la dottrina della 'sostanza-accidente', grande baluardo del pensiero scolastico. Era dunque necessario superare anche questo ostacolo se si voleva fornire la nuova teoria dello spazio di un altro fondamentale criterio epistemologico, oltre quelli telesiani. Compito che si assunse il filosofo dell'Accademia neo-platonica di Firenze Fr. Patrizi (1529-1597). « Che cosa è dunque lo spazio? È sostanza e distanza, è divisione, estensione, è intervallo, è capedo, e intercapedo. Quindi è quantità? accidente? accidente prima della sostanza? e prima del corpo? Archita il vecchio, discepolo di Pitagora, e Archita il giovane, discepolo di Platone, nonchè quegli autori che seguirono l'interpretazione categoriale, non conobbero questo tipo di spazio ». Secondo il Patrizi quindi i concetti di sostanza e di accidente non si applicano al concetto di spazio, poichè essi nient'altro sono che modi di caratterizzare gli oggetti nello spazio. Si deve presupporre che lo spazio sia solo una condizione necessaria per tutto ciò che esiste nello spazio stesso. Inoltre le qualità stesse dipendono dallo spazio, per cui lo spazio stesso non può adattarsi allo schema aristotelico « sostanza-accidente ». Anche per il Patrizi, come per il Telesio, lo spazio non è una qualità, ma una estensione sostanziale sussistente per sè, non inerente ad alcuna cosa, una quantità cioè non appartenente alle categorie aristoteliche, ma anteriore ad esse e loro fonte ed origine. Per il Telesio e il Patrizi lo studio dello spazio dunque deve venire prima dello studio della materia, e, poichè lo spazio condiziona non solo la materia come tale, ma anche le sue qualità, l'investigazione dello spazio è una condizione indispensabile per tutta la scienza della natura. Lo spazio rende possibile non solo la natura, ma la conoscenza della natura, la conoscenza cioè dei fenomeni naturali.

Infine occorre ricordare che molto importanti sono, in rapporto allo sviluppo storico del metodo sperimentale, i tre opuscoli del Telesio pubblicati a Napoli nel 1570: il primo dedicato ai fenomeni atmosferici (devesi al filosofo cosentino se furono impugnati e vittoriosamente confutati molti errori di Aristotele circa le cause delle piogge e circa la dinamica dei venti); il secondo dedicato ai fenomeni marini (ove, fra l'altro, il T. chiarisce le ragioni vere per cui l'acqua di mare è salata); infine il terzo opuscolo dedicato alla teoria dei colori.

GIORDANO BRUNO. — Il rinnovamento filosofico iniziato dal Telesio trova un grande continuatore in Filippo Bruno, nato a Nola nel 1548, il quale, entrando a sedici anni nell'ordine dei domenicani, assunse il nome di Giordano. Per il suo spirito ardente e battagliero fu ben presto costretto ad abbandonare l'abito, iniziando così una vita raminga e tribolata attraverso l'Italia prima e l'Europa poi. Rientrato a Venezia nel 1591, vi viene arrestato nel maggio 1592, per tradimento, essendo stato accusato di eresia. Condotto a Roma, e, dopo sette anni di durissimo carcere, processato, sostenne fieramente le proprie dottrine fondate sul sistema copernicano, rifiutando di abiurarle; degradato e scomunicato, fu consegnato al braccio secolare, con la consueta preghiera di esser punito « senza sparger sangue », che nella interpretazione ormai consacrata da una lunga consuetudine, voleva dire essere arso vivo. Il rogo fu innalzato in Campo dei Fiori a Roma il 17 febbraio 1600!

Si hanno del B. opere varie didattiche e filosofiche, scritte per la più parte in forma dialogica e in una lingua che è un pittoresco miscuglio di latino e di 'volgare'. La sua filosofia, ispirantesi fra l'altro per la sostanza alla filosofia dell'infinito di N. Cusano (1401-1464) e per la forma a Copernico, si può dire racchiusa nei tre opuscoli intitolati « La cena de le ceneri », « De la causa, principio et uno », « De l'infinito universo et mondi », da lui pubblicati a Londra nel 1584. Il nucleo essenziale del suo pensiero è che la Terra non solo si muove attorno al Sole, ma anche il Sole è in movimento; che non v'è nulla di assolutamente immobile; che le stelle procedono nello spazio infinito a varie distanze da noi, sono mobili le une rispetto alle altre ed esse stesse sono centri di sistemi analoghi a quello solare; che l'Universo, essendo infinito, non può fornirci alcun criterio assoluto di posizione; che il nostro sistema solare, cui appartiene la Terra, non è sotto alcun punto di vista il centro dell'Universo, ed infine che esiste una comune anima universale che dà al Tutto il suo essere ed è nel contempo individuale e si trova nel Tutto e in ogni sua parte.

Il B. non lasciò discepoli, ma, sebbene la sua vita raminga e tribolata fosse rimasta quasi oscura, la propagazione della sua dottrina furtiva, la sua influenza segreta, indiretta, inconfessata, dovevano dare ben presto frutti inattesi. Non erano passati infatti ancora quaranta anni dalla sua morte sul rogo quando videro la luce il « Dialogo sopra i due massimi sistemi del mondo » e i « Discorsi intorno a due nuove scienze » di Galileo, per merito dei quali il mondo cessava definitivamente di essere finito per diventare infinito, e quindi di infinite possibilità, ma tuttavia un mondo le cui parti erano matematicamente interdipendenti e le cui fondamenta fisiche stavano venendo gradatamente alla luce. I tre opuscoli del B. quindi segnarono il vero ponte di passaggio dal pensiero medioevale a quello moderno e soprattutto al pensiero scientifico. È importante tener presente che le idee del B. non si fondavano su esperimenti od osservazioni. Il suo contributo fu di natura prettamente filosofica, non fu un metodo o un sistema scientifico. La dottrina della relatività del moto, dello spazio, dello stesso pensiero, divenne nella mente del B. una fede ardente, appassionata. La definizione bruniana dello spazio è brevemente contenuta in queste sue parole: « Uno è il loco generale, uno il spacio immenso che chiamar possiamo liberamente vacuo; in cui sono innumerabili e infiniti globi, come vi è questo, in cui vivemo e vegetamo noi. Cotal spacio lo diciamo infinito, perchè non è ragione, convenienza, possibilità, senso o natura che debba finirlo...Si diffonde per tutto, penetra il tutto ed è continente, contiguo e continuo al tutto, e non lascia vacuo alcuno; eccetto se quello medesimo, come in sito e luogo in cui tutto si muove, e spacio in cui tutto discorre, ti piacesse chiamar vacuo, come molti chiamorno ».

Importante osservare che anche il peso assume per il B. un significato relativo: i corpi devono cadere alla superficie delle stelle e dei pianeti, così come cadono sopra la Terra. In forma grandiosa si veniva così a realizzare sopra una base più larga di osservazione, la visione cosmica degli antichi filosofi ionici, da Anassimandro a Democrito. In modo particolare da Pitagora e da Democrito il B. riprende il concetto della 'monade' o dell''atomo', e con esso il criterio dell'importanza del numero come regolatore dell'ordine cosmico. Allargati così smisuratamente i confini dell'Universo, quale diviene ora la posizione dell'uomo in questo Universo? Nel suo dialogo « Il Copernico » il Leopardi mostra le conseguenze morali della rivoluzione copernicana, per cui l'umanità umiliata sa ormai di essere una quantità trascurabile dell'Universo, e di nessun valore quindi le sue speranze e le sue pene. Tutto il contrario è invece il sentimento cosmico del B. Per lui, come per il Telesio, lo spirito si esalta quando contempla l'infinità dell'Universo, estaticamente vista dal nolano in questi suoi versi

Quindi l'ale sicure a l'aria porgo
Nè temo intoppo di cristallo o vetro,
Ma fendo i cieli e a l'infinito m'ergo.
E mentre dal mio globo a gli altri sorgo,
E per l'eterio campo oltre penetro,
Quel ch'altri lungi vede, lascio al tergo.

TOMMASO CAMPANELLA. — Giovanni Campanella, nato a Stilo Calabro nel 1568, prese il nome di Tommaso quando entrò a quattordici anni nell'ordine domenicano. Ma ben presto anche Tommaso, come Giordano Bruno, per il suo antiaristotelismo e il suo animo fiero e battagliero si attirò le diffidenze dei superiori. Studiò a Napoli, a Roma, a Firenze, a Bologna e a Padova, facendo apprezzare ovunque la sincerità e l'audacia delle sue idee e il vigore della sua eloquenza.

Nel trattato « Philosophia sensibus demonstrata » difende decisamente la filosofia del Telesio ed attacca la filosofia aristotelica e scolastica. Caduto poi in sospetto per le sue idee comunistiche, espresse nella celeberrima opera « Città del Sole » (da lui in seguito tradotta in latino con poche varianti), e così diffuse in quei tempi di grandi fermenti sociali, non meno che per i pronostici che credeva poter trarre dai segni della natura, fu accusato di eresia. In seguito, anche conformemente al suo pensiero politico, rivoluzionario per quei tempi, sognava una forma perfetta di società umana, un'utopistica teocrazia che alla luce della religione naturale reggesse tutti i popoli affratellati in una sola grande famiglia, e, vagheggiando un ordinamento politico fondato su pure astrazioni razionali, tentava di attuarlo nella sua Calabria mediante una congiura (1599) contro il governo spagnolo, per la quale molti furono giustiziati e il frate ribelle fu imprigionato a Napoli, sottoposto ad atroci torture, perchè accusato già di eresia, e infine condannato al carcere perpetuo. Uscì di prigione solo alla morte di Filippo III, dopo ben ventisette anni di sofferenze, durante i quali ebbe la forza di scrivere le sue opere principali; fra queste nel 1622 un'ammirabile « Apologia pro Galilaeo », che fu stampata a Francoforte con l'aiuto di un amico. Si trasferì a Roma, e qui nel 1634 i suoi nemici sollevarono contro di lui un vero tumulto popolare, onde il C. fu costretto a fuggire e a rifugiarsi in Francia, ove il Richelieu lo protesse generosamente. A Parigi ebbe modo di frequentare i circoli cartesiani, in cui s'incontrò col celebre fisico francese P. Gassendi (1592-1655), altro deciso assertore antiaristotelico, ed autore delle « Epistolae tres de motu impresso a motore translato » che sviluppano la concezione del C. di uno spazio infinito e omogeneo. Morì nella capitale francese nel 1639, mentre attendeva all'edizione completa delle sue ottantaquattro opere (che doveva chiamarsi « Instauratio scientiarum », rimasta incompiuta), con le quali contribuì a rinnovare la filosofia in Italia e in Europa, iniziando nelle dottrine speculative il metodo sperimentale, tentando una nuova classificazione delle scienze e ideando regole che aiutassero a scoprire le leggi della natura. Come il Bruno ebbe a subìre l'influenza di Copernico, così il C. ebbe a subìre l'influenza di Galileo. Avendo adottato il programma del Telesio, secondo cui la filosofia naturalistica deve partire dalla esperienza, egli sostenne, giustamente, che le osservazioni e le esperienze di Galileo non potevano esser confutate se non con altre osservazioni ed esperienze.

Ci piace chiudere questi cenni sul C. col ricordare che il grande filosofo calabrese occupa altresì un posto singolare tra i poeti del suo tempo. Benchè piccola la parte della sua produzione poetica rimastaci, pure essa è sufficiente per rivelarci una fortissima tempra lirica, rude ed esuberante, sdegnosa delle raffinatezze mariniane della sua epoca. Nelle belle poesie 'filosofiche', come egli stesso le chiama, composte per la maggior parte in carcere, il C. esprime una propria concezione religiosa, oppure polemizza contro potenti e ipocriti, o profetizza il prossimo rinnovamento del mondo, sempre preoccupato solo di far sentire la sua voce schietta, di persuadere il « buono e il vero in quanto buono ».

Brescia.

I. BOLLETTI - CENSI

TRANSLATION: A. COWAN - BURRINGTON

TELESIO - BRUNO - CAMPANELLA

General introductory outline. - *The Renaissance, golden age of the human mind in the figurative arts and literature. Beginning of a new phase in philosophy and in science, to replace Aristotelianism.* B. Telesio, *founder of the Academy of Cosenza and* « novorum hominum primus ». *In his work* « De rerum natura juxta propria principia » (1586) *are found concepts new from the epistemological point of view: among them, for example, the* « principia agentia » *and in particular those on space and time, which anticipate in an original way modern relativistic theories.* G. Bruno, *man of genius who furthered Telesio's thought. Of outstanding interest are his views on the Universe and on the relativity of phenomena not only in the terrestrial but also in the extraterrestrial sphere, by virtue of which weight comes to acquire a relative significance.* T. Campanella, *fervent upholder of modern science as instituted by Galileo, and author of important works in which he contributed to the renewal of philosophy in Italy and in Europe, initiating the experimental method in the speculative sciences, and introducing a new classification of the sciences as well as new canons for inquiry into the laws of nature.*

GENERAL INTRODUCTORY OUTLINE. — At no other period in history has the continuity of ideas in the development of Western thought been as evident as in the Renaissance, and probably no other period in history has expressed so strongly, through the words of its most important writers, the desire for the new, the need for a total and radical transformation not only of the intellectual but also of the social world. The chief reason is to be found in the clear contrast between Mediaeval philosophy, already in decline, and the thought of classical antiquity, which only in the fifteenth century became known through the original works, which had been lost for centuries. The thorough study of Greek and Latin texts and the diffusion of Humanistic culture gave rise, first in Italy and subsequently also in Germany, France, England and other countries, to anti-scholastic thought, which was characterized, as to method, by the freeing of apodictic deduction from the presuppositions of any authority and, as to form, from the hardness and inelegance of monastic Latin. This climate of admiration for the resurrected thought of antiquity, of hedonistic sensibility, so to speak, of sharp observation of man and nature, of deep aesthetic feeling, brought forth masterpieces in the field of literature and the arts. Also evident was an ever-increasing interest in the sciences, particularly in astronomy, mathematics and mechanics. This may have been due to the fact that astronomy, mathematics and mechanics were already centuries old and could therefore be critically reviewed in the light of the new ideas.

The daring theories of Copernicus and Kepler, which made public property of the first views clearly opposed to those of Ptolemaic astronomy, and the bold ventures of Columbus, Vespucci and Magellan, had revolutionary consequences: our Earth must be considered no longer as the centre of the Universe, but as a simple planet rotating around the Sun, just as the Moon rotates around the Earth. Mathematical studies boast in Germany the names of M. Stiefel (1486-1567), C. Rudolff (?-?), A. Riese (1492-1559), in Italy those of S. Dal Ferro (1465-1526), N. Tartaglia (1500-1557), G. Cardano (1501-1576), L. Ferrari (1522-1565), R. Bombelli (?-?), whose discoveries in algebra were to a great extent to determine the direction of subsequent advances in mathematical analysis, and in France the name of F. Viète (1540-1603), the originator of « symbolic » algebra.

There is thus a new world of thought in the making. Aristotelianism, which has hitherto reigned unchallenged in every field, begins to feel the first effective blows delivered by the scholars and researchers of Europe: A. Alciato (1492-1550) rid jurisprudence of scholastic subtleties and revived the study of Roman law; A. Cesalpino (1519-1603) devised the first natural classification of plants and discovered the circulation of the blood; Berengario da Carpi (?-1550) demonstrated in experiments that, contrary to what the scholastics with their syllogisms maintained, the nerves come from the brain and from the spinal cord, not from the heart; Leonardo da Vinci (1452-1519) and the abovementioned Tartaglia and Cardano, and their disciples, boldly begin the series of argumentations against Aristotelianism in the field of mechanics. All these and other victories, from the gnosiological point of view, can be regarded as the concrete results of the movement initiated by Duns Scotus (1274-1308) in the heyday of Mediaeval philosophy - a movement based on the independence of philosophy from theology. In the age of Humanism, the more firmly philosophy becomes established as autonomous knowledge, the more its principal function comes to be seen as the understanding of nature. This is the common end of every stream of the Renaissance. Philosophy is the science of man, the science of nature: this is the essential drive behind the new thought. But the development of such thought was not at first able to break free from the traditional schemes, based as they all were on the concept of the Earth, and with it Man, as the centre of the Universe. So that naturalistic enquiry in all its forms, and the scientific discoveries, the technical inventions, the geographical ventures and the resulting social changes, determined the orientation of Western thought, freed from Aristotelianism, towards the vision of a new world. And this new world was to be announced to men by the splendid cosmic poetry of Telesio, Bruno and Campanella, the source of such fruitful developments in the centuries to come.

BERNARDINO TELESIO, born in Cosenza in 1509, after thorough studies in the humanities, particularly in Milan and Padua, taught at Naples and subsequently founded in his native city an Academy which is still functioning today.

His stay in Padua, the stronghold of Italian Aristotelianism, marked a fundamental stage in Telesio's thought, represented by the definitive break with the scholastic Aristotle of his times - not, be it noted, with the ancient Aristotle, who is still admired even by the thinkers of modern times. In opposition to the tendency of his predecessors and contemporaries, who claimed that one could deduce *a priori* all human science simply by means of the subtleties of syllogistic logic, Telesio proposed the *a posteriori* or deductive method, based firmly on observation and experiment; and it was thanks to this decided position of his that he was celebrated by his contemporaries as the boldest upholder of the idea that the science of nature must have a basic foundation of accessible experiments, thus reinstating the autonomy of nature itself divorced from any magical or theological concepts. Indeed the famous English philosopher Francis Bacon (1561-1626) described Telesio as « *novorum hominum primus* ». The thinker of Cosenza died in 1588.

Telesio's masterpiece bears the title « *De rerum natura juxta propria principia* » (Naples, 1586); in this work, because in his day « the creation of the

Universe », he says, « is a matter of individual fancy and imagination », he set himself the task of replacing subjective opinions with « certain principles deduced from actual facts and from the observation of nature ». For Telesio the only true guides are our senses and nature, which, being always consistent, and behaving in accordance with eternal and necessary laws, produces the same results for everyone and presents us with a spectacle so regular and so imposing that we are led to imagine it as a living, organized, animate being. Even the title Telesio gave this work calls to mind the « *De rerum natura* » of the Roman poet Lucretius (c.95-55 B.C.), revealing the sympathy of the philosopher of Cosenza with the naturalistic thought of classical times. For example, he takes up and develops in depth the notion of matter as inert and subject to the action of two opposing forces, the one attracting and the other repelling, in other words heat, which produced and now dominates the heavens, and cold, which produced and now dominates the Earth. The eternal struggle between these two antithetical principles creates the Universe and all the infinite variety of natural phenomena. In fact, though, this heat-cold hypothesis adopted by Telesio has little enough foundation, departing as it does from the methodical standard which he himself propounded. Nevertheless it is on the whole true to say that Telesio's philosophic vision plays an important historical role in that it ousts Aristotelian finalism and replaced it with actions produced by natural causes. Thus where the Aristotelians speak, for example, of « matter » and « form », Telesio introduces the completely new concept of « active forces » (« *principia agentia* »). Also of great interest even today are his views on the fundamental concepts of space and time, to which he attributes an autonomous reality, thus placing them on the same level as concrete matter. Telesio considers space no longer as a simple quality, as in the Aristotelian view, but as a real autonomous entity like matter or « *moles* » - a concept which comes very close to Newton's concept of mass. Telesio's space is the great receptacle of all beings, animate and inanimate. If a body leaves its particular place or is expelled from it, the place in question does not move and is not expelled but remains the same, at once becoming the receptacle for another body. « ... Therefore the place can become the receptacle for any being whatsoever and if the beings which occupy it withdraw or are expelled from it, the space does not withdraw and is not expelled, but remains exactly the same, ready to receive whatever beings may replace the others, and at once assumes the dimensions of all beings that occupy the place, and is always equal to the things that occupy it, and yet is identical with none of them but is always completely distinct from all things » (*De rerum natura*).

Thus space, though equal to the things which occupy it, is not identical with any of these things. Space is above all incorporeal, and since it is nothing more than pure aptitude to receive matter, it is free from any action and operation. Space does not reveal to our senses any qualitative difference, but is completely homogeneous in its structure in every direction, so that the Aristotelian «natural places» cannot exist. All parts of space show equal aptitude to receive evey kind of matter. The movement of bodies in space is not caused by any qualitative difference inherent in space itself, but is the effect of physical forces. Space as a whole is motionless, and it is perceptible to our senses, as is clearly shown by experiments with a vacuum. Basing himself on physical grounds, Telesio attacks the Aristotelian thesis which denies the possibility of empty space - a thesis which had obstinately refused to admit the evidence of actual experience. These and other original views of Telesio clearly showed the spirit of his new philosophy; views which could not but lead, of course, to the formulation of a new and distinctly anti-Aristotelian physics. This not unnaturally caused a great stir throughout Italy: all over the place debates were organized in which Telesio's « *principia agentia* » were matched against Aristotelian « forms », and the philosophical arguments often had to retreat before others more ponderous, such as, for example, the doctrine of « substance-accident », a great bastion of scholastic thought. So this obstacle too had to be overcome if the new theory of space was to be provided with another fundamental epistemological standard in addition to those of Telesio. This task was undertaken by a philosopher of the Florentine Neo-Platonic Academy, Fr. Patrizi (1529-1597). « What then is space? It is substance and distance, it is separation, extension, it is interval, it is gap and interstice. Is it then quantity? accident before substance? and before body? Archytas the elder, the disciple of Pythagoras, and Archytas the younger, disciple of Plato, as well as those authors who followed the interpretation according to Aristotelian categories, did not know this type of space ». According to Patrizi, then, the concepts of substance and accident do not apply to the concept of space, since they are nothing more than ways of characterizing objects in space. Space must be considered as simply a necessary condition for all that exists in space. Moreover qualities themselves also depend on space, so that space itself cannot be fitted into the Aristotelian scheme of « substance-accident ». For Patrizi, as for Telesio, space is not a quality but a substantial extension existing by itself, not inherent in anything; a quantity, that is to say, not belonging to the Aristotelian categories, but preceding them and their source and origin. For Telesio and Patrizi the study of space must therefore come before the study of matter and, since space conditions not only matter as such but also its qualities, the investigation of space is an indispensable precondition for the whole science of nature. Space makes possible not only nature, but the knowledge of nature - the knowledge, that is, of natural phenomena.

Finally we must mention the three minor works of Telesio published in Naples in 1570, which play a very important part in the historical development of the experimental method: the first deals with atmospheric phenomena (it was thanks to Telesio that many of Aristotle's erroneous ideas on the causes of rain and on the dynamics of winds were challenged and victoriously refuted); the second deals with marine phenomena (among other things Telesio here elucidates the real reasons for the saltness of sea water); and finally the third is devoted to the theory of colours.

GIORDANO BRUNO. — The philosophical revival initiated by Telesio finds a great continuator in Filippo Bruno. He was born at Nola in 1548, and assumed the name of Giordano when he joined the Dominican order at the age of sixteen. Because of his fiery and combative nature he was soon obliged to quit the habit, and so began a life of harassment and wandering, first through Italy and then through Europe. In 1591 he returned to Venice, and there, under a charge of heresy, in May 1592 he was betrayed and arrested. He was taken to Rome. After seven years of great suffering in prison, he was brought to trial; he proudly upheld his own doctrines, based on the Copernican system, and refused to abjure them; disgraced and excommunicated, he was handed over to the secular arm with the customary request that he be punished « without the shedding of blood » - which according to the time-honoured interpretation meant being burnt alive. The stake was erected in Campo dei Fiori in Rome on the 17th February 1600.

Bruno left various didactic and philosophical works, written for the most part in dialogue form and in a language which is a picturesque mixture of Latin and the vernacular. His philosophy, in part inspired as to substance by the philosophy of the infinite of Nicholas of Cusa (1401-1464), and as to form by Copernicus, is more or less contained in the three small works entitled « *La Cena de le Ceneri* » (« Ash Wednesday »), « *De la causa, principio et uno* » (« Concerning the Cause, Principle and One »), and « *De l'infinito universo et mondi* » (« On the Infinite Universe and Worlds »), published by him in London in 1584. The essential nucleus of his thought is that not only does the Earth move round the Sun, but the Sun too is in motion; that nothing is absolutely motionless; that the stars proceed through infinite space at varying distances from us, that they move in relation to each other, and are themselves the centres of systems similar to our own solar system; that the Universe, being infinite, cannot provide us with any absolute point of reference for determining position; that our solar system, to which the Earth belongs, is not from any point of view the centre of the Universe; and finally that there is a common universal soul which gives the whole its being and is at the same time individual, and is found in the whole and in its every part.

Bruno left no disciples, but even though his harassed and wandering life had remained more or less obscure, the surreptitious propagation of his theory, and his secret, indirect, unconfessed influence, were very soon to bear unexpected fruit: for less than forty years after his death at the stake appeared Galileo's « *Dialogo sopra i due massimi sistemi del mondo* » (« Dialogue on the Great World Systems ») and the « *Discorsi intorno a due nuove scienze* », thanks to which the world finally ceased to be finite and became infinite, and therefore of infinite possibilities, though nevertheless a world whose parts were mathematically interdependent and whose physical foundations were gradually coming to light. Bruno's three little works thus really mark the passage from mediaeval to modern, above all to scientific, thought. It is important to bear in mind that Bruno's ideas were not based on experiment or observation. His contribution was of a purely philosophical nature, it was not a method or a scientific system. The theory of the relativity of motion, of space, of thought itself, became in the mind of Bruno a burning, passionate faith. His definition of space is summed up in these words of his: « General place is one, and one the immense space that we may loosely call a vacuum; in which are innumerable and infinite globes, as there is this in which we ourselves live and vegetate. This space we call infinite, because it is not reasonable, nor fitting, nor possible, nor sensible, nor natural that it should have an end... It is everywhere diffused, it permeates all and is constant, continuous and contiguous with all, and it leaves no vacuum: unless it please you to call a vacuum, as many have done, that which is the site and place in which everything moves, the space through which all things run ».

It is important to note that for Bruno weight too takes on a relative significance: bodies must fall on the surface of stars and planets just as they fall onto the Earth. Thus it was that in a grandiose form the cosmic vision of the ancient Ionic philosophers, from Anaximander to Democritus, came to be realised on a broader basis of observation. In particular Bruno takes from Pythagoras and Democritus the concept of the « monad » or the « atom », and with it the principle of the importance of number as the regulator of cosmic order. With the confines of the Universe thus immeasurably extended, what is to be the position of Man in this Universe? In his dialogue « *Il Copernico* » Leopardi shows the moral consequences of the Copernican revolution: in his view the result is that Man is humbled and realises that he is but a negligible part of the Universe, and that his hopes and his sufferings are therefore of no value. Bruno's cosmic feelings, however, are quite the opposite: for him, as for Telesio, the spirit is exalted when it contemplates the infinitude of the Universe, his ecstatic vision of which he expresses in these lines:

So I spread my sure wings to the air
Fearing no barrier of crystal or glass
But cleave the heavens and rise towards the infinite.
And as I fly up from my globe towards the others,
And penetrate further into the etherial field,
That which others see from afar, I leave behind me.

TOMMASO CAMPANELLA. — Giovanni Campanella was born at Stilo in Calabria in 1568, and took the name of Tommaso when he entered the Dominican order at the age of fourteen. But very soon Tommaso too, like Giordano Bruno, came to be distrusted by his superiors on account of his anti-Aristotelianism and his proud and combative spirit. He studied at Naples, Rome, Florence, Bologna and Padua, earning appreciation everywhere for the sincerity and the boldness of his ideas and the power of his eloquence.

In his treatise « *Philosophia sensibus demonstrata* » he stoutly defends the philosophy of Telesio and attacks Aristotelian and scholastic philosophy. Then he fell under suspicion, partly because of his communistic ideas which he expressed in his famous work the « *Città del Sole* » (« City of the Sun ») (later translated by him, with a few variants, into Latin), and which were widely circulated in those days of great social ferment; and also because of the predictions he believed he was able to make from the signs of nature, he was accused of heresy. Subsequently, also in conformity with his political thought, which was revolutionary for the time, he dreamed up a perfect model of human society, a Utopian theocracy which in the light of natural religion would govern all peoples as brothers in one great family. This vision of a political order established on a basis of pure abstract reason he attempted to put into practice in his native Calabria, by means of a plot (in 1599) against the Spanish government; this resulted in many executions, and the rebellious friar himself was imprisoned in Naples, subjected to appalling tortures, since he was already under the charge of heresy, and finally condemned to perpetual imprisonment. He was only released from prison on the death of Philip III, after twenty seven years of suffering, during which time, however, he had the strength to write his chief works, among them an admirable « *Apologia pro Galilaeo* », (1622), which was printed in Frankfurt with the help of a friend. He moved to Rome, and there in 1634 his enemies stirred up a real popular riot against him, so that he was forced to flee and take refuge in France, where he was generously protected by Richelieu. In Paris he was able to frequent Cartesian circles, where he met the famous French physicist P. Gassendi (1592-1655), another resolute champion of anti-Aristotelian thinking, author of the « *Epistolae tres de motu impresso a motore translato* » which develops Campanella's concept of infinite and homogeneous space. Campanella died in Paris in 1639, while he was seeing to a complete edition of his eighty four works (which was to be called « *In-*

stauratio scientiarum », and remained unfinished); with these works he contributed to the revival of philosophy in Italy and in Europe, introducing the experimental method into the speculative sciences, attempting a new classification of the sciences, and devising rules to help in discovering the laws of nature. Just as Bruno came under the influence of Copernicus, so Campanella came under the influence of Galileo. Having adopted Telesio's scheme according to which natural philosophy must be based on experience, he maintained, rightly, that Galileo's observations and experiments could not be refuted except by other observations and experiments.

We would like to conclude these notes on Campanella with the reminder that the great Calabrian philosopher also occupies a unique position among the poets of his age. Although only a small part of his poetic output has come down to us, it is still enough to reveal a most powerful lyric style, rough and exuberant, scornful of the refinements of contemporary *marinismo*. In the beautiful « philosophical » poems, as he himself calls them, composed for the most part in prison, Campanella expresses a religious conception of his own, or polemicises against the powerful and hypocrites, or prophesies the coming renewal of the world, always concerned only to make his voice heard plain and clear, and to convince of « the good, and the true that is the good ».

Brescia.

I. Bolletti-Censi

CRONACHE - REPORTS

Un Convegno di studi sul pensiero di F. Enriques

Nel centenario della nascita di Federigo Enriques, la figura e l'opera dell'illustre matematico e filosofo sono state rievocate nel corso di un convegno promosso dall'Accademia dei Lincei. I lavori del convegno, ai quali hanno partecipato numerosi studiosi italiani e stranieri, si sono svolti a Pisa, Bologna e Roma fra il 7 e il 12 ottobre.

Rinunciamo, qui, a fare una cronaca minuziosa e puntuale del dibattito - anche se ciascuna delle relazioni presentate nel corso del convegno meriterebbe ben più che un cenno sommario - per esporre alcune delle conclusioni a parer nostro più significative emerse attraverso il succedersi degli interventi. Il loro senso globale equivale ad una rivalutazione del pensiero dell'Enriques, ed all'accostamento di esso, per vie non marginali, a concezioni oggi vive ed attive.

In primo luogo si dovrà ricordare il riconoscimento - presente in tutta l'opera dell'Enriques - dell'unità del pensiero scientifico, del quale le singole discipline sono, in qualche modo, le articolazioni. Tanto significava, evidentemente, opporsi alla polverizzazione del sapere in una miriade di discipline specialistiche, ciascuna delle quali incurante di tutto ciò che non fosse direttamente connesso con il suo campo rigidamente delimitato; significava, soprattutto, ricondurre le scienze, o, piuttosto il pensiero scientifico, nell'ambito della cultura dal quale tendeva ad escluderlo il neo-idealismo in quegli anni imperante in Italia.

Allo stesso modo, è stato posto in rilievo il ruolo che, nel quadro di una cultura rinnovata, Federigo Enriques assegnava alla storia della scienza, che non poteva essere intesa come erudita raccolta di notizie, ma come attività razionale - essa stessa scientifica - intesa a ordinare i dati secondo un preciso disegno, a interpretarli, a spiegarli. La storia della scienza finiva così per identificarsi con la storia della filosofia e della cultura, nè poteva rinunciare ad esaminare l'evolversi delle conoscenze scientifiche in stretto rapporto con le civiltà nelle quali esse si realizzavano.

Sulla base di questi presupposti Enriques svolse nei primi decenni del secolo la sua opera di studioso e di docente, opera che spesso ebbe il carattere di una battaglia non priva di asprezza: questa stessa Rivista nacque, per iniziativa di Enriques, per affermare questi principi, la cui validità si va ancor oggi scoprendo. E tuttavia (lo hanno ricordato, nel corso del convegno, tanto il Prof. Ludovico Geymonat quanto il Prof. Lucio Lombardo Radice) il pensiero di Federigo Enriques non trovò allora, in Italia, nè la risonanza nè il gran numero di seguaci che la sua vitalità avrebbero dovuto guadagnargli: rimase, per così dire, sospeso in terra ostile.

Perchè questo accadde? Diverse spiegazioni di questa incomprensione sono state tentate; tutte, in ogni caso, possono essere ricondotte alla constatazione che il pensiero di Enriques si muoveva su un piano affatto diverso da quello (rigidamente idealistico) sul quale la cultura italiana si soffermava in quegli anni. La paralisi, la segregazione e l'avvilimento che il fascismo (esso stesso, del resto, legato ad equivoche matrici idealistiche) fece pesare sul nostro paese, fecero il resto, condannando ad una temporanea infecondità fermenti che soltanto in seguito, in un clima più limpido e libero, avrebbero dato frutto.

A Study Meeting on the Ideas of F. Enriques

On the occasion of the centenary of the birth of Federigo Enriques, the personality and work of the eminent mathematician and philosopher were recalled during the course of a Meeting promoted by the Accademia dei Lincei. The activities of the Meeting, which was attended by numerous Italian and foreign scholars, took place in Pisa, Bologna and Rome between 7th and 12th October.

It is not our intention here to give a detailed and exact report of the debate - even if each of the papers read during the course of the meeting would deserve far more than a brief mention. We prefer to give some of the conclusions that we consider to be the most significant among those that emerged during the course of the various activities. Taken as a whole, they amount to a reassessment of the ideas of Enriques and a confrontation, by no means marginal, between them and concepts that are still alive and relevant today.

First of all, mention must be made of the recognition - present in all Enriques' work - of the unity of scientific thought, of which the individual disciplines form, in a certain way, the separate joints. This obviously meant opposition to the pulverising of knowledge into a myriad specialistic disciplines, each of them heedless of anything that was not directly connected with its rigidly confined field; above all, it meant restoring the sciences - or rather, scientific thought - to the sphere of culture from which it tended to be excluded by the neo-idealism that was dominant in Italy at that time.

At the same time, attention was drawn to the role which, within the framework of a renewed culture, Federigo Enriques assigned to the history of science, which could not be understood as an erudite collection of facts but as a rational activity - itself scientific - aimed at arranging the various data according to a precise pattern, interpreting them and explaining them. Thus

the history of science ended by being identified with the history of philosophy and culture; nor could it relinquish the task of examining the evolution of scientific knowledge in close relationship with the civilisations in which it came into being.

It was on the basis of these assumptions that Enriques, during the early decades of the century, carried out his work as a scholar and a teacher - a work that often assumed the character of a battle not devoid of asperity. This selfsame Review was created, through Enriques' initiative, in order to assert these principles, the validity of which is still being discovered today. Nevertheless (as was mentioned in the course of the Meeting both by Prof. Ludovico Geymonat and by Prof. Lucio Lombardo Radice) Federigo Enriques' ideas found in Italy, at that time, neither the response nor the great number of followers that their vitality should have gained for him: they remained, so to speak, suspended in a hostile land.

Why did this happen? Various different explanations of this incomprehension have been put forward; however, they can all be brought down to the confirmation that Enriques' thought moved on a plane that was entirely different from the (rigidly idealistic) plane on which Italian culture rested at that time. The paralysis, segregation and dejection inflicted upon Italy by Fascism (itself, moreover, associated with doubtful idealistic moulds) did the rest by condemning to a temporary unfruitfulness ferments that only later, in a clearer and freer climate, were to bear fruit.

Il 51mo congresso della SIPS

Nei primi giorni dello scorso mese di ottobre si è tenuto a Pugnochiuso nel Gargano il cinquantunesimo congresso della Società Italiana per il Progresso delle Scienze. I temi dei lavori erano due, entrambi strettamente connessi dall'elevatissimo grado di sviluppo scientifico e tecnologico raggiunto oggi dalla società: « Le alte velocità, lo spazio, il tempo e l'uomo » e « L'aggiornamento degli insegnanti di discipline scientifiche ».

Argomento di una parte delle relazioni presentate nel corso del congresso sono stati i più avanzati strumenti e le più ardite realizzazioni che la scienza e la tecnica mettono oggi a disposizione dell'uomo: si è parlato così del futuro della ricerca spaziale, delle prospettive di evoluzione dei trasporti aerei iperveloci ed aerospaziali, del futuro dell'automobile nelle nuove dimensioni dell'habitat, e quindi delle varie applicazioni dei satelliti artificiali, dei calcolatori, dei nuovi mezzi di trasporto, dei moderni strumenti di informazione. È stato, in particolare, posto in rilievo il fatto che i frutti del progresso scientifico e tecnologico devono essere - e, in parte almeno, sono - posti al servizio dell'uomo. Ad esempio: i satelliti artificiali vengono attualmente impiegati per lo studio della Terra e dei fenomeni meteorologici; o ancora: la tecnica dell'informazione consente numerose applicazioni in delicati settori ed offre prospettive senza dubbio incoraggianti; l'impiego di tali nuovi sistemi, infatti, renderà presto possibile l'istituzione di « banche di dati » attraverso le quali si disporrà di informazioni preziose riguardanti la sanità pubblica, si potrà attuare una più equa politica fiscale, o si potranno utilizzare più pienamente le risorse naturali di una regione.

Allo sviluppo della scienza e, soprattutto, alle sue sempre più affinate applicazioni tecnologiche sono però connessi anche pericoli non trascurabili: alcuni dei relatori hanno ricordato le trasformazioni indotte nell'ambiente naturale con sempre maggior frequenza e in misura sempre più ampia, in parte per l'accrescersi delle capacità di manipolazione (creazione di nuove stirpi di piante ed animali, progressiva distruzione di altre), in parte per gli inquinamenti determinati dall'espandersi dei complessi industriali. Di qui la necessità, posta in rilievo dal Prof. Giacomini, di una più vigile consapevolezza delle responsabilità che il progresso tecnologico comporta, proprio perchè esso offre all'uomo la possibilità di alterare, anche in modo irreparabile, l'ambiente naturale.

Questa indispensabile consapevolezza, è stato osservato dai relatori intervenuti nel dibattito sui problemi dell'aggiornamento degli insegnanti di discipline scientifiche, può derivare soltanto dall'educazione. La crisi che attualmente la scuola attraversa, secondo alcuni degli intervenuti, ha all'origine il mancato adeguamento dei concetti fondamentali sui quali è impostata l'educazione, alla nuova realtà proposta dall'evolversi della scienza e della tecnica. La scuola è invece rimasta ancorata a concezioni valide in passato, in un mondo stazionario o in lento cambiamento: ad esse è legata la tendenza alla divisione della cultura in compartimenti stagni, che preclude la visione totale dei problemi. Ciò assume particolare gravità nelle scuole di livello superiore, perchè da esse escono gli uomini destinati ad assumere le maggiori responsabilità nei confronti della società, alla quale trasmettono quindi le impostazioni culturali assorbite nella scuola.

È perciò necessario che si intensifichi l'azione intesa a trasformare l'educazione, specie quella universitaria e post-universitaria, limitandone il carattere specialistico, ampliandone il contenuto, ponendo a fondamento di essa la stretta connessione fra i diversi ordini di problemi, fra le diverse discipline. Soltanto in tal modo l'insegnamento delle scienze può assumere una fondamentale funzione formativa, e non puramente informativa; può divenire cultura; può, in sostanza, acuire le capacità critiche e farsi in tal modo strumento di consapevole difesa dai pericoli legati a un impiego arbitrario degli strumenti offerti all'uomo dalla tecnica.

Non si potrà non osservare però, a questo riguardo, che tali pericoli, che vanno divenendo di giorno in giorno più gravi, non potranno essere rimossi se la possibilità di valersi degli strumenti della scienza e della tecnica e la capacità di valutarne gli effetti non verranno assegnate a tutti gli uomini, in una società meno rotta da dislivelli e da barriere.

The 51st SIPS Congress

At the beginning of last October, there was held at Pugnochiuso, in the Gargano district, the fifty-first congress of the Italian Society for the Progress of the Sciences. The papers were all related to two themes, both of them closely connected with the extremely high level of scientific and technological development attained by society today: « High speeds, space, time and man » and « Bringing teachers of scientific disciplines up to date ».

Part of the papers presented during the course of the congress were devoted to the subject of the most advanced instruments and the boldest achievements that science and technology today place at man's disposal: for example, the future of space research, the prospects of the evolution of super-fast and air-space air-transport, the future of the motor-car in the new dimensions of

man's habitat, and the various applications of artificial satellites, computers, new means of transport and modern instruments of information. Particular attention was drawn to the fact that the results of scientific and technological progress must be - and (at least partly) are - placed at the service of man. For example: artificial satellites are at present employed for the study of the Earth and of meteorological phenomena; or again: information technique allows numerous applications in delicate sectors and offers prospects that are undoubtedly encouraging; the use of these new systems, in fact, will soon make possible the institution of « data banks » by means of which valuable information will be available regarding public health; it will be possible to put into practice a fairer tax policy and achieve a fuller utilisation of the natural resources of a region.

However, the development of science and, above all, its increasingly refined technological applications are also accompanied by dangers that are by no means negligible: some of the speakers mentioned the transformations brought about in the natural environment with ever greater frequency and to an ever wider extent, partly through the growth of the possibilities of manipulation (the creation of new strains of plants and animals and the progressive destruction of others), partly through pollution caused by the expansion of industrial complexes. Hence the necessity, emphasised by Prof. Giacomini, for a more alert awareness of the responsibilities involved in technological progress, for the very reason that it offers man the possibility of impairing his natural environment, even to an irreparable extent.

This indispensable awareness was mentioned by the speakers that took part in the debate on the problems involved in bringing teachers of scientific disciplines up to date. They stated that it can derive solely from education, and some of them asserted that the crisis through which the school is passing at present originates from the failure of the fundamental concepts on which education is established to adapt to the new reality proposed by the evolution of science and technology. The school, on the contrary, has remained anchored to concepts that were valid in the past, in a world that is either stationary or in a state of slow change; they are bound up with a tendency towards the division of culture into watertight compartments - a division that precludes an overall view of the problems involved. This becomes particularly serious in the advanced schools, since it is from these schools that emerge the men destined to assume the greatest responsibilities with regard to society, to which they then transmit the cultural background and attitudes absorbed in the school.

Intensified action is therefore necessary, aimed at a transformation of education, especially university and post-university education, by limiting its specialistic nature, broadening its content and basing it upon the close connection between the different orders of problem and between the different disciplines. Only in this way can the teaching of the sciences assume a fundamental formative function, and not a merely informative one; only in this way can it become culture: in short, sharpen the critical capacities and in this way become an instrument of conscious defence against the dangers involved in an arbitrary use of the instruments offered to man by technology.

However, in this respect, it cannot but be observed that these dangers, which are becoming more serious day by day, will never be removed if the possibility of making use of the instruments of science and technology and the capacity of assessing their effects are not assigned to all men, in a society less broken up by barriers and differences in level.

Un dibattito a Roma sulle teorie di Darwin

Per iniziativa dell'Accademia Nazionale dei Lincei si è svolto a Roma, in occasione del primo centenario della pubblicazione dell'opera di Charles Darwin « L'origine dell'uomo », un colloquio internazionale sui problemi dell'evoluzionismo quali appaiono oggi, alla luce delle più recenti scoperte scientifiche.

Nel corso del 'colloquio' sono state ricordate le accoglienze discordanti che la rivoluzionaria teoria darwiniana trovò presso gli uomini di cultura dell'epoca: basterà dire, qui, che in Italia, mentre scienziati di impostazione positivistica come il Lombroso o il Mantegazza espressero con calore il loro consenso, altri (Niccolò Tommaseo prima d'ogni altro) respinsero la dottrina evoluzionistica con argomentazioni che non possono oggi non apparire ridevoli o volgarmente superficiali: lo stabilire un qualunque nesso fra l'uomo e la scimmia parve scandaloso oltraggio alla specie umana ed alle sue conquiste. Ma nei cent'anni trascorsi da allora, quel che per taluno fu motivo di scandalo è divenuto realtà ovvia e largamente verificata.

Così il Prof. Dobzhansky della Columbia University ha potuto ricordare le tappe della evoluzione dell'uomo attraverso quattro milioni di anni, dagli ominidi africani - che già si distinguevano per la stazione eretta, un sia pur rozzo linguaggio simbolico ed una primitiva organizzazione sociale - all'uomo di Neanderthal che centomila anni fa già svolgeva attività agricole ed artigianali. L'illustre studioso ha però riconosciuto che, se è assodata la stretta affinità che lega la specie umana alle altre specie animali (affinità che è stata dimostrata anche alla luce dei più recenti studi di biochimica), resta ancora da scoprire il meccanismo che ha portato l'uomo a distinguersi dalle altre specie e, unico fra gli esseri viventi, ad avviarsi, sia pure attraverso un lentissimo processo evolutivo, verso livelli sempre più alti di affinamento intellettuale.

Il convegno di Roma non ha tenuto conto soltanto degli aspetti biologici dell'evoluzione della specie umana, ma si è soffermato a lungo anche su quelli più specificamente culturali. Questi fenomeni, che sono quelli che caratterizzano la nostra civiltà, non possono essere spiegati sulla base dei soli principi della selezione naturale: questo rilievo è stato fatto da un illustre biologo e studioso di genetica, il Prof. Giuseppe Montalenti dell'Università di Roma, il quale ha tenuto a ricordare che l'evoluzione della specie umana non è stata determinata soltanto da cause genetiche, ma anche da fattori estranei agli ordinari meccanismi biologici, e principalmente dalla capacità di autodeterminarsi insita nell'uomo. Sarebbe perciò grave errore il pretendere di ricondurre alle cause biologiche e genetiche del processo evolutivo anche il crescere e l'affinarsi della civiltà e della cultura.

Infine il Prof. Balout, dell'Institut de Paléontologie Humaine di Parigi, ha osservato che gli strumenti di pietra scheggiata risalenti al paleolitico superiore testimoniano di un sorprendente livello intellettivo dei loro antichissimi artefici, i quali erano evidentemente in grado di indirizzare le loro pur elementari operazioni manuali ad un disegno preciso, e di realizzare oggetti dei quali avevano già inizialmente un'idea esatta e che rispondevano a funzioni predeterminate. Il Prof. Balout ha posto in rilievo l'importanza fondamentale che questa capacità ebbe nel quadro dello sviluppo della civiltà, poichè consentì il graduale affinamento così degli strumenti come della capacità di usarli, dai primi stadi di un rozzo artigianato ai livelli più elevati di una tecnologia che è spesso, ancora oggi, motivo di sorpresa per chi pure viva a contatto con essa.

A Debate in Rome on Darwin's Theories

Through the initiative of the Accademia Nazionale dei Lincei, and on the occasion of the first centenary of the publication of Charles Darwin's work « The Origin of Man », an international discussion was held in Rome, devoted to the problems of evolutionism as they appear today, in the light of the most recent scientific discoveries.

In the course of the discussion, mention was made of the varied reception that Darwin's revolutionary theory met with among men of learning at that time. It will suffice to say here that, in Italy, while scientists of a positivistic attitude, such as Lombroso and Mantegazza, heatedly expressed their agreement, others (the foremost of which was Niccolò Tommaseo) rejected the evolutionistic doctrine with arguments that can only appear laughable and superficial today: the establishing of any connection between man and the ape appeared a scandalous outrage to the human species and its achievements. But in the hundred years that have elapsed since then, what was a motive of scandal for some has become an obvious and widely proved reality.

Thus Prof. Dobzhansky of Columbia University was able to recall the stages in man's evolution over a period of four million years, from the African hominids - who were already distinguished by their upright posture, an albeit rudimentary symbolic language and a primitive social organisation - to Neanderthal man, who, a hundred thousand years ago, was already engaged in agricultural and artisan activities. However, the eminent scholar also acknowledged that, even if the close affinity that binds the human species to the other animal

species is firmly established - an affinity that has also been demonstrated in the light of the latest studies in biochemistry - it still remains to discover the mechanism by which man has come to be distinguished from the other species and, alone among living creatures, has progressed, albeit through a very slow evolutional process, towards ever higher levels of intellectual refinement.

The discussion was not confined solely to the biological aspects of the evolution of the human species; it also dwelt at length upon the more specifically cultural aspects. These phenomena, which are those that characterise our civilisation, cannot be explained on the basis of the principles of natural selection alone; this was pointed out by an eminent biologist and genetics expert, Prof. Giuseppe Montalenti of Rome University, who drew attention to the fact that the evolution of the human species has not been determined solely by genetic causes but also by factors extraneous to the ordinary biological mechanisms, mainly by the capacity of self-determination that is inborn in man. It would therefore be a serious mistake to claim that also the growth and refinement of civilisation and culture are to be attributed to the biological and genetic causes of the evolutional process.

In conclusion, Prof. Balout, of the Institut de Paléontologie Humaine in Paris, observed that the chipped stone tools dating back to the later paleolithic age are evidence of a surprising intellectual level among their ancient inventors, who were evidently able to direct their albeit elementary manual operations according to a precise plan and to construct objects that answered predetermined functions and of which they already had a precise idea, right from the start. Prof. Balout emphasised the fundamental importance that this capacity had in the pattern of the development of civilisation, since it permitted the gradual refinement both of the tools themselves and of the ability to use them, from the early stages of a rough craftsmanship to the highest levels of a technology that, even today, is often a motive of surprise even for those living in contact with it.

Un Convegno sulla difesa dell'ambiente marino

La progressiva degradazione dell'ambiente marino lungo le coste italiane dovuta al crescente grado di inquinamento delle acque, e la necessità di opporre misure adeguate a questo preoccupante processo, sono stati gli argomenti di un convegno sui parchi marini e sulla difesa delle risorse dei mari, svoltosi a Napoli per iniziativa della Società Italiana di Biologia Marina e di altri enti ed associazioni.

A conclusione dei lavori è stata approvata all'unanimità una mozione nella quale si afferma: « La difesa del mare deve essere realizzata attraverso l'azione coordinata di tutte le forze culturali, politiche ed economiche interessate. Essa deve essere attuata in un quadro di pianificazione globale delle risorse e del territorio. I poteri pubblici devono essere sollecitati comunque perchè intervengano con gli strumenti giuridici più adatti, caso per caso, a loro disposizione: provvedimenti amministrativi, piani urbanistici, leggi. Il collegamento dei vari interessi in grado di agire per la difesa del mare deve essere attuato da un comitato che costituisca il punto di riferimento per l'espressione e il rafforzamento di tali interessi e per il coordinamento delle loro iniziative ».

In un'altra mozione si denuncia « la degradazione dell'ambiente marino che è chiaramente conseguente all'inquinamento determinato dalla pressione antropica nel continente e sulle coste con il relativo scarico in mare di rifiuti di ogni genere e in particolar modo degli idrocarburi e dei detersivi sempre più diffusi nell'attuale società ». I partecipanti al convegno hanno anche sottolineato, nella stessa mozione, « i gravi danni causati dalle discariche di materiale di risulta lungo le coste italiane, che, alterando il ritmo di sedimentazione, impediscono il naturale evolversi delle praterie a fanerogame e riducono notevolmente i substrati duri con conseguenze irreparabili sulle biocenosi che in essi vivono ». Per tali motivi hanno invitato gli organismi così parlamentari come amministrativi che ne hanno il compito ed il potere, ad applicare rigorosamente le leggi esistenti in materia.

Un'altra causa di danni gravissimi si è identificata nell'esercizio irrazionale e indiscriminato della pesca, al quale si addebita lo stato di grave depauperamento biologico in cui versano i fondali della platea continentale italiana. Un possibile rimedio è stato indicato nell'istituzione, in ciascun compartimento marittimo, di zone di tutela biologica, del resto previste dalle norme vigenti.

Un altro pericolo è stato ravvisato nell'uso, ormai invalso, di affon-

dare in mare carcasse di macchine, nella presunzione che ciò possa essere di qualche utilità ai fini del ripopolamento delle acque. I partecipanti al convegno hanno chiesto che tale pratica venga abbandonata fino a che non ne sia stata accertata la reale efficacia e fino a che approfondite ricerche non abbiano assicurato che le carcasse affondate in mare, diano assoluta garanzia di non contribuire all'inquinamento delle zone costiere, già fin troppo soggette ad accogliere rifiuti di ogni genere.

Infine, in un'altra mozione, i partecipanti al convegno, dopo aver denunciato i danni causati tanto dal sovrasfruttamento quanto dagli inquinamenti, tipici di una società industriale consumistica, sollecitano la creazione di parchi marini, nei quali ravvisano « un mezzo efficace, sia per la protezione di ambienti e biocenosi di particolare interesse, anche ai fini di un benefico irradiamento di determinate specie nelle zone limitrofe, sia per finalità di ricerche scientifiche ed applicative, sia per scopi didattici ed educativi ».

A Meeting on the Defence of the Marine Environment

The progressive degradation of the marine environment along the coasts of Italy, due to the increasing degree of pollution of the waters, and the necessity of opposing this worrying process by means of suitable measures were the subjects of a Meeting on sea areas and the protection of their resources, held in Naples through the initiative of the Italian Society of Marine Biology and of other organisations and associations.

At the conclusion of the activities of the Meeting, there was unanimous approval of a motion in which it was stated : « Defence of the sea must be achieved through the co-ordinated action of all the cultural, political and economic forces concerned. It must be carried out within a framework of the overall planning of the resources and of the territory. In any case, the public powers must be urged to intervene, case by case, with the most appropriate legal means at their disposal: administrative measures, twon-planning schemes and laws. The various interests capable of acting for the defence of the sea must be combined and connected by a committee constituting the reference point for the expression and development of these interests and for the co-ordination of their particular undertakings ».

Another motion denounced « the degradation of the marine environment, which is clearly the result of the pollution caused by the anthropic pressure in the continent and on the coasts with the relative discharge into the sea of waste products of every kind and particularly of hydrocarbons and detergents which are becoming increasingly widespread in present-day society ». In the same motion, those taking part in the Meeting also emphasised « the serious damage caused by the dumping of waste material along the coasts of Italy, which, by altering the rate of sedimentation, prevent the natural evolution of the aphanerogamous beds and considerably reduce the hard substrata, with irreparable consequences to the biocoenoses that live in them ». For these reasons they urged both parliamentary and administrative organisms, entrusted with such a task and the power to carry it out, to see to the strict application of the existing laws on the matter.

Another cause of very serious harm is to be ascribed to irrational and indiscriminate fishing, which is responsible for the state of serious biological impoverishment to be found at the depths of the Italian continental shelf. A possible remedy has been indicated in the institution, in each coastal sector, of areas of biological protection, which are also envisaged by the regulations in force.

Another danger was recognised in the habit, prevailing at present, of dumping old motor-car bodies in the sea, in the presumption that it may be of some use in repopulating the waters. Those present at the Meeting asked that this practice should be abandoned until its real efficacy had been established, or until thorough research had proved that car bodies sunk in the sea can be guaranteed not to contribute to the pollution of the coastal areas, which are already too prone to the accumulation of rubbish of all kinds.

In conclusion, in another motion, after denouncing the damage caused both by over-exploitation and by pollution, typical of an industrial consumer society, those present at the Meeting urged the creation of marine « parks », in which they recognise « an effective means, both for the protection of environments and biocoenoses of particular interest - also for the purposes of a beneficent diffusion of particular species into the boundary areas - and for objectives of scientific and applied research, as well as for teaching and educational purposes ».

Un congresso di cultori di studi cibernetici

A Cascina Terme, nei giorni 11 - 12 - 13 Ottobre, si sono riuniti i ricercatori scientifici che in Italia si occupano di cibernetica. Il congresso è stato promosso e organizzato dal Consiglio Scientifico del Gruppo di Ricerca per la Cibernetica del C.N.R.

I temi del convegno erano i seguenti: elaborazione dell'informazione negli organismi viventi; proprietà fisiche dei sistemi biologici; interazione uomo-macchina; cibernetica dei sistemi artificiali; modelli biocibernetici; metodi e misure.

Hanno partecipato, con contributi scientifici, i ricercatori dei centri e laboratori cibernetici di Pisa, Bologna, Camogli, Arco Felice, Napoli (tutti del gruppo ricerche del C.N.R.), del Centro di Cibernetica Sammarinese (Dicastero della P.I.), ed i ricercatori del CISE e di Istituti Universitari interessati ai problemi cibernetici.

Sono stati affrontati vasti campi interdisciplinari che vanno dalla relazione fra l'ordine biologico e l'informazione, alla capacità dell'operatore umano in situazioni di riconoscimento percettivo, affrontando problemi come: la trasduzione dell'energia in alcuni processi biologici; la formazione di strutture organizzate in un sistema fluido; i rapporti fra i modelli della mente e i fenomeni presi a modello; il modello per lo studio del sistema neurovegetativo, fino al modello macroscopico di diffusione atto a descrivere l'aumento di popolazione in un ambiente variabile in modo casuale.

È stato esposto e discusso l'interessante problema dell'interazione uomo-calcolatore il cui scopo è quello di trovare, per l'uomo, modelli sufficientemente approssimati per descriverne il comportamento e per la macchina, invece, la struttura più idonea.

Delle complesse reti di circuiti omeostatici che operano nel sistema nervoso centrale e controllano molteplici condizioni interne del sistema, si è parlato nella relazione dedicata ai fenomeni emotivi e « Data Processing » nel S.N.C.

Con una relazione su « L'uso dei modelli matematici in problemi bioingegneristici per sistemi sensoriali menomati » è stato presentato, oltre ad un inquadramento generale di tali problemi, un modello matematico del sistema acustico che ha portato alla progettazione di una protesi per sordi.

Infine alcuni dei vari altri argomenti trattati riguardavano: la messa a punto di un dispositivo che consente di alterare la relazione esistente tra movimento dell'occhio e conseguente movimento dell'immagine sulla retina (ossia di variare a piacere il valore del Feedback retinico); la presentazione di una classe di automi astratti e lo studio di proprietà dei linguaggi accettati degli automi considerati; gli algoritmi per il conteggio degli oggetti posti su un piano e nello spazio; il progetto di un calcolatore stocastico; la previsione meteorologica automatica con macchine probabilistiche istruite; gli studi sulla stabilità del sistema oculo motorio.

L'intensa attività che si sta svolgendo nei vari centri, laboratori, istituti, illustrata dagli oratori con circa 130 fra relazioni, comunicazioni e memorie, dimostra come i problemi connessi alla ricerca cibernetica siano fortemente sentiti anche in Italia. Tuttavia, secondo quel che è dato sapere, non sembra sia ancora ben delineata una programmazione, nè che esista una coordinazione chiaramente strutturata.

A questo scopo è stata dedicata una parte dei dibattiti svoltisi nel corso del convegno. Il problema tocca infatti alcuni settori della ricerca che, come è stato fatto notare, trovano scarsi contatti con la cibernetica cosiddetta tradizionale.

Quello che sembra necessario evitare è una certa confusione nell'apporto delle varie discipline ad un programma cibernetico. Allo stesso modo però, sarebbe gravissimo errore il voler ignorare la realtà dei vari contributi interdisciplinari.

Non si può dimenticare d'altro canto che vi sono scienze umane che con le loro motivazioni e i loro interessi tendono sempre più a radicarsi nelle forme caratteristiche di un sistema 'chiuso'.

Così è auspicabile che tutti gli aspetti degli studi cibernetici possano trovare, come il recente congresso ha cercato di dimostrare, punti in comune su cui far convergere le future ricerche a patto di raggiungere una ben coordinata collaborazione di tutte le competenze.

Repubblica di San Marino, Centro di Ricerche di Cibernetica.

A. CASALI

A Congress of Experts in Cybernetic Studies

At Cascina Terme, on 11th, 12th and 13th October, a meeting was held of scientific research workers concerned with cybernetics in Italy. The Congress was promoted and organised by the Scientific Council of the Research Group for Cybernetics of the C.N.R.

The themes of the Meeting were as follows: Information processing in living organisms; the physical properties of biological systems; the man-machine interaction; the cybernetics of artificial systems; biocybernetic models; methods and measurements.

Scientific papers were read by research workers from the cybernetic centres and laboratories of Pisa, Bologna, Camogli, Arco Felice and Naples (all of the C.N.R. research Group), from the Cybernetics Centre of San Marino (Ministry of Education) and from the CISE and University Institutes concerned with cybernetic problems.

The subjects discussed covered a vast range of interdisciplinary fields, from the relationship between biological order and information to the capacity of the human operator in situations of perceptive recognition, dealing with problems such as: The transduction of energy in certain biological processes; the formation of organised structures in a fluid system; the relationships between models of the mind and phenomena taken as a model; the model for the study of the neurovegetative system; and the macroscopic model of diffusion, intended to describe the population increase in a variable environment in a random manner.

Another interesting problem discussed was that of the man-computer interaction, the purpose of which is to find, for man, sufficiently approximate models to describe his behaviour and, for the machine, its most suitable structure.

In the report devoted to emotive phenomena and « Data Processing » in the S.N.C., mention was made of the complex networks of homeostatic circuits that operate in the central nervous system and control a multiplicity of internal conditions of the system.

A paper on « The use of mathematical models in bio-engineering problems for impaired sensory systems » was concerned not only with a general survey of such problems but also with a mathematical model of the acoustic system that has led to the design of a new hearing aid.

Other subjects covered included: The perfecting of a device allowing the alteration of the existing relationship between movement of the eye and the consequent movement of the image on the retina (or the variation at will of the value of the retinal feedback); the presentation of a class of abstract automata and the study of properties of the languages accepted by the automata considered; algorithms for the counting of objects situated on a plane and in space; the design of a stochastic computer; automatic weather forecasting with instructed probabilistic machines; studies on the stability of the oculo-motorial system.

The intense activity that is going on in the various Centres, Laboratories and Institutes, demonstrated by the lecturers and speakers in about 130 reports and papers, shows how deeply felt, also in Italy, are the problems connected with cybernetic research. However, as far as could be learnt, it seems that there is not yet any well-defined planning and that no clearly organised coordination exists.

This question formed the subject of some of the debates held during the course of the Meeting. In fact, as was pointed out, the problem touches certain sectors of research that have little contact with so-called traditional cybernetics.

What seems to be necessary to avoid is a certain confusion in the contribution made by the various disciplines to a cybernetics programme. In the same way, however, it would be a very serious mistake to ignore the reality of the various interdisciplinary contributions.

On the other hand, it cannot be forgotten that there are human sciences which, with their motivations and interests, tend increasingly to be rooted in the characteristic forms of a 'closed' system.

It is therefore to be hoped that all the aspects of cybernetic studies may - as the recent Congress has sought to show - find points in common on which future researches may be made to converge, on condition that a well-coordinated collaboration of all spheres of competence is achieved.

Repubblica di San Marino, Centro di Ricerche di Cibernetica.

A. Casali

RECENSIONI - BOOK REVIEWS

F. Jacob, *La logique du vivant* (*Une histoire de l'hérédité*), (Série: *Bibliothèque des sciences humaines*), Editions Gallimard, Paris, 1970, 354 pp., No price stated.

The history of biology (and of the other sciences of nature) can be conceived in two ways, states the author in his introductory chapter, entitled *Programme*. One way, the more usual way, is to trace the ideas in their historical development, their evolution, their diffusion and possible dispersion and finally their death (if this occurs): that is to say, up to the moment at which they cease to be valid, or are transformed into somewhat different ideas or are replaced by new ideas. For example: tracing the history of the idea of spontaneous generation up to the final demonstration of its fallacy. The historical development of ideas does not always correspond to the logical development of the facts known about a particular subject, or at least to present-day logic, as it satisfies us today.

The other method of writing the history of biology consists in tracing its logic: « in seeking to discover how objects have become accessible to analysis, allowing the constitution of new fields in science ». This is the method that has been chosen by the author, who has divided his subject matter into five lengthy chapters: Visible structure; Organisation; Time; The Gene; The Molecule. These correspond to as many different levels of study, which, though substantially successive in time, do not necessarily represent the historical sequence of the ideas.

The historical account reveals that the author is very well informed on the history of biology. This is not always the case with people who, as François Jacob (Nobel Prize winner together with A. Lwoff and J. Monod for their work in molecular biology) has been in the past and still is fully engaged in experimental work. The subject matter is all concerned with the logic of the living beings that is to say, the logic that Jacob - and with him, a good many modern biologists - recognise in the living beings. In fact, the author states in the introduction: Biology, like the other sciences of nature, has shed many of its illusions today; it no longer seeks truth, it builds it for itself.

On these bases, Jacob pursues a succinct and continuous discourse, showing how biology, through attack at different levels of organisation and along various different lines of study, has arrived at a knowledge of some of the fundamental problems of life, on the molecular level: that is, at the level at which takes place the origin of life from chemical substances, i.e. the birth of « living beings » from « things ».

In the concluding chapter, entitled *L'intégron,* the author discusses how biology, having arrived at the most minute analysis of the molecules of living organisms - that is to say, at an interpretation of heredity and the elementary functions of life, such as protein synthesis, in molecular terms - may ascend the scale of complications and reconstruct, in its immense complexity, the living organism.

The author states that, whatever the level studied, whether that of the molecule, of the cell, of the organism or the population, the history is the necessary line of approach, and succession is the explanatory principle. The organization of living systems obeys a series of physical and biological principles: natural selection, minimal energy, self-regulation and building-up in planes through successive integrations of the sub-units. Natural selection imposes a finality not only on the organism as a whole but also on each of its component parts. Thus, living beings are built up through a series of actions of packaging (*empaquetages*). At each level, units having relatively well-defined dimension and almost identical structure unite to form a unit of a higher degree. Each of these units constituted by the integration of sub-units may be indicated by the term *integron*.

The author does not conceal - indeed, he emphasises - the difficulties that are still encountered at many points in the application of this theory: in particular, the origin of life, the passage from prokaryotic (bacteria and cyanophyceae) to eukaryotic stage (all the other organisms having a cellular structure), the interpretation of the phenomena governed by the nervous system, etc. He mentions the problems in which, for want of documentation or evidence, biology must be content with conjectures. However, thanks to the particular studies carried out by Jacob and many other molecular biologists and to this splendid and consistent overall survey that the author has offered, the logic of the living being, as we see it today, is certainly a valid instrument of study. And it can readily be asserted that it will remain valid for many years to come and will inspire many lines of research, the results of which will give the answer to many questions that still remain open.

Will it be possible to arrive at a complete description of thought and feelings in physical terms? Possibly. But let us remember that it has been shown that a logical system is not sufficient for its proper description. Will the explanatory system of present-day science also be sufficient in the future? Will not a new « Russian puppet » emerge from human thought - a new sleight of hand that will appeal to other possible logics?

These are the questions with which the author ends this fascinating volume, which no biologist should ignore and which philosophers should read.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

PH. HANDLER (ed.), *Biology and the Future of Man*, Oxford University Press, London, 1970, XXIV+936 pp., figs. and tables, £5.25.

The Committee « on Science and Public Policy » of the National Academy of Sciences of the United States has published a series of reviews of the present state of knowledge in the various scientific disciplines. Obviously, the most complex and difficult of these reviews has been that relative to the biological sciences. No less than 22 commissions were constituted, composed of from 3 to 16 members selected from among the most distinguished and qualified experts in the various subjects, who were entrusted with the task of compiling the essential summaries of the knowledge so far acquired in the various fields; these summaries were to be presented in a form that would also be comprehensible to non-specialists, while maintaining a high scientific tone and level of accuracy. Mention was to be made of the main problems that confront research workers and of the methods by which these problems are studied today, with an indication of the presumable lines that will be followed by future developments.

The work of the individual commissions was then carefully and rigorously scrutinised and criticised in collective meetings and was finally subjected to further scrutiny by the editor. The present volume is the result of this lengthy and not inconsiderable task. After the initial introductory chapter, the following twelve chapters constitute a compendium of the knowledge acquired in the following fields of science: molecular biology; the materials of the vital phenomena and their transformations; the cell as a biological unit; the origin of life; development; the functions of the tissues and organs; biological structures, from molecules to man; the nervous system; behaviour; ecology; heredity and evolution; the diversity of organisms. Chapter 14 constitutes an exception to the principle adopted of not dealing with techniques and instruments: it is devoted to the use of electronic computers in the biological sciences.

The remaining chapters, from 15 to 20, instead of being devoted to purely scientific problems, like the previous ones, are concerned with the practical applications of biology: subjects of extreme topicality being studied today and disputed at various levels. Foremost among all is the problem of the possibilities of feeding an ever-increasing population. Another highly complex subject is that of the relationships between pure science and medicine. Separate chapters are devoted to biology and industrial technologies, the problems of environment and that of the renewal and administration of the resources. The twentieth and last chapter, entitled Biology and the Future of Mankind, presents an overall survey of the possibilities of intervention of the biological sciences in guiding the destinies of mankind, at times accrediting somewhat utopian visions that, in the reviewer's opinion, are over-optimistic. The writer of this chapter concludes with the following observation: *Homo sapiens*, that product of nature, has surpassed nature. From a product of circumstances, as he was originally, he has risen to a position of responsability: he has become Man. May he behave as such.

In general, the verdict on this volume is decidedly favourable. It is a sort of epitome of the biological sciences and their applications, summing up the main achievements of modern biology in its multiple specialities and its prospects of future development. However, there are no bibliographical references, neither are any authors cited, save for a few classics, and the illustrations are reduced to a minimum.

The work will prove highly useful to anyone who, though not a biologist, may wish to have a survey of the biological sciences and of the importance of their applications for the welfare of mankind. It is also useful for immediately giving the biologist an orientation in sectors foreign to that of his own particular competence.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

Readings in the Life Sciences, 7 VOLS. (*Scientific American Resource Library*, Offprints 1-199, 1001-1120), W. H. Freeman and Company, Berkshire, 1969, 2834 pp., figs., £32.18.

The periodical « Scientific American » has published a number of articles, usually excellent, written by the most competent experts and copiously illustrated on various scientific topics. All those pertinent to biology, printed between 1948 and 1969 are collected in seven volumes, according to the order of their first appearance in the journal. They are numbered from 1 to 199 and to 1001 to 1120. A Topic Index and an Author Index make easy the consultation of these extremely useful books, which should be present in any biological or general university library.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

Z. KIELAN-JAWOROWSKA, *Hunting for Dinosaurs*, The M.I.T. Press, London, 1969, 178 pp., figs, £3.75.

This is the description of an expedition of Polish paleontologists into the Gobi desert in search of fossil remains of dinosaurs. The volume, which is intended for the general reader, contains many photographs illustrating the various stages in the expedition and the excavations. The reader will find it highly interesting and stimulating and praise is therefore due to the enterprise of the M.I.T. Press in giving it wide circulation through the English translation.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

J. C. ECCLES, *Facing Reality* (Philosophical Adventures by a Brain Scientist), (Series: *Heidelberg Science Library*, Vol. 13), Springer-Verlag, Berlin, 1970, XII+210 pp., figs., DM 22.60.

« Facing Reality » represents an interesting although difficult and often incomplete bio-philosophical essay on problems proposed by the great development of biological sciences and particularly of Neurobiology. The book appears to be very good and contains useful information particularly in those chapters devoted to neurophysiological problems, such as Chapters II and III. Here the physiology of synapses and the possibility that synaptic mechanisms are involved in learning and memory are discussed in detail. Highly informative is also the concept, presented in Chapter VI, that scientists cannot be « ... merely passive in physical observations but participate in respect both of the common frame of reference and of interference that is necessarily produced in any frame under observation » and the author's preoccupation that science be only a « means to achieve one's personal advantage and fame ».

The negative aspects of the book are, in my view, represented by those parts in which the author appears to be too inclined towards mysticism and spiritualism and in which he is seriously limited by his religious and ideological prejudices. Just to mention a few examples: in the general conclusion of Chapter V, after a detailed account of the neuron mechanisms which operate in the brain as the basis for one's conscious experience, Eccles states that « the evidence presented in this lecture shows that these events in the material world are necessary but not sufficient causes for conscious experiences... » and then discusses the religious concept of the soul and its special creation by God. Continuing on this mystical line, the author « cannot believe that this wonderful divine gift of a conscious existence has no further future, no possibility of another existence... » and comes to the conclusion that « ... this possibility of a future existence cannot be denied on scientific grounds ». Of similar style are the author's statements about Communist science and the Communist world: in Chapter VIII the author leads the reader to the conclusion that no substantial differences exist between Hitlerian, Egyptian and the present Russian tyranny. Then he expresses a sincere sorrow for the Russian people!: « it is pathetic to witness the scientific presentations of some of these unfortunate people (Russian), ... Russia is the most reactionary country in the world... » and finally concludes « Soviet biology is really very poor ». No less drastic and dogmatic are the author's opinions about young people and modern expression of life: « Mankind — he writes in Chapter I — is sick... There are many symptoms of this sickness or alienation. It has resulted in various forms of irrationality such as existentialism in philosophy and the meaninglessness and formlessness of so much of so-called modern art ».

Obviously on this basis no fruitful discussion is possible. There is a danger that statements such as those reported above obscure the meaning of the book and reduce its « biological » value.

Milano, Università, Laboratorio di Fisiologia.

M. MANCIA

A. FRAJESE, *Attraverso la storia della matematica* (Series: *La Matematica nella Cultura e nella Scuola*, No. 3), Felice Le Monnier, Firenze, 1969, XII+372 pp., figs., L. 3.800.

The notably increased amount of attention being devoted to studies in this field is justified by the importance attaching to the history of mathematics both as a cultural factor and as a survey of the theories of a science seen in course of evolution. This book by Frajese, an author of wide culture, open-minded to both scientific and human problems, must be recognised as a very compelling work by reason of both the enthusiasm which has inspired it and the fascination it arouses.

Rather than reduce the history of mathematics to a lifeless enumeration of data and erudite references the author has preferred to examine its development as seen in broad historical contexts, tracing the essential problems of each subject with scientific rigour and indicating the lines along which they can be explored in pursuance of solutions. In this way the reader is made to feel drawn into the arguments and let to a more dialectic approach.

Consideration is given to pre-Euclidean geometry and the influence of Platonic philosophy on the evolution of Greek mathematics, attention is focused on Euclid's Elements, a fundamental work of synthesis and the starting point for all and more through developments. The author emphasizes the fundamentals and the modes of procedure of this work as determinants of the scope and growth of mathematics. He goes on to examine the theory of parallelism and that of proportionality; then considers problems whose treatment involves the use of methods more akin to algebra, such as those relating to areas and to the theory of irrationals. He deals with the method of exhaustion in its historical context ranging from Democritus to Eudoxus and then its further development by that giant of thought, Archimedes, using infinitesimal techniques.

The author stresses the importance in mathematical history of Leonardo Pisano whose re-examination of ancient classical mathematics, based on absolute mastery of the material, started the revival of algebra in the West. The story of this Western rebirth is followed in the works of the sixteenth century Italian algebraists who showed how to solve equations of the third degree and developed algebraic symbolism. Particular attention is paid to Galileo's influence on the genesis of the infinitesimal calculus. The development of projective geometry, originated among the Greeks, is illustrated by reference to the original contributions made by Desargues. Finally, consideration is given to Dedekind's postulate of continuity and to the theory of sets as created and developed by G. Cantor.

The book should commend itself to teachers and cultivated readers, by reason of the author's vividness of exposition and the interest engendered by the problems he restates against the background of their sequence in time and the dynamics of their evolution.

Milano, Politecnico.

P. Cavallotti

A. Dumitriu, *Istoria Logicii*, Editura Didactica si Pedagogica, Bucuresti, 1969, xii+1050 pp., figs. and tables, Lei 53,90.

This treatise constitutes a historical survey of logical developments beginning with those that can be traced as inherent in primitive thought (for instance, the 'pré-logisme' so described by L. Lévy-Bruhl) and concluding with modern logistic. A chapter is devoted to ancient Chinese and Indian logic. The author does not confine himself to formal logic but also attends to related philosophical propositions. In particular he devotes much space to Kant, Hegel and Husserl. In explaining the logic of Aristotle and that of the Megaric-Stoic school, etc. he takes account of modern symbolic methods such as those of J. Lukasiewicz, I. M. Bochenski, and others. About one-third of the volume is devoted to mathematical logic ranging from Raimondo Lullo and Leibniz to authors of the present day. As the text is set out from a historical standpoint, without overemphasizing the technical aspects, it is intelligible also by non-specialists, though at the same time the reader, even if his interest is solely historical, is introduced to the main symbolisms, as in the case of G. Frege. One chapter is devoted to logic in Roumania. An index is provided in French, English and Russian, besides Roumanian.

Messina, Università, Istituto di Filosofia.

G. Vaccarino

C. Gillispie (ed.), *Dictionary of Scientific Biography*, 2 vols., Ch. Scribner's Sons, New York, 1970, Vol. I: *From P. Abailard to L. Berg*, 624 pp., figs.; Vol. II: *From L. Berger to Ch. Ballot*, 628 pp., figs., No price stated.

This work, with reason definable as fundamental for the history of Science, was born under the auspices of the American Council of Intellectual Societies in collaboration with the American Society of the History of Science. It went to possibility by the generous contribution of the Natural Science Foundation.

Editor in chief is Ch. Gillispie, managing editor is Marshall De Bruhl. There is also a committee of consultants among whom we notice the Italian Maria Luisa Righini Bonelli, Directress of the Institute and Museum for the History of Sciences of Florence, and in each book the names of the authors of the various biographies, selected among the experts of history of the sciences from every place in the world, are given.

This dictionary really opens new ways of research and study in the development of sciences from the ancient commencements up to the current age. The various items relate to the main mathematicians and naturalists from every part of the world and from all the historic periods and gives information never before available in a so comprehensive manner. When brought to completion it will be a real compendium of history of the sciences.

The volumes appear in a perfect, plain edition, really honouring the Editorial Committee and the Scribner's Sons publishing house. Consultation of the individual items, full of data and information about life and works of the various scientists, is easy.

Turning over the pages of the first volume we find out that the first term refers to Pierre Abailard, born in Britanny in 1079, who minded logic, theology, philosophy. We find then, for example, Aristarchus from Samos, mathematician and astronomer celebrated to have been the first to devise the heliocentric theory 18 centuries before Copernicus. A long and learned treatment of the life and work of Aristotle which runs through 31 pages. Interesting, also for the difficulty to examine the original texts, is the item Al-Bāttanī, in Latine known as Albatenius, one of the greatest Islamic astronomers, born before the year 858 in the neighbourhood of Harrar, in Northwestern Mesopotamia. He is famous for his work, entitled in Arab language Zīj, which was translated into Latin language by G. Nallino with the cooperation of G. Schiaparelli with the title *Opus Astronomicum*.

The work's originality consists in the fact that the authors of the various biographies were selected among the ones known to be acquainted with the works and lives exposed and, then, with full competence of the treated personalities.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

M. I. Pereira de Queiroz, *Riforma e rivoluzione nelle società tradizionali* (Storia e etnologia dei movimenti messianici), (Series: *Saggi*, No. 27), Edizioni Jaca Book, Milano, 1970, 274 pp., L. 2.200.

The book by Pereira de Queiroz resembles W. E. Mühlmann's study in German, entitled *Chiliasmus und Nativismus*, and also translated in French, in so far as both these works are about messianic movements among primitive peoples and in modern peasant societies. They differ, however, in that the present author bases her work largely upon data gathered in reference to the peasant society of modern Brazil whereas Mühlmann's work rests upon ethnological data relating to tribal populations. The criterion she adopts is sociological. Whether in primitive or in Western societies, mediaeval or modern, 'messianisms' are sociologically speaking all on the same plane: they occur in societies that are organized in large families but not in societies whose organization takes the form of social classes, unless indeed the latter happen to be mixed societies in which social classes coexist with groups organized as large families, as is true of peasant societies in Europe and in modern Brazil.

Abandoning, therefore, the old distinction between 'primitive' and 'Western societies, the author is enabled to contrast two major types of society: on the one hand, the 'traditional' type based on a structure of large families, a rhythmical cycle of development, and a religious outlook towards the world; on the other hand, the 'modern' type based on a class structure, on development by way of evolution, and on a lay outlook towards the world. This definition, although not in fact a new one - since differentiation between the cyclic and the evolutionary concepts of time, and between civilizations that accept magical-religious obligations and those which have become secularized, is obsolete - has been found effective for correlating the phenomenon of messianic movements with the existence of a particular substratum of sociological experiences: namely, the substratum that consists of groups which are firmly consolidated by internal kinship or clan links.

The important point is that the author then goes on to distinguish between two alternative and mutually opposed types of messianic movement: the reformist and the revolutionary. Movements of the reformist type (e.g., the movement led by David Lazzaretti at Arcidosso in 1878; the messianic movements of Canudos, Contestado, Joazeiro, which lasted among the Brazilian peasantry from the latter part of the 19th century into the beginning of the 20th; the migratory messianic movements of the Tupi Indians four centuries ago) are those triggered off by defective conditions of life and by internal disorganization: they aim at re-establishment of the order that has been lost. On the other hand the revolutionary movements (such as millennialisms in Europe during the middle ages and in the United States during the last century; likewise movements among colonially governed peoples incited by prophets) ensue from an urge to counteract oppression exercised by social strata or groups occupying a position of hegemony.

While appreciating that De Queiroz has usefully expanded the concepts of Mühlmann who related both 'millennialism' and 'nativism', as such, to a single pan-revolutionary denominator, we must nevertheless enter a reserve against the possibility of sharply separating reformism and revolution within messianisms in general, thus turning them into heterogeneous types. Consider, for instance, the movement led by David Lazzaretti in Tuscany, cited by the author as 'reformist' on the basis of an interpretation which seems to us one-sided and partial, she being unaware of the book by A. Moscato (*Rivolta Religiosa nelle campagne*). That movement proclaimed the apocalyptic, and therefore revolutionary, overthrow of the existing institutions, or in other words of the Church and of the Monarchy which at that time had only just been established. What it illustrates apart from anything else is that on one side it was a movement aiming to re-establish a lost order and on the other side a denunciation of the oppression exercised by the establishment and the governing institutions.

Another consequence which seems unrealistic and artificial is the distinction the author draws between internal factors of disorganization (anomie) and oppression from outside; for in most cases a historical and sociological continuity of both these disturbing factors in combination is found to have existed. Other examples may be given: thus Gandhism (which the author does not mention) is revolutionary in a political sphere, reformist in a religious and social sphere; Ambedkar, founder of the movement to emancipate the untouchables in India, and likewise the Black Muslims in America aiming to emancipate the negroes there must be classed as revolutionary in regard to religious and social policy while reformist as regards cultural policy. In point of fact reformism and revolutionism coexist, variously combined, in every religious or messianic movement. Even the Brazilian peasant messianisms to which the author pays particular attention include revolutionary elements in addition to their social reformism in so far as they envisage a reversion - not merely in a utopian sense, but actively, by way of 'holy wars' - from republicanism to monarchism.

The book is a mine of very interesting data, especially on the many Brazilian popular messianisms that exist even today. It appears to us, however, that messianic and prophetic movements in general ought to be attributed to a plurality of interacting tendencies and functions which are liable to develop more or less actively according to whatever changes are occurring in particular historical situations.

Roma.

V. Lanternari

P. W. Bridgman, *La critica operazionale della scienza* (Series: *Biblioteca di Cultura Scientifica*), Boringhieri Editore, Torino, 1969, 454 pp., L. 6.000.

This book presents a collection of writings by Percy W. Bridgman, ranging from his article of 1934, entitled 'A physicist's second reaction to Mengenlehre', to the last volume, published in 1959, *The Way Things Are.* The italian translation is by Giorgio Gabella.

This work completes for the Italian reader his own panorama of Bridgman's principal methodological work, initiated by Vittorio Somenzi, whose translation of *The Logic of Modern Physics* was published by Einaudi in 1952.

In my opinion a great merit of the methodological positions taken up by Bridgman lies in their being more or less directly inspired by the problematics of physical research developed by the author in the field of very high pressures,which led to the award of the Nobel Prize in 1946. This is one of the reasons for their originality, which makes it difficult to label them under the heading of any more or less recently recognized school of thought. That indeed is what his editor, Bruno Cermignani, clearly demonstrates in the latter's introduction to the collection of papers, wherein he seeks to protect the originality of Bridgman's position against the risk of its being interpreted as neo-positivist or conventionalist.

Roma, Università, Istituto di Fisica « G. Marconi ».

S. D'Agostino

E. Cassirer, *Determinismo e indeterminismo nella fisica moderna*, La Nuova Italia Editrice, Firenze, 1970, xvi+320 pp., L. 3.500.

No-one who is already familiar with the ideas of E. Cassirer can be surprised by his solution, on the 'critical' plane, of the fundamental problem of determinism. Nor, for that matter, have the physical theories so long regarded as models of determinism ever been completely deterministic in the material sense. (Consider for instance the admitted inability of Newton to solve by means of a unique mathematical description, the problems concerning the dynamical explanation of the ellipticity of planetary orbits).

At the epistemological level, however, the new physical theories are as deterministic as those of the Newtonian type. Where the essential differences lie, according to Cassirer, is at the level of 'objectivation', corresponding to what other authors have seen as the progressive "desubstantiation,, of physical objects. Hence the effect produced by the unrestrained adoption of intuitive models to interpret a mathematical formalism which has no need of this, referring as it does mainly to measuring operations and not to objects. Though the author's conclusions on the matter are very important what he says on the subject of particular laws is not acceptable: for even leaving their essentially symbolic value an open question, it is clearly impossible to ascribe this to the self-duplicating effect of the form in which general laws are stated.

Roma, Università.

V. Cinanni

C. J. Timmons (ed.), *Modern Reactions in Organic Synthesis*, Van Nostrand Reinhold Company, London, 1970, vi+312 pp., tables, £5.10.

The book contains seven articles by various authors relating to applications of new methods of synthesis developed in the last ten years.

The articles deal with reaction mechanisms and show the selectivity of the new techniques and their application in complicated cases. No experimental details are given, and in general there is no list of products obtained from a particular reaction, reference being made instead to more specialised publications.

Reduction methods are described by R. F. Evans, as regards both reagents (hydrogen, complex hydrides, hydrazine) and the transfer of hydrogen from one substrate to another.

In the chapter on oxidation methods, R. G. R. Bacon discusses new techniques using conventional oxidising agents, and also hydrogen abstraction or oxygen donation by means of metal oxides or salts or organic substances.

G. B. Gill deals with synthetic uses of free radicals, describing first the radical-forming reactions and then the chief classes of radical reactions.

R. Brettle covers electrochemical methods, with special reference to oxidation and reduction methods.

In the chapter on photochemical methods, E. V. Blackburn and C. J. Timmons consider photolysis, addition, cyclisation, and rearrangement reactions, and the formation of carbenes and nitrenes.

J. W. Barton gives an account of the synthesis of benzene rings from aliphatic intermediates and benzynes, and the introduction of unusual functional groups by unconventional methods.

In the final chapter, G. W. H. Cheeseman deals with various methods for obtaining heterocyclic rings, including the 1,3 dipolar cyclo-addition reaction, the Diels-Alder reaction, syntheses using pyridinium, pyrylium and phosphonium salts, and syntheses using benzyne intermediates and ylides.

The size of the book shows that the various topics are not deeply examined, but the numerous bibliographical references allow the reader to pass easily to more specialised or original publications. The book has the merit of bringing together in order, though briefly, a large quantity of information, and will be useful both for a rapid or initial study and as a guide to the relevant literature. From this aspect, though agreeing with Timmons that the choice of topics is entirely personal, I feel that (if only for uniformity) some topics would have merited fuller treatment, for example radical syntheses, 1,3 dipolar cyclo-additions and ylide syntheses.

Most of the literature references are up to 1968. It would certainly have been very useful to add to each chapter a note of more recent topics and references. This has been done in chapter 3, and somewhat briefly and incompletely in chapter 8.

Milano, Politecnico, Istituto di Chimica.

C. CARDANI

J.-E. LOISEAU, *La phyllotaxie* (Series: *Monographies de Botanique et de Biologie Végétale*), Masson et C.ie, Paris, 1969, x+224 pp., figs., 70 F.

The term "Phyllotaxis" may sound slightly old-fashioned to many contemporary botanists. Yet the orderly arrangement of leaves on the axis is a very good example of organised growth in plants, and everybody must agree that the problems of biological organisation and control of developement are far from being solved by molecular biology.

Professor J.-E. Loiseau authoritatively expounds the views of the so-called "French school" on phyllotaxis, based on Plantefol's original theory and linked with the interpretation of the shoot apex also given by the French school. According to this latter theory, the cells at the tip of the shoot apex form a "méristème d'attente" of practically no importance during vegetative growth, while the principal activity occurs in the "anneau initial" or meristem ring. On the basis of many kinds of evidence, including that derived from surgical experiments, Loiseau believes that Plantefol's theory of multiple foliar helices is definitely proved, and that a synthesis of Plantefol's and M. and R. Snow's "available space" theory is possible. Loiseau concludes by remarking that his study confirms a fact of capital importance, already pointed out by Buvat: *i.e.* the *importance of the leaf* whose formation precedes the ontogenesis of the apical meristem, also in the embryo.

As I said before, the implications of phyllotaxis are extremely important in studies of morphogenesis and development, and the monograph under review will be very useful as the "official" and up-to-date account of the results achieved by a scientific school which is still - I believe - not very popular outside France.

Siena, Università, Istituto di Botanica.

G. SARFATTI

M. J. Schleiden, *Principles of Scientific Botany* (Series: *The Sources of Science*, No. 40), Johnson Reprint Corporation, New York, 1969, xliv+616 pp., figs. and plates, $27.50.

"The life of the entire plant is the result of life in its individual cells; we shall therefore gain no insight into our subject, and no possibility of explaining it, so long as we are unable to trace back the general results of vitality to their origin in the individual cells. Hitherto, in the absence of a right method of investigation, little has been done. The consideration of this part of organology must, therefore, consist principally in stating correctly the problems involved and the mode of solving them. The same, also, with respect to that part of the science which has its foundation in morphology. There it must be discovered what morphologically-similar organs the plant possesses; here it must be considered how far morphologically-similar organs present also similar phases of the universal life of the cell, and how far they may thereby be converted into physiologically-similar organs".

This quotation from the "Grundzüge der wissenschaftlichen Botanik", first published in 1842-43, can give and idea of Mathias Jacob Schleiden's main traits and merits: the typical highly theoretical approach, the need to systematise the whole science of botany, his somewhat low opinion of fellow botanists, but at the same time his taking the study of the cell as the point of departure for the study of the whole plant.

Schleiden's treatise is of great historical interest and the reprinting of the English translation published in 1849 will be welcomed by students of botany and of philosophy of science. The introduction by Jacob Lorch is extremely illuminating and convincingly explains why Schleiden, notwithstanding his several gross blunders and his relatively scanty achievements in research, had such a profound influence on botanical science and produced a treatise which, at the opening of a new age in botany, "stands out as a landmark of lasting merit, second to none".

Siena, Università, Istituto di Botanica.

G. Sarfatti

P. Parenzan, *Carta d'identità delle conchiglie del Mediterraneo*, Vol. I: *Gasteropodi*, Bios Taras Editrice, Taranto, 1970, 284 pp., figs. and tables, L. 5.500.

The present volume appears two years after the one by F. Nordsieck entitled "Die europäischen Meeres-Gehäuseschnecken (Prosobranchia) vom Eismeer bis Kapverden und Mittelmeer" (G. Fischer Verlag; Stuttgart, 1968), the layout of which it retraces: a very brief identification of the various species and drawings of the various shells.

Though the volume is perhaps slightly clearer than the German work, from the iconographic point of view, on the whole it contains a smaller amount of information, as regards both the morphology and the ecology and general geographical distribution of the individual species. Furthermore - though bearing in mind the difficulties, often objectively unsurmountable, involved in dealing with such a vast amount of material - the author makes such ingenuous statements and falls into so many gross analytical and methodological errors that grave doubts may be cast upon the reliability of the whole of this weighty work that has been undertaken. It is therefore only right to warn budding malacologists and amateur conchologists - who will undoubtedly make extensive use of the volume - that it must be used with due caution.

Another defect of this "Identity cards of the Mediterranean shells" - which is all the more serious in view of the fact that the volume is one that is chiefly intended for non-professionals - is the complete lack of even a summary bibliography and of an introductory chapter on the methods of collecting, preserving, studying and arranging shells.

Firenze, Università, Istituto di Zoologia.

B. LANZA

V. VEROLE BOZZELLO, *Le grotte d'Italia* (Guida al turismo sotterraneo), Bonechi Editore, Firenze, 1970, VIII+320 pp., figs., L. 4.500.

As may be seen from the subtitle, and as stated in the preface, the volume is a guide to speleotourism in Italy. This is the first work of its kind to be published in Italy and the lack of such a volume has been felt for some time, in view of the ever-increasing general interest in the subterranean world.

The volume opens with a complete account - naturally on a level suited to the general reader - of the main historical, scientific and technical problems involved in speleology. There follows a description, region by region, of the caves, both large and small, that have so far been exploited on a touristic level in Italy.

However, the work is marred by a number of inaccuracies and some errors in the first part, which the author could have avoided by submitting the various sections to particular specialists for their critical revision. For example, it is wrong to attribute to the category of the troglobia either the guanobious animals or those, like the mammal *Heterocephalus*, that are merely endogeous.

Nevertheless, the volume is one that may be consulted with profit and pleasure both by the budding speleologist and by one that is already experienced, and praise is therefore due to the publisher, who has produced such an attractive edition, and to the author, who, in my opinion, has fully achieved his intended objective: to enable the visitor not only to "look" but also to "see".

Firenze, Università, Istituto di Zoologia.

B. LANZA

R. G. BURNS, *Mineralogical Applications of Crystal Field Theory*, (*Cambridge Earth Sciences Series*), Cambridge University Press, London, 1970, XIV+224 pp., figs. and tables, £4.00.

This book offers a new and useful tool in mineralogical, petrological and geochemical research. The problem of the relations between crystal structure and chemical composition can be attacked by crystal field theory for the elements in the first transition series of the periodic table, including, for example, Fe, Mn, Ni, Cu and other metals forming a total of 40% of the Earth by weight. Since the absorption spectrum of electromagnetic radiation (including colour and pleochroism, in the visible as well as in the infra-red and ultraviolet) in crystals is governed by characteristic transitions of electrons at various energy levels when an atom or ion is bound to others in particular relations of symmetry, by measuring the absorption of light one can obtain further knowledge of the coordination and valency state of these elements in many silicates, which are important constituents of rocks. As Dr. Burns shows, after a discussion of the methods of application and many examples of absorption diagrams and the assessment of splitting and of stabilisation energy in the crystal field these results can be correlated with crystal chemistry, e.g. of isomorphous series, and with thermodynamic properties of the minerals considered. For example, it is possible to analyse the fractionation of a particular element between coexisting minerals. Other chapters deal with possible applications of the method to geochemical research and to problems of the composition of the mantle. Lastly, metal sulphides are discussed, although their interpretation cannot be based on crystal field theory alone. The book is notable for its well-conceived plan and balanced development. It ends with a lengthy bibliography and a long and accurate subject index.

Torino, Università.

A. DEBENEDETTI

G. MILLOT, *Geology of Clays* (Weathering, Sedimentology, Geochemistry), Springer-Verlag, Berlin, 1970, XVI+430 pp., figs., tables and plates, DM 61.

What are generally called clays contain various minerals which have been extensively studied and well defined structurally. In his book, now translated from French into English, Professor Millot considers the geological aspects of formations containing clay minerals as principal or accessory constituents. Since these are secondary minerals, formed by various processes (usually hydrochemical) from silicates, the geological study of such formations must consider the conditions which caused the alteration of the original minerals, the production, transport and deposition of the secondary minerals, and any diagenesis or metamorphism which they may have undergone. In each stage of this development, conditions may have varied according to the types of original minerals, the physical and chemical conditions under which clay minerals are formed, the continental or marine environment the transport and sedimentation processes, and the subsequent conditions of temperature and pressure. The author analyses many of these relevant conditions, dealing especially with weathering and pedogenesis in various climates, the siderolitic and red beds facies, the glauconitic, iron mineral and saline deposits. Four sedimentary series of different geological age are then discussed as an example of the evolution of clay minerals. The last part of the book again discusses the genesis of clay minerals, with reference to the results of laboratory syntheses, and ends with geochemical considerations. Despite the author's obviously deep knowledge of much of the subject and the extensive bibliographical material, the book leaves the impression of a certain onesidedness and unbalance. There is a very long bibliography, but the index is not sufficiently detailed.

Torino, Università.

A. Debenedetti

V. Ronchi, *The Nature of Light*, Heinemann, London, 1970, 288 pp., plates, £. 6.0.0.

This is the translation into English by V. Barocas of Ronchi's well-known book entitled: *Storia della luce*, the first edition of which appeared in 1939; the second followed in 1952 and to this the French edition in 1956. While the English edition has now been published, one in German is being prepared. It was time that such an important book should be made available in foreign languages more widely known than Italian. Furthermore, to this English version the author has brought new and important additions. The Italian title *History of Light* has been changed in English into *The Nature of Light* as the author not only outlines the history of light from the first scientific attempts up to our present time but often dwells also upon problems and discussions which arose as the characteristics, that is to say, the very nature of light, were little by little discovered. One who has not had the possibility of reading the Italian text will find this translation in a language so well-known in the scientific world, very interesting for the clear, precise and detailed exposition of the origin and development of optical researches which, it can be said, started in the Greco-Roman age (Chap. 1).

During the Middle Ages particularly interesting is the contribution given to optics by the Arab scientist known in the West by the name of Alhazen. It is especially in regard to his contributions that the author has made further additions in the English version and observes that in all Alhazen's work, although an effort to coordinate phenomena examined experimentally is noted, still an influence of old methods of reasoning is to be found and, more particularly, those of the atomists.

As earlier optics gradually disappeared we come to the invention of the telescope which opened out a new era for optical researches. While Galileo, reconstructing the telescope, limited himself to note that *dovesse trovare fondamento su la scientia di prospettiva* (« was to find its basis on the science of perspective »), Kepler in his *Dioptrice* develops the geometrical theory of lenses as it is still expounded today, thus explaining the Galilean telescope and even the principle of the telephoto lens.

The 17th century then had a brilliant beginning whereas ancient optics was destroyed. So we reach the discoveries of Descartes and Grimaldi which opened the road to those by Newton and Huygens. The valuable book by Grimaldi *De Lumine* is amply dealt with, showing that Grimaldi made very important advances in the knowledge of light. For him light was no more something without colour; when propagated linearly it shows the phenomena of reflection, refraction and diffraction but still is capable of stimulating the vision of illuminated objects.

Chapter 5 of the book is dedicated to Newton and Huygens. With them the great controversy started between corpuscular and undulatory theories of light. In reality, as the author observes, although he did not uphold the corpuscular, Newton was not opposed to the undulatory theory. Huygens instead was a valid and staunch upholder of this theory.

The passage from the 17th to the 19th century (chap. 6) decidedly brings the triumph of the undulatory theory (chap. 7) with the later discoveries by Malus, Young and Fresnel. Owing especially to the work of these researchers the corpuscular theory, after a few decades, could be considered to have only a historical interest; whereas the undualtory theory has acquired fundamental importance in 19th century physics.

In Chap. 8, entitled *What is light?*, the author discusses the significance that must be given to the words *lumen* and *lux*. The first is nothing but « radiation » or « radiant energy »; the second is what one sees - in the words of the author - « the very same meaning attributed to it by philosophers two thousand years ago. That there is light simply means that the psyche is not idle but produces phantoms, even if only in a dream ».

Firenze, Osservatorio Astrofisico di Arcetri.

G. ABETTI

LIBRI RICEVUTI - BOOKS RECEIVED

Le opere elencate nella rubrica "Libri ricevuti,, non vengono raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

The works listed in the section "Books received,, are not grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

AMSTUTZ G. C. - Glossary of Mining Geology.
ASIMOV I. - Une particule fantôme: Le Neutrino.
ASTRUC L. - Créativité et sciences humaines.
AVRAM M. and MATEESCU GH. D. - Spectroscopie infrarouge.
BALDRY P. E. - The Battle Against Heart Disease.
BARRY J.-M. and BARRY E.-M. - Eléments de biochimie structurale.
BEER S. - La science de la gestion.
BELL P. R. and WOODCOCK C. L. F. - The Diversity of Green Plants.
BERRY R. J. and SOUTHERN H. N. (eds.) - Variation in Mammalian Populations.
BIEMOND A. - Brain Diseases.
BLACK R. M. - The Elements of Palaeontology.
BOURNE G. H. (ed.) - The Chimpanzee.
BROWN W. F. - Principes de ferromagnétisme.
BRUTER C. P. - Les matroïdes.
BURROW J. W. - Evolution and Society.
BUTTERFASS T. - Wachstums- und Entwicklungsphysiologie der Pflanze.
CAIRNS-SMITH A. G. - The Life Puzzle.
CAMPEDELLI L. - La geometria dei parallelogrammi.

Chinn W. G. and Steenrod N. E. - Topologie élémentaire.

Clapp L. B. - La chimie du groupe H0.

Conte R. R. - Eléments de cryogénie.

Crook J. H. (ed.) - Social Behaviour in Birds and Mammals.

Currey J. D. - Animal Skeletons.

Dacunha-Castelle D., Revuz D. and Schreiber M. - Recueil de problèmes de calcul des probabilités.

Daniel J. C. Jr. (ed.) - Methods in Mammalian Embryology.

Davies R. D. and Smith F. G. (eds.) - The Crab Nebula.

Degos R. and Lortat-Jacob E. - Dermatologie.

Dickson J. H. (ed.) - Optical Instruments and Techniques 1969.

Dorst J. and Dandelot P. - A Field Guide to the Larger Mammals of Africa.

Glansdorff P. and Prigogine I. - Structure, stabilité et fluctuations.

Gougenheim G. - Études de grammaire et de vocabulaire français.

Grattan-Guinness I. - The Development of the Foundations of Mathematical Analysis from Euler to Riemann.

Green D. E. and Goldberger R. F. - Molekulare Prozesse des Lebens.

Grigorov G. and Kanev V. - Le vide poussé.

Guntrip H. - Struttura della personalità e interazione umana.

Hahn R. - The Anatomy of a Scientific Institution.

Hall V. E., Giese A. C. and Sonnenschein R. (eds.) - Annual Review of Physiology.

Halstead L. B. - The Pattern of Vertebrate Evolution.

Hanson N. R. - Patterns of Discovery.

Harris H. and Hirschhorn K. (eds.) - Advances in Human Genetics.

Helfrich H. W. Jr. (ed.) - Agenda for Survival.

Hennequet A. - Mémento de pédiatrie pratique.

Heslop R. B. - Numerical Aspects of Inorganic Chemistry.

Higgins L. G. and Riley N. D. - A Field Guide to the Butterflies of Britain and Europe.

Highnam K. C. and Hill L. - The Comparative Endocrinology of the Invertebrates.

Hilpinen R. (ed.) - Deontic Logic: Introductory and Systematic Readings.

Hoeschele D. F. Jr. - Techniques de conversion analogique-digitale et digitale-analogique.

Hoffman I., Hamby R. I. and Glassman E. (eds.) - Vectorcardiography 2.

Höhler G. (ed.) - Springer Tracts in Modern Physics.

Hoogerwerf A. - Udjung Kulon.

Hyvärinen L. P. - Information Theory for Systems Engineers.

Introduzione alla scienza fisica.

Järnåker J. - The Double Solution of the Theory of Relativity.

Jessop N. M. - Biosphere.

Kämpfe L., Kittel R. and Klapperstück J. - Leitfaden der Anatomie der Wirbeltiere.

Katz B. - Nervi, muscoli e sinapsi.

Kingdon J. - East African Mammals.

Kojima K. (ed.) - Mathematical Topics in Population Genetics.
Krauth L. - Die Philosophie Carnaps.
Lacadena J.-R. - Genética vegetal.
Lang S. - Introduction to Linear Algebra.
Larrauri I. - Dizionario di otorinolaringologia.
Larson D. B. - Quasars and Pulsars.
Ledbetter M. C. and Porter K. R. - Introduction to the Fine Structure of Plant Cells.
Levy P. - Quelques aspects de la pensée d'un mathématicien.
Limoges C. - La sélection naturelle.
Lippard S. J. (ed.) - Progress in Inorganic Chemistry.
Longmore D. - Chirurgie et organes de rechange.
Luyten W. J. (ed.) - White Dwarfs.
Meylan S. - Bioélectricité.
Mihalas D. (P. Routly) - Galactic Astronomy.
Nouvelles données scientifiques sur le massif du Tsaratanana (Madagascar).
Palomba G. - Morfologia economica.
Parsons R. T. (ed.) - Windows on Africa.
Pecker J.-C. - Space Observatories.
Picard H. - Utilisation thérapeutique des oligo-éléments.
Pierre-Noel L. - Manuel de santé publique.
Pistor M. - Les micro-injections de la Mésothérapie.
Pointeau R. and Bovastre J. - Eléments de polarographie.
Quintin M. - Electrochimie.
Raguin E. - Pétrographie des roches plutoniques dans leur cadre géologique.
Răznjević K. - Tables et diagrammes termodynamiques.
Reichle D. E. (ed.) - Analysis of Temperate Forest Ecosystems.
Reyment R. A. - Introduction to Quantitative Paleoecology.
Rocard J.-M. - Mécanique.
Rotberg R. I. (ed.) - Rebellion in Black Africa.
Roy R. S. and Laliberte-Robert L. - Travaux pratiques de microbiologie.
Schweitzer J. - Ordinateurs et comportement conscients?
Sciama D. W. - Modern Cosmology.
Science and Synthesis.
Simon J.-C. - Introduction au fonctionnement des ordinateurs.
Staniland A. C. - Patterns of Redundancy.
Stecker F. W. - Cosmic Gamma Rays.
Synge J. L. - Talking about Relativity.
Tetau M. and Bergeret C. - L'organothérapie.
Tortrat A. - Calcul des probabilités et introduction aux processus aléatoires.
Tusques J. - Organisation nerveuse et sensorielle de l'homme.
Tymieniecka A.-T. (ed.) - Analecta Husserliana.
Vallée R. L. - L'énergie électromagnétique matérielle et gravitationnelle.
Valnet J. - Aromathérapie.
Valnet J. - Traitement des maladies par les légumes, les fruits et les céréales.
Vaughan P. - Pro e contro la pillola.

VERNON M. D. - Reading and its Difficulties.
VIÉNOT J.-C., SMIGIELSKI P. and ROYER H. - Holographie optique.
WENDLING E. - Problèmes de chimie minérale physique.
WEYL H. - Raum, Zeit, Materie.
World Survey of Climatology.
YOURGRAU W. and BRECK A. D. (eds.) - Physics, Logic, and History.
ZEMANN J. - Chimie cristalline.

AMSTUTZ, G. C., **Glossary of Mining Geology** (In English, Spanish, French and German), Elsevier Publishing Company, Amsterdam, 1971, XII+196 pp., figs., Dfl. 48.00.

The *Glossary of Mining Geology* is intended to fill a gap existing in the field of economic geology. It can be used as a reference book and as a short introduction to foreign terminology. It contains about two hundred terms which are defined in English, Spanish, French and German. Whenever a genetic meaning is involved an attempt has been made to word it according to a modern approach or to remove onesided connotations which might lead the observer to form premature opinions before he understands the context of an ore deposit. A complete index in four languages and an extensive appendix increase the value of the Glossary as a reference book.

ASIMOV, I., **Une particule fantôme: Le Neutrino,** Dunod, Paris, 1970, XIV+168 pp., figs., 24 F.

CONTENTS. - Introduction: *L'univers, cette énigme.* - *I: La quantité de mouvement.* - *II: L'énergie.* - *III: La structure atomique.* - *IV: La masse-énergie.* - *V: La charge électrique.* - *VI: Les antiparticules.* - *VII: Apparition du neutrino.* - *VIII: Détection des neutrinos.* - *IX: L'astronomie du neutrino.* - *X: Le champ nucléaire.* - *XI: Les muons.* - *XII: Le neutrino et le muon.* - Annexe: *Les nombres exponentiels.*

ASTRUC, L., **Créativité et sciences humaines,** Librairie Maloine S.A., Paris, 1970, 420 pp., 48 F.

Cet ouvrage s'adresse aux psychologues, aux philosophes et aux psychiatres. En un premier temps, l'auteur dégage une structure psychique spécifique. Chez certains sujets le désir n'est pas orienté vers la conquête de l'objet mais vers la jouissance de ce qui est acquis. Il en résulte une attitude défensive plutôt qu'agressive, une certaine passivité favorable à l'intégration prolongée des perceptions, un surmenage anxieux plutôt qu'une assimilation au groupe, un besoin de tout construire par soi-même au lieu de se mimétiser. Les théories psychosomatiques sur l'adaptation et les recherches actuelles sur le rôle des perceptions, sur les fonctions créatrices et sur l'imagerie mentale trouvent tout naturellement leur justification dans ce cadre. Par ailleurs, la créativité n'apparaît plus comme une aptitude intellectuelle commune qu'il suffit de développer mais comme une production mentale de signification très différente selon la structure psychique.

AVRAM, M. and MATEESCU, Gh. D., **Spectroscopie infrarouge** (Applications en chimie organique), Dunod, Paris, 1970, 642 pp., figs. and tables, 215 F.

La spectroscopie infrarouge est actuellement utilisée pour l'étude des relations entre la structure, la réactivité et les propriétés physiques des molécules, qui joue aujourd'hui

un rôle essentiel dans la recherche en chimie organique. Cette technique très moderne est indispensable aussi bien à la recherche fondamentale qu'à la recherche destinée aux applications pratiques dans les domaines les plus divers: chimie, physique, biologie, médecine, pharmacie, cosmonautique, astronomie, etc. Les auteurs de ce livre présentent tous les aspects théoriques et pratiques de cette discipline. La dernière partie, qui est la plus importante, porte sur les applications de la spectrochimie infrarouge en chimie organique. En particulier, les indications nécessaires pour déterminer les structures moléculaires et pour contrôler la pureté et l'analyse quantitative des substances organiques y sont données. Les très nombreux spectres infrarouges présentés dans cette dernière partie sont classés d'après les fonctions chimiques et l'on peut établir ainsi les relations entre les structures chimiques et les spectres infrarouges des corps envisagés. Comportant une abondante bibliographie et illustré de nombreuses tables et figures, cet ouvrage intéressera tous les chimistes et ingénieurs-chimistes travaillant dans les laboratoires de recherche. Son caractère didactique devrait également le rendre très utile aux étudiants en chimie de l'Université et des grandes écoles.

BALDRY, P. E., **The Battle Against Heart Disease** (A Physician Traces the History of Man's Achievements in this Field for the General Reader), Cambridge University Press, London, 1971, x+190 pp., figs., £3.00.

This book describes, in historical sequence, the progress in understanding the heart's action, the diagnosis of its disorders and their treatment from the time of Hippocrates to the present day. In an early part of the book a simple up-to-date account of the structure and function of the heart is given for the information of the general reader. The subjects covered include: the development in knowledge concerning the major diseases affecting the heart, including rheumatic fever, syphilis, bacterial infection of the valve, disorders of rate and rhythm, high blood pressure, coronary thrombosis and congenital heart disease. The various forms of congenital heart disease are explained in simple terms so that such expressions as 'hole in the heart' and 'blue babies' much used by modern journalists may be properly understood. Accounts are given of the invention and use of the stethoscope; of the application of discoveries in electricity to the diagnosis of heart disease, and of the steps which led to the discovery of important drugs used in treatment. The final chapter describes progress in surgery during the past forty years and ends with a discussion of the technique and ethics of heart transplantation.

BARRY, J.-M. and BARRY, E.-M., **Eléments de biochimie structurale,** Masson & C.ie, Paris, 1971, 184 pp., figs. and tables, 36 F.

Les auteurs s'attachent essentiellement à montrer que les réactions biologiques, au niveau des molécules, exigent de préférence certaines structures moléculaires plutôt que d'autres, le comportement des produits organiques, de composition chimique identique, pouvant varier da façon très sensible, si leurs structures moléculaires diffèrent tant soit peu. Les premiers chapitres de l'ouvrage rappellent les principes structuraux moléculaires, les méthodes permettant de déterminer les structures, les règles adoptées pour la dénomination des molécules organiques suivant leurs structures moléculaires. Les chapitres suivants étudient les principes structuraux moléculaires des hydrates de carbone, protéines, acides nucléiques, lipides, cires, stérols et molécules biologiques diverses. L'ouvrage a été conçu en vue de préparer le lecteur à l'étude des structures des molécules biologiques. L'ouvrage comporte, au terme de chaque chapitre, une bibliographie succincte, mais suffisante pour permettre à ceux qui le désirent de pousser plus loin leurs études.

BEER, S., **La science de la gestion** (La recherche opérationnelle dans l'entreprise) (Series: *Techniques d'aujourd'hui*), Larousse, Paris, 1970, 192 pp., figs., 14,50 F.

Cet ouvrage expose le développement, les méthodes et les résultats du traitement scientifique des problèmes de gestion des grandes entreprises privées ou publiques, civiles ou militaires. L'ouvrage constitue une excellente initiation à cette science nouvelle.

CONTENTS. - *I: Diriger c'est choisir.* - *II: Hasard, risque et malveillance.* - *III: L'intuition et les chiffres.* - *IV: Un ABC des modèles.* - *V: Deux exemples.* - *VI: Le régulateur organique.* - *VII: Problèmes de l'automation.*

BELL, P. R. and WOODCOCK, C. L. F., **The Diversity of Green Plants,** 2nd ed. (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1971, x+374 pp., figs., £5.00.

The Diversity of Green Plants has been written in the conviction that never before has the science of botany had so much to offer the enquiring student. The constant emphasis on evolution and natural selection is what distinguishes this volume. Frequent reference to paleobotanical evidence is made, and the development towards more complex structures is related throughout to possible selective advantages. The authors stress that although they have adopted a form of classification as a framework for investigation, they have thought of the autotrophic plants as a whole. In this second edition there are a number of additional illustrations. Also the sections on the algae (Cyanophyta and Chlorophyta), mosses and liverworts (Bryophyta), conifers (Gymnospermae), and flowering plants (Angiospermea), have been substantially revised.

BERRY, R. J. and SOUTHERN, H. N. (eds.), **Variation in Mammalian Populations** (Symposia of the Zoological Society of London, No. 26), Academic Press, London, 1970, XVI+404 pp., figs. and tables, £6.00.

This book consists of papers by biologists of different disciplines, techniques and interests in mammal population studies who were present at a symposium held by The Zoological Society of London and The Mammal Society. It is chiefly concerned with substantiating the considerable inherent variety in mammalian populations, and it seeks to explore some of the implications which are important for our understanding of man as well as other mammals. The systematic problems of interpopulation differences, and the way in which the genetical constitution of mammals alters in response to changes in population or environment are also discussed, as are the more immediately practical topics of (for instance) the resistance level of rats to poison and other matters of pest control.

BIEMOND, A., **Brain Diseases,** Elsevier Publishing Company, Amsterdam, 1970, XXII+880 pp., figs., Dfl. 200.

This volume is the translation of a Dutch book published in 1946, and reprinted several times, which has been brought up to date. It is based on clinical symptoms observed by the author and his staff as compared with findings subsequently acquired during autopsy. These observations include about 1000 clinical anatomical cases, as well as several hundred surgical cases, most of them relating to brain tumours. Therapy is discussed in its broadest sense. Here too, the material is based mainly on the author's own substantiated findings. Modern mechanical diagnostic methods - although often indispensable - tend to restrict the role of the clinical neurologist. On the basis of extensive anatomically verified material the author proves that, without the use of technical devices, carefully elaborated case histories and simple well considered diagnoses may give reliable results. In any case, an indication is given of the particular methods of examination that would ensure diagnostic safety. The book consists of a general and a special part. The former deals with the well-known cranial nerves, complete with the pertinent anatomical and physiological data, while the latter treats the pathology of the brain. Via a detailed index, information concerning the rapid solving of diagnostic problems, can easily be found. This book will be an extremely valuable aid in the diagnostic therapy of brain diseases. Of interest to: Neurologists, neurosurgeons, neuropathologists, specialists in internal medicine, general practitioners, medical students, neuroclinical researchers and ear specialists.

BLACK, R. M., **The Elements of Palaeontology,** Cambridge University Press, London, 1970, VIII+340 pp., figs., £4.50.

A textbook of palaeontology for students in schools, colleges and universities who require a knowledge of the subject at an introductory level. The book covers vertebrate, invertebrate and plant fossils. Over 200 examples are described; virtually all forms mentioned by name are illustrated. All the major groups of invertebrate fossils are described and an outline of the palaeontology of the vertebrates and plants is included. Particular attention is given to the biological aspects of palaeontology. The book is illustrated with photographs and drawings. Many of the photographs and nearly all the drawings have been specially prepared for this volume under the supervision of the author, and the pictures complement and reinforce the descriptions given in the text. The autbor has had considerable experience as a lecturer and examiner in palaeontology.

CONTENTS. - I. Introduction. *I: Introduction. - II: Preservation of Fossils. - III: Occurrence of Fossils. - IV: Fossils as Indices of Environment.* - II. The Biology of Fossils and Their History. *V: Foreword to the Study of the Different Groups of Fossils. - VI: Mollusca, Class Bivalvia. - VII: Mollusca, Class Gastropoda. - VIII: Mollusca, Class Cephalopoda. - IX: Echinodermata, Class Echinoidea. - X: Echinodermata, Class Crinoidea and Minor Echinoderms. - XI: Brachiopoda. - XII: Coelenterata. - XIII: Arthropoda. - XIV: Graptolithina. - XV: Porifera, Bryozoa and Annelida.- XVI: Microfossils. - XVII: Vertebrata, Fish. - XVIII: Vertebrata, Tetrapods. - XIX: Plants. - XX: Epilogue: Evolution and the Origin of Life.*

BOURNE, G. H. (ed.), **The Chimpanzee,** VOL. 4: **Behavior, Growth, and Pathology of Chimpanzees,** S. Karger, Basel, 1971, VIII+408 pp., figs. and tables, sFr. 120.

Volume 4 of *The Chimpanzee* adds further chapters to the growth and anatomy of this animal, and makes contributions also to its diet, its pathology and its intelligence. In addition there are studies on the placenta, on the dermatoglyphics and the sex behavior of nursery reared captive-born chimpanzees. A distinguished group of authors have contributed this volume and they include R. Baur, J. Biegert, G. H. Bourne, J. A. Gavan, N. B. Guilloud, K. S. Ludwig, H. M. McClure, W. Platzer, A. H. Riesen, and D. M. Rumbaugh.

BROWN, W. F., **Principes de ferromagnétisme** (Series: *Monographies Dunod,* No. 79), Dunod, Paris, 1970, XII+252 pp., figs., 29 F.

Dans ce livre les définitions du champ sont d'abord données dans le vide (interaction de courants) puis étendues à la manière aimantée en montrant que si les vecteurs excitation et induction magnétiques ont un rôle identique dans le vide, il n'en est plus de même dans les corps magnétiques: ces deux vecteurs sont alors nécessaires pour la description physique complète du milieu magnétique. Ce livre montre aussi que l'utilisation du vecteur aimantation pour cette description, conduit à une formulation mathématique plus simple dans le cas du ferromagnétisme. Le point de vue énergétique est également étudié et les énergies sont définies par leur sens thermodynamique, ce qui permet d'écrire correctement les équations d'équilibre. C'est alors seulement qu'est introduite la notion de domaine magnétique.

BRUTER, C. P., **Les matroïdes** (Nouvel outil mathématique), (Series: *Initiation aux Nouveautés de la Science,* No. 21), Dunod, Paris, 1970, XII+192 pp., figs., 38 F.

Cet ouvrage présente croissante toutes les notions fondamentales, ensemble, ordre, fonction. Les exemples cités appartiennent d'abord au domaine du concret. Les graphes sont ensuite introduits de façon naturelle en montrant par des exemples l'éventail de leurs usages possibles, ainsi que les treillis. Une partie du livre est consacrée à l'algèbre linéaire, outil premier du mathématicien, basée sur la méthode qui consiste à raisonner par vrai ou faux, existe ou n'existe pas. Les groupes, anneaux, corps, espaces vectoriels sont ainsi sommairement étudiés.

BURROW, J. W., **Evolution and Society** (A Study in Victorian Social Theory), Cambridge University Press, London, 1970, XXVI+294 pp., £0.65.

CONTENTS. - *I: Introduction. - II: A Knowledge of Human Nature. - III: The Reasoned History of Man. - IV: The Laws of Nature and the Diversity of Mankind. - V: Sir Henry Maine. - VI: Herbert Spencer. - VII: Tylor and the Growth of Anthropology. - VIII: Conclusion.*

BUTTERFASS, T., **Wachstums- und Entwicklungsphysiologie der Pflanze,** Quelle & Meyer, Heidelberg, 1970, 242 pp., figs. and tables, DM 17.

CONTENTS. - *I: Grundlagen.* - *II: Wachstum.* - *III: Entwicklung und Umwelt.* - *IV: Ruhezustände und Reaktivierungen.* - *V: Embryonalisierung und Regeneration.* - *VI: Einige Formbildungen an vegetativen Organen.* - *VII: Die Generative Entwicklung.* - *VIII: Der Generationswechsel.* - *IX: Die Abstossung von Organen.*

CAIRNS-SMITH, A. G., **The Life Puzzle** (On Crystals and Organisms and on the Possibility of a Crystal as an Ancestor), Oliver and Boyd, Edinburg, 1971, VIII+166 pp., figs. and plates, £1.95.

What is the origin of life? Molecular biology shows us one kind, but in thinking about it we must consider those generalised aspects of living organisation that are common to all conceivable forms of life. The author believes that only a combination of general biological theory and particular chemical knowledge can solve the problems of the origin and re-creation of life. This book does not fall into either the classical Haldane or Oparin schools of thought on the origin of life, but advances a thesis of its own, which, according to Professor C. H. Waddington, is one of the most important recent intellectual developments in this field. Part I considers the role of molecular biology in formulating a view of life as it exists now. Part II turns to more general aspects of the organisation of matter. In Part III the author advances his own theories on the origin of life—theories which are both revolutionary and reactionary. As he remarks 'If my conclusions are correct it may be difficult to find the right system, but it would be easy to make a very simple organism once we have'.

CAMPEDELLI, L., **La geometria dei parallelogrammi** (Series: *La Matematica nella Cultura e nella Scuola*, No. 2), Felice Le Monnier, Firenze, 1970, XII+204 pp., figs., L. 3.200.

CONTENTS. - *I: Sulla struttura logica della geometria.* - *II: I triangoli omologici.* - *III: Il piano affine.* - *IV: L'uguaglianza per traslazione.* - *V: La scala grafica.* - *VI: L'omotetia.* - *VII: L'affinità omologica.* - *VIII: Il gruppo delle affinità.* - *IX: Le pararotazioni.* - *X: Le paracongruenze.* - *XI: Nel piano metrico.* - *XII: Archi e angoli.*

CHINN, W. G. and STEENROD, N. E., **Topologie élémentaire** (Géométrie des fonctions de segments, de courbes, de cercles et de disques), Dunod, Paris, 1970, VI+186 pp., figs., 26 F.

Le présent ouvrage tente de montrer pourquoi la topologie a été créée et de présenter certains de ses aspects et de ses applications les plus simples. L'accent est mis notamment sur les théorèmes d'existence qui permettent de savoir s'il y a une solution à un problème avant même de la calculer. La première partie de ce livre est consacrée à la topologie de la droite autour du théorème de la valeur intermédiaire et à l'existence de solutions à l'équation: $y = ax$; la démonstration de ce théorème permet de mettre en évidence comment naît la topologie et à quoi elle sert. La seconde partie traite de la topologie du plan. Le théorème qu'elle démontre est une généralisation du précédent dans un espace à deux dimensions. Dans les deux cas étudiés, les auteurs concluent par un théorème d'équipartition - partage en parties égales de figures topologiques - et donnent ainsi l'un des résultats fondamentaux de la recherche actuelle.

CLAPP, L. B., **La chimie du groupe HO** (Series: *Fondements de la chimie moderne*, No. 7), Dunod, Paris, 1970, XII+138 pp., figs. and tables, 28 F.

Ce livre expose d'une part les propriétés essentielles d'un groupe très répandu: le groupe hydroxyle. Dans son étude de l'eau et des alcools, l'auteur s'attache plus particulièrement à l'influence du voisinage sur les propriétés du groupe, ainsi qu'aux réactions de condensation entre molécules contenant des OH. Les considérations relatives au groupe OH sont ensuite généralisées au groupe NH_2 et l'ouvrage se termine par une analyse, en géométrie à trois dimensions, de la structure des composés hydroxyles. Cet ouvrage devrait intéresser les étudiants en chimie et ingénieurs chimistes. Il devrait répondre en outre à la plupart des questions qu'un industriel ou un chercheur de laboratoire peut rencontrer dans son travail.

CONTE, R. R., **Eléments de cryogénie,** Masson & C.ie, Paris, 1970, x+332 pp., figs. and tables, 130 F.

CONTENTS. - *I: Historique et développements actuels. - II: Étude des cryostats. - III: Propriétés mécaniques des matériaux. - IV: Mesure et régulation de température. - V: Transfert des liquides cryogéniques. - VI: Production de températures inférieures à 4,2 °K.*

CROOK, J. H. (ed.), **Social Behaviour in Birds and Mammals** (Essays on the Social Ethology of Animals and Man), Academic Press Inc., London, 1970, XL+492 pp., figs. and tables, £7.00.

The authors stress the multiplicity of factors governing the performance of complex social behaviour and the gradual programming of each individual's development. The essays have been presented in three deliberately ordered sections dealing first with the adaptive significance of social structure, secondly with processes occurring in differing kinds of animal and human social relationships, and thirdly with developmental problems. While this book is primarily intended for university students in ethology, animal behaviour, psychology, anthropology and sociology, it is also of interest to the general reader who wants to evaluate popular ideas concerning the social ethology of animals and man.

CONTENTS. - I. Ecology and Behaviour. *I: Feeding Dispersion in Some Overwintering Wading Birds*, by J. D. GOSS-CUSTARD. - *II: Ecological Determinants of Breeding Adaptations and Social Behaviour in Two Fish-eating Birds*, by K. E. L. SIMMONS. - *III: Problems of Social Structure in Forest Monkeys*, by F. P. G. ALDRICH-BLAKE. - *IV: The Socio-ecology of Primates*, by J. H. CROOK. - II. Social Processes. *V: Effects of Population Density on Behaviour in Rodents*, by J. ARCHER. - *VI: Gender Role in the Social System of Quelea*, by J. H. CROOK and P. A. BUTTERFIELD. - *VII: The Pair Bond in the Zebra Finch*, by P. A. BUTTERFIELD. - *VIII: Communication by Facial-Visual Signals*, by I. VINE. - III. Problems in Behavioural Development. *IX: The Adaptive Radiation of Parental Care in Waterfowl*, by J. KEAR. - *X: Perceptual Factors and Observational Learning in the Behavioural Development of Young Chicks*, by E. CRONHELM. - *XI: Visual Experience and Early Social Behaviour in Chicks*, by S. J. DIMOND.

CURREY, J. D., **Animal Skeletons** (Series: *Studies in Biology*, No. 22), Edward Arnold (Publishers) Ltd., London, 1970, VI+52 pp., figs. and plates, £0.75.

In the past thirty years or so the study of skeletons has usually been limited to the study of their growth and their physiological relations with the rest of the body. To a large extent the mechanical aspects of their function have been taken for granted. Recently, however, probably because of advances that have been made in the manufacture of man-made materials and in the understanding of their properties, there has been an increase in the study of skeletons considered as mechanical objects. This book treats animal skeletons mainly from this point of view.

DACUNHA-CASTELLE, D., REVUZ, D. and SCHREIBER, M., **Recueil de problèmes de calcul des probabilités,** 2nd rev. ed., Masson & C.ie, Paris, 1970, XII+234 pp., figs., 48 F.

Ce recueil comporte douze chapitres couvrant les parties habituellement enseignées dans la plupart des cours. Certains énoncés ont été donnés aux examens de Calcul des Probabilités de la Faculté des Sciences de Paris, d'autres plus classiques visent à construire des contre-exemples à des propositions fausses, d'autres enfin servent à introduire des résultats débordant le cadre d'un cours, mais utiles à la résolution de certains problèmes. Chaque chapitre comporte quelques énoncés simples pour se familiariser avec les notions nouvelles et des problèmes difficiles pour ceux qui veulent aller plus loin. Les énoncés sont approximativement classés par ordre de difficulté croissante à l'intérieur de chaque chapitre. Les corrigés proposés sont en général assez détaillés.

CONTENTS. - *I: Problèmes combinatoires. - II: Théorie de la mesure. - III: Fonctions génératrices. - IV: Lois et fonctions caractéristiques. - V: Convolution. - VI: Probabilités conditionnelles. - VII: Lois normales. - VIII: Convergence en loi. - IX: Loi des grands nombres et convergence des variables aléatoires. - X: Processus de Poisson. - XI: Chaînes de Markov. - XII: Fonctions du second ordre et processus gaussiens.*

DANIEL, J. C., Jr. (ed.), **Methods in Mammalian Embryology,** W. H. Freeman and Company, Berkshire, 1971, XVI+532 pp., figs. and tables, £10.70.

Prepared specifically for use by investigators in experimental mammalian embryology, this handbook presents detailed discussions or reliable methods that previously were incompletely described or scattered throughout the literature. The methods detailed here by their developers—some of the most successful current techniques used in experimental mammalian embryology—extend the limits of this newly emerging field by ensuring that potential researchers can make proper applications of the methods appropriate for their research. The contributors emphasize the newest techniques developed through work with both laboratory and domestic animals and minimize description of biochemical, physiological, histological, and endocrinological techniques already in common use. The thirty-four chapters are arranged in a sequence that parallels stages of mammalian development. They begin with methods for studying the production of gametes and for gamete collection, range through methods for studying fertilization, cleavage, blastocyst and fetal stages (with attention to the properties and functions of both embryonic and extraembryonic special tissues), and culminate with techniques helpful in the culture and handling of embryonic organs.

DAVIES, R. D. and SMITH, F. G. (eds.), **The Crab Nebula** (I.A.U. Symposium No. 46, held at Jodrell Bank, England, August 5-7, 1970), D. Reidel Publishing Company, Dordrecht, 1971, XX+470 pp., figs. and tables, Dfl. 87.

This book is a record of the proceedings of the International Astronomical Union Symposium No. 46, held at Jodrell Bank, England, in August 1970. It contains the contributed papers and a full summary of the discussion which followed each contribution. The symposium brought together recent observational data on the Crab nebula and its pulsar and discussed the interpretation of this material in relation to other supernova remnants and pulsars. The Crab nebula and its pulsar are unique in having been observed over the entire electromagnetic spectrum from radio wavelengths through infrared and optical wavelengths to X-rays and γ-rays. Astronomers from each of these experimental disciplines made contributions. These included descriptions of detailed structure in the optical and radio emission of the Crab nebula and its polarization. Particular attention was given to recent measurements of the pulse shape and polarization and of the pulse period of the Crab pulsar. Two sessions of the symposium were devoted to a comparison of the Crab nebula and its pulsar with other supernova remnants and pulsars.

DEGOS, R. and LORTAT-JACOB, E., **Dermatologie,** 8th ed., (Series: *Les Petits Précis*), Librairie Maloine, S.A., Paris, 1971, XII+430 pp., 36 F.

Cette huitième édition n'est pas une réimpression avec quelques adjonctions, mais une recomposition de tous les chapitres, effectuée par le Professeur Degos. Plusieurs sujets, comme les purpuras, les infiltrats hypodermiques, les angiomes, les xanthomes ont été complètement remaniés. Cette édition réalise une mise à jour de l'essentiel des connaissances sur les principaux problèmes cliniques, étiopathogéniques et thérapeutiques concernant les différentes dermatoses, ainsi que sur les rapports de ces affections cutanées avec des troubles généraux, métaboliques et immunologiques récemment précisés.

DICKSON, J. H. (ed.), **Optical Instruments and Techniques 1969,** Oriel Press Ltd., Newcastle upon Tyne, 1970, XIV+562 pp., figs. and tables, £7.00.

Proceedings of the Conference held at the University of Reading during July 1969 under the auspices of the International Commission for Optics and arranged by the Optical Sub-Committee of the British National Committee for Physics. The book is divided into six parts: I. New techniques and instruments for spectroscopy, including modern interferometric methods, stimulated emission and non-linear phenomena. II. Recent developments in optical production techniques, including the use of new materials. III. Optical metrology and the optical processing of data, including coherent light techniques. IV. Advances in assessment and specification of the performance of optical instruments. V. Image-forming systems of novel design. VI. Systems design of astronomical instruments. These Proceedings reflect the advances which have been made in optics in recent years, particularly due to the invention of the laser and the increasing influence of computer technology. Contributions come from 15 countries throughout the world.

DORST, J. and DANDELOT, P., **A Field Guide to the Larger Mammals of Africa,** Collins, London, 1970, 288 pp., figs. and plates, £2.50.

The mammals of Africa south of the Sahara comprise the most important and spectacular fauna on earth. This is an illustrated guide to all of them (save only bats and the smallest, generally nocturnal, species). There is a description of every species, with measurements and details of form, colour, markings and comparison with similar species. For each species are also given its various names, notes on its habitat, behaviour and habits—feeding, breeding, calls, social habits, migration, enemies—on variation within the species and the points distinguishing principal groups or subspecies. There is a distribution map for every species in the book (except the Swamp Cat). Every species is illustrated—six in line drawings, all others in colour plates. These have been done specially for the book by Pierre Dandelot and use the famous Peterson identification system, with bars to indicate important characters and explanatory notes opposite each plate.

GLANSDORFF, P. and PRIGOGINE, I., **Structure, stabilité et fluctuations,** Masson & C.ie, Paris, 1971, 288 pp., figs. and tables, 70 F.

CONTENTS. - I. Théorie générale. *I: Lois de conservation et bilans. - II: Le second principe de la thermodynamique et le bilan entropique. - III: Thermodynamique linéaire des phénomènes irréversibles. - IV: Théorie de la stabilité de l'équilibre thermodynamique par la méthode de Gibbs-Duhem. - V: Théorie générale de la stabilité de l'équilibre thermodynamique. - VI: Conditions de stabilité thermodynamique et hydrodynamique des systèmes hors d'équilibre. - VII: Forme explicite des conditions de stabilité des processus hors d'équilibre. - VIII: Stabilité et fluctuations. - IX: Le critère général d'évolution.* - II. Méthodes variationnelles et applications hydrodynamiques. *X: Le potentiel local. - XI: Problèmes de stabilité dans les fluides au repos. - XII: Application du potentiel local à des problèmes de stabilité de mouvements laminaires. - XIII: Stabilité des ondes d'amplitude finie.* - III. Systèmes chimiques. *XIV: La notion d'ordre dans les réactions chimiques homogènes. - XV: Ordre spatial et dissipation dans les réactions chimiques non homogènes. - XVI: Etats stationnaires multiples. - XVII: Unité des lois physiques et niveaux de description.*

GOUGENHEIM, G., **Études de grammaire et de vocabulaire français,** Editions A. & J. Picard, Paris, 1970, XVI+432 pp., tables, No price stated.

CONTENTS. - I. Grammaire. *I: Structure de la phrase et emploi des prépositions. - II: Syntaxe des pronoms. - III: Syntaxe du verbe. - IV: Grammaire et stylistique.* - II. Vocabulaire. *I: Principes d'étude du vocabulaire. - II: Moyen age. - III: XVIe siècle et temps modernes.*

GRATTAN-GUINNESS, I., **The Development of the Foundations of Mathematical Analysis from Euler to Riemann,** The M.I.T. Press, London, 1970, XIV+186 pp., £4.65.

This book recounts the efforts of a glorious mathematical century—which included the Bernoullis, Taylor, Euler, Gauss, d'Alembert, Cauchy, Lagrange, Bolzano, Fourier, Poisson, Abel, and Riemann—to restructure the calculus into what is now known as "mathematical analysis", with at times much less rigor and clarity than has generally been supposed. The book takes up the specific problems that motivated these developments, which by the early decades of the nineteenth century had led to a broadening of the subject to embrace theories of functions, continuity, limits, and the uniform and nonuniform convergence of series (including Fourier series) as well as the formation and use of the derivative and integral. The study ends with the work of Riemann on the foundations of the calculus, for it was other ideas of his and their consequences that led to an entirely new era in analysis, in which set theory and measure theory were the most important components.

GREEN, D. E. and GOLDBERGER, R. F., **Molekulare Prozesse des Lebens,** Springer-Verlag, Berlin, 1971, x+240 pp., figs. and tables, DM 38.

CONTENTS. - *I: Einführung in die Chemie des Lebens. - II: Atome und Moleküle der Zelle. - III: Makromoleküle. - IV: Enzyme. - V: Enzyme, Spurenelemente, Coenzyme. - VI: Der Energiehaushalt. - VII: Energieliefernde biochemische Prozesse. - VIII: Energieabhängige Synthesen. - IX: Energieübertragungen. - X: Die Zellmembranen. - XI: DNS, RNS und Proteinsynthese. - XII: Kontrollmechanismen lebender Systeme. - XIII: Biochemie und Krankheit. - XIV: Pharmaka und Gifte. - XV: Universelle biochemische Fakten. - XVI: Evolutionäre Prozesse und universelle Mechanismen. - XVII: Aspekte biochemischer Forschungen.*

GRIGOROV, G. and KANEV, V., **Le vide poussé** (Au laboratoire et dans l'industrie), Masson & C.ie, Paris, 1970, VIII+482 pp., figs. and tables, 160 F.

CONTENTS. - *I: Bases physiques de la technique du vide. - II: Moyens d'obtention du vide. Pompes à vide. - III: Mesure du vide. - IV: Installations de vide et technique du vide poussé.*

GUNTRIP, H., **Struttura della personalità e interazione umana** (Sintesi evolutiva della teoria psicodinamica), (Series: *Psicologia e Psichiatria*), Boringhieri Editore, Torino, 1971, XII+450 pp., L. 9.000.

Questo libro costituisce uno studio approfondito dell'evoluzione della teoria psicoanalitica che, dall'originaria impostazione neurobiologica e psicobiologica, è andata sempre più trasformandosi in una teoria dinamica della personalità, fondata su una filosofia dell'uomo particolarmente attenta ai problemi della sua individualità. Partendo dall'esame della teoria freudiana, con particolare riguardo all'analisi dell'Io e alle ipotesi metapsicologiche, e delle posizioni dei neofreudiani, si giunge a intravvedere la sintesi che, secondo l'autore, emerge nelle opere di Melanie Klein e di Fairbairn e si completa nella descrizione delle forme fondamentali — normali e patologiche — del rapporto interumano. La preparazione umanistica dell'autore lo rende particolarmente sensibile alle implicazioni filosofiche della psicoanalisi, che deve porsi come disciplina autonoma, trovando concetti e schemi propri, non derivandoli o trasponendoli dalle scienze naturali.

HAHN, R., **The Anatomy of a Scientific Institution** (The Paris Academy of Sciences, 1666-1803), University of California Press Ltd., London, 1971, XIV+434 pp., £5.70.

The Paris Academy of Sciences was the first in Western Europe created to accommodate scientific specialists by providing them with salaries, and the first to set national standards and exercise regulatory power. This book analyzes the evolution of the Academy during its most creative period of activity, from Louis XIV to Napoleon. The author is particularly concerned with the interaction of the Academy with society, and he applies a Parsonian model of social action theory to explain the tensions, conflict, and political cross-pressures that led to the dissolution of the Academy, and to its reestablishment and assumption of a new role during Napoleonic times.

HALL, V. E., GIESE, A. C. and SONNENSCHEIN, R. (eds.), **Annual Review of Physiology,** VOLUME 33, Annual Reviews Inc., Palo Alto (California), 1971, x+662 pp., figs. and tables, $10.00.

CONTENTS. - *I: Catecholamine Functions,* by J. AXELSSON. - *II: Actions of Vertebrate Sex Hormones,* by H. G. WILLIAMS-ASHMAN and A. H. REDDI. - *III: Kidney,* by J. ORLOFF and M. BURG. - *IV: Respiration: Alveolar Gas Exchange,* by J. PIIPER and P. SCHEID. - *V: Temperature Acclimation in Birds and Mammals,* by R. R. J. CHAFFEE and J. C. ROBERTS. - *VI: Central Nervous System: Afferent Mechanisms,* by R. W. RODIECK. - *VII: Nervous Function at the Cellular Level: Glia,* by A. LASANSKY. - *VIII: Peripheral Mechanisms of Vision,* by P. WITKOVSKY. - *IX: Physiology of Hearing,* by D. H. ELDREDGE and J. D. MILLER. - *X: Cyclic Nucleotides,* by J. G. HARDMAN, G. A. ROBINSON and E. W. SUTHERLAND. - *XI: Motor Mechanisms: The Role of the Pyramidal System in Motor Control,* by V. B. BROOKS and S. D. STONEY, JR. - *XII: Brain-Adenohy-*

pophysial Communication in Mammals, by F. E. YATES, S. M. RUSSELL and J. W. MARAN. - *XIII: The Regional Circulation*, by G. ROSS. - *XIV: Heart: Excitation and Contraction*, by E. A. JOHNSON and M. LIEBERMAN. - *XV: Feeding: Neural Control of Intake*, by B. G. HOEBEL. - *XVI: Comparative Physiology: Gas Exchange and Circulation in Fishes*, by K. JOHANSEN. - *XVII: Some Related Articles Appearing in Other "Annual Reviews"*. - Indexes.

HALSTEAD, L. B., **The Pattern of Vertebrate Evolution** (Series: *University Reviews in Biology*, No. 10), Oliver & Boyd, Edinburgh, 1969, XII+210 pp., figs., £1.50.

The author discusses a number of topics which illustrate the pattern of vertebrate evolution. He not only presents his personal views but shows that palaeontology, while absorbing for its own sake, should be an integral element in any disciplined approach to living things as they appear today. The early chapters deal with the establishment of a basic vertebrate plan. Emphasis is placed on the first vertebrates and many neglected aspects of them are considered, including details of the internal anatomy. The original roles and early evolution of the tissues, bone and dentine are discussed in sections based largely on the author's own researches. The following chapters focus attention on key points in vertebrate evolution, which are examined in the context of the conditions under which they took place. It is demonstrated that the major changes came about by pre-adaptation and conferred advantages in the immediate environment. Some topics covered include the origins of jaws and fins, the transition from water to land, the origin of the amniote egg, polyphyly and problems of clarification, the achievement of mammalian status and the evolution of food chains. In two final chapters the author describes the origin and evolution of man, and defines our unique contribution to evolution as the manufacture of weapons for further use. He attributes a number of basic human traits to our group-hunting predator ancestry, points out trends in contemporary society to which they have given rise and outlines their possible influence and development in the future.

HANSON, N. R., **Patterns of Discovery** (An Inquiry into the Conceptual Foundations of Science), Cambridge University Press, London, 1961, X+242 pp., figs., £2.50.

The book begins with an amplification of some of Wittgenstein's remarks on seeing. Early chapters consider in detail the differences between the ways in which two people may see the same thing and different things, the connection between seeking and knowing, the relation between facts and the language in which they can be stated, and the dependence of causal explanation on a background of theory. These topics provide the essential basis for an account of scientific hypotheses and their evolution, from the first surprising or anomalous observations to the surmounting of conceptual obstacles, and thence to the discernment of a pattern in the data. Theories, once developed, can be used in a variety of ways, each leading to some particular view of the nature of science: and any such difference in conceptual organization will lead to a difference in practice and expectation. This is illustrated in the final sections, in which the Author considers the still developing science of elementary particle physics.

HARRIS, H. and HIRSCHHORN, K. (eds.), **Advances in Human Genetics**, VOL. I, Plenum Press, New York, 1970, XII+340 pp., figs. and tables, $19.50.

CONTENTS. - *I: Analysis of Pedigree Data*, by J. H. EDWARDS. - *II: Autoradiography in Human Cytogenetics*, by O. J. MILLER. - *III: Genetics of Immunoglobulins*, by H. H. FUDENBERG and N. E. WARNER. - *IV: Human Genetics of Membrane Transport with Emphasis on Amino Acids*, by C. R. SCRIVER and P. HECHTMAN. - *V: Genetics of Disorders of Intestinal Digestion and Absorption*, by J. FRÉZAL and J. REY.

HELFRICH, H. W. Jr. (ed.), **Agenda for Survival** (The Environmental Crisis - 2), Yale University Press, London, 1970, XII+234 pp., £1.35.

CONTENTS. - Foreword, by F. MERGEN. - Preface, by H. W. HELFRICH, JR. - *I: Total Environment: A New Political Reality*, by S. L. UDALL. - *II: Human Nonproliferation: A Political Responsibility*, by E. BERMAN. - *III: DDT and the Environment*, by Ch. F. Wurster. - *IV: The Search for an Environmental Perspective*, by D. Brower. - *V: A Strategy for the Ghetto: The Philadelphia Story*, by H. J. Haskins. - *VI: Congress and Environmental Quality: Now or Never*, by P. N. McCloskey, Jr. - *VII: Social-Sector Industries: The Challenge of Our Conscience*, by D. Carley. - *VIII: Energy: Economics of the Environment*, by Ch. F. Luce. - *IX: Action Imperatives for Population Control: A Woman's View*, by S. Mills. - *X: Is Clean Air Possible in an Industrialized Society?*, by R. Cadle. - *XI: Modest Proposals for More Livable Cities*, by P. N. Ylvisaker. - *XII: Are Capitalism and the Conservation of a Decent Environment Compatible?*, by H. M. Caudill. - *XIII: This Generation's Strategy to Save the Environment*, by G. Nelson. - *XIV: The Creative Approach to a Quality Environment*, by L. Halprin. - *XV: Can Technology Rescue the Cities?*, by M. Safdie. - *XVI: Limits: The Environmental Imperative of the 1970s*, by S. L. Udall.

HENNEQUET, A., **Mémento de pédiatrie pratique,** Librairie Maloine S.A., Paris, 1971, 280 pp., figs. and tables, 36 F.

Dans ce petit ouvrage destiné aux étudiants en médecine comme aux puéricultrices ou aux pédiatres, l'auteur a rassemblé une somme de documents sur des sujets très divers: unités de mesure, repères de croissance intra et extra-utérins, notions d'embryologie, de physiologie, constantes biologiques, notions fondamentales sur l'eau et les électrolytes, épreuves fonctionnelles, radiologie, électrocardiographie, thérapeutique, diététique, etc. Par contre, l'ouvrage ne contient aucun texte se rapportant à la description de la pathologie: il ne s'agit donc pas d'un abrégé de pédiatrie, mais bien d'un mémento de pédiatrie pratique qui permettra de donner une réponse immédiate à des questions aussi différentes que la longueur du foetus de 27 semaines, le poids atomique du soufre, le volume débité en 24 heures par une perfusion réglée à 12 gouttes/m, la composition de la banane ou les règles de la digitalisation chez le nourrisson.

HESLOP, R. B., **Numerical Aspects of Inorganic Chemistry,** Elsevier Publishing Company, Amsterdam, 1970, VIII+190 pp., figs. and tables, Dfl. 13.00.

This book makes use of the international system of units (SI) in the treatment of numerical problems in inorganic chemistry. The system is a coherent, rationalised one, which makes possible the use of the method of calculation known as quantity calculus in which the units, as well as the measure, of a physical quantity emerge naturally in the calculated answer. This book uses the SI and quantity calculus throughout in the development of the numerical concepts like ionisation energy, lattice energy, bond energy term, paramagnetic moment, standard redox potential, which are of particular value in inorganic chemistry. It also deals with such unifying concepts as effective nuclear charge, screening constant, electronegativity and oxidation number which aid the teaching of the subject. The book combines the treatment of all of these concepts with numerical examples to which answers are given. A few of the non-SI units still in common use are included in the more descriptive parts of the book, but these are defined in terms of the international units.

HIGGINS, L. G. and RILEY, N. D., **A Field Guide to the Butterflies of Britain and Europe,** Collins, London, 1970, 380 pp., plates, £2.10.

The book covers every species and important subspecies or form—in Europe west of the Russian frontier, in western North Africa, in Madeira the Canaries and the Azores. For each are given its names: details of size, markings, characters important for identification; flight months, habitat, food-plants, altitude, range, distribution and rarity; other species with which it might be confused; and references to its illustration and its map. There is also an introduction to the whole subject; a glossary, check-list, selective bibliography and index.

HIGHNAM, K. C. and HILL, L., **The Comparative Endocrinology of the Invertebrates,** Edward Arnold (Publishers) Ltd., London, 1969, x+270 pp., figs., £4.40.

In this book the authors show that the principles of hormonal integration and coordination are as well exemplified in the invertebrate animals as in the vertebrate. These principles can be demonstrated practically, using species which are not precluded by the vivisection regulations, and are often more suitable than vertebrates for class use. A certain amount of practical detail has, therefore, been incorporated into the text to illustrate the relatively simple experiments and techniques involved.

CONTENTS. - *I: Nervous and Chemical Co-Ordination.* - *II: Invertebrate Endocrine Systems.* - *III: Endocrine Mechanisms in Coelenterata, Turbellaria, Nematoda and Echinodermata.* - *IV: Endocrine Mechanisms in the Annelida.* - *V: Endocrine Mechanisms in the Mollusca.* - *VI: Endocrine Mechanisms in the Insecta-I.* - *VII: Endocrine Mechanisms in the Insecta-II. Hormones and Reproduction.* - *VIII: Endocrine Mechanisms in the Insecta-III. Other Endocrine-Controlled Processes.* - *IX: Endocrine Mechanisms in Crustacea-I.* - *X: Endocrine Mechanisms in Crustacea-II. Endocrine Control of Reproduction.* - *XI: Endocrine Mechanisms in Crustacea-III.* - *XII: The Chemistry of Invertebrate Hormones.* - *XIII: Pheromones.* - *XIV: Hormones and the Environment.*

HILPINEN, R. (ed.), **Deontic Logic: Introductory and Systematic Readings** (Series: *Synthese Library*), D. Reidel Publishing Company, Dordrecht, 1971, VIII+182 pp., Dfl. 35.

The book contains seven essays dealing with the development and problems of deontic logic in the past forty years; the semantical foundations of normative discourse; applications of deontic logic to problems of ethics; conditional norms and the notion of commitment; and significant new contributions to this subject. Also, interesting analogies between deontic logic and the logic of necessary and sufficient conditions are discussed. Three papers were written specifically for this volume: the ones by Føllesdal and Hilpinen, Hintikka, and Segerberg. The rest of the papers, or earlier versions, were published before. A characteristic feature of this book is the prevalence of semantical methods.

HOESCHELE, D. F. Jr., **Techniques de conversion analogique-digitale et digitale-analogique,** Masson & C.ie, Paris, 1971, VIII+398 pp., figs. and tables, 130 F.

Cet ouvrage permet de se familiariser avec les techniques de conversion usitées actuellement. Il s'agit d'un livre qui met en relief les techniques qui peuvent être et ont été employées pour la réalisation de diverses fonctions. Quoique fortement orienté vers l'électronique, des codeurs de position angulaire et des commutateurs électromécaniques sont décrits en détails. Les nombreux schémas permettent de suivre parfaitement l'exposé. Chaque chapitre débute par les connaissances fondamentales qui seront nécessaires plus avant. Des avant-projets de sous-ensembles, aboutissant à celui d'un convertisseur analogique-digital au complet, sont aussi présentés et discutés.

CONTENTS. - *I: Convertisseurs.* - *II: Répartition et analyse d'erreurs.* - *III: Logique de conversion.* - *IV: Tensions analogiques de commutation.* - *V: Décodage digital analogique.* - *VI: Projet d'un décodeur digital analogique.* - *VII: Tensions de référence.* - *VIII: Comparateurs de tensions analogiques.* - *IX: Multiplexage analogique (commutation, souscommutation).* - *X: Codage digital de position angulaire.* - *XI: Types de convertisseurs analogiques digitaux.* - *XII: Projet d'un convertisseur analogique-digital.* - Appendices.

HOFFMAN, I., HAMBY, R. I. and GLASSMAN, E. (eds.), **Vectorcardiography 2** (Proceedings of the XIth International Symposium on Vectorcardiography, held in New York and sponsored by The Long Island Jewish Medical Center, from 15-17 May, 1970), North-Holland Publishing Company, Amsterdam, 1971, x+706 pp., figs. and tables, Dfl. 108.

Containing approximately 80 papers by leading research cardiologists covering all aspects of vectorcardiography research under current active investigation, this book is the companion volume to Vectorcardiography 1965, which became the standard source between 1965-69. It is believed that the present volume will be the standard reference work on the subject for some years.

CONTENTS. - *I: Lead Systems, Theoretical and Mathematical Considerations.* - *II: Surface Mapping.* - *III: Computer Applications.* - *IV: QRS Conduction Abnormalities (Hemiblocks, Bundle Branch Blocks, Pacemaker VCG and W-P-W).* - *V: Vectorcardiography in Coronary Artery Disease and Myocardial Infarction.* - *VI: Miscellaneous Clinical Applications.* - *VII: Pediatric Vectorcardiography.* - *VIII: T and P Loops.*

HÖHLER, G. (ed.), **Springer Tracts in Modern Physics** (Ergebnisse der exakten Naturwissenschaften), Vol. 58, Springer-Verlag, Berlin, 1971, IV+266 pp., figs. and tables, DM 88.

CONTENTS. - *I: Survey of Cosmology*, by W. KUNDT. - *II: Silicon Carbide as a Semiconductor*, by J. FEITKNECHT. - *III: High Energy Treatment of Atomic Collisions*, by K. DETTMANN. - *IV: Dynamisches Verhalten von Metallen unter Stosswellenbelastung*, by K. H. SCHRAMM.

HOOGERWERF, A., **Udjung Kulon** (The Land of the Last Javan Rhinoceros), E. J. Brill, Leiden, 1970, XIV+512 pp., plates, guilders 90.

In the present work, intended as a monograph on one of the most important wildlife reserves in Indonesia, the author presents his experiences over a twelve-year association with Udjung Kulon. In view of the fact that Udjung Kulon is the only place where one of the rarest animals in the world, *Rhinoceros sondaicus*, has survived, the chief focus of this book is on this species of rhinoceros, which was, as early as 1949, placed first on the list of Vanishing Mammals of the World compiled by the International Union for Conservation of Nature and Natural Resources. The other large mammals of Java, the Banteng, Javan tiger and Javan deer, for whom this reserve may be considered a last stronghold, are discussed with scarcely less thoroughness. The data presented for the Javan tiger are of especial significance in that the threat of extinction facing this animal is even more immediate than in the case of the Javan rhinoceros. The chapters dealing with these animals are amplified with many photographs. In discussing the ruminants, great attention has been devoted to the influence of predators and parasitic disease and to the food situation. Measures considered necessary to ensure a permanently sufficient food supply are also discussed.

HYVÄRINEN, L. P., **Information Theory for Systems Engineers** (Series: *Econometrics and Operations Research*, No. 17), Springer-Verlag, Berlin, 1970, VIII+198 pp., figs. and tables, DM 44.

The lectures attempt to give an exposition of the logical foundation and basic principles, and to provide at the same time a basis for further study in more specific areas of this expanding theory, such as coding, detection, pattern recognition, and filtering.

CONTENTS. - *I: Introduction.* - I. Discrete Information Systems. *II: Noiseless Channels.* - *III: Coding for Noiseless Channels.* - *IV: Joint Events, Natural Languages.* - *V: Applications of Noiseless Coding.* - *VI: Noisy Channel.* - *VII: Error Checking and Correcting Codes.* - II. Continuous Information Systems. *VIII: Properties of Continuous Channels.* - *IX: Detection of Continuous Signals.* - *X: Information Filters.*

Introduzione alla scienza fisica (Corso sperimentale di chimica e fisica per il biennio delle Scuole Medie Superiori), Zanichelli, Bologna, 1971, XII+220 pp., figs. and tables, L. 1.850.

L'opera è un corso sperimentale di chimica e fisica che tiene conto dei più recenti sviluppi della ricerca e degli orientamenti più aggiornati della didattica.

CONTENTS. - *I: Introduzione.* - *II: Quantità di materia: la massa.* - *III: Proprietà caratteristiche.* - *IV: Solubilità e solventi.* - *V: La separazione delle sostanze.* - *VI: Composti ed elementi.* - *VII: Radioattività.* - *VIII: Il modello atomico della materia.* - *IX: Le dimensioni e le masse degli atomi e delle molecole.* - *X: Il moto molecolare.* - *XI: Il calore.*

JÄRNÅKER, J., **The Double Solution of the Theory of Relativity** (A Study of Ontological Symmetry), Almqvist & Wiksell, Uppsala, 1970, 82 pp., figs., Sw.Kr. 25.

CONTENTS. - *I: Symmetry of the Lorentz Transformation and Sommerfeld's Fine Structure Constant.* - *II: The Symmetry of Discontinuity and Continuity.* - *III: Michelson's Experiment and the Concept of Ether.* - *IV: Four-Dimensional Space and Perspectival Transformation.* - *V: Experimental Proofs of the Two Significations of the Velocity of Light.* - Conclusion: *The Cosmological Postulate.*

JESSOP, N. M., **Biosphere** (A Study of Life), Prentice-Hall, London, 1970, XIV+954 pp., figs. and plates, £5.75.

CONTENTS. - I. The Diversity of Life. *I: Analyzing the Living World. - II: A Survey of the Living World.* - II. Causes of Biotic Diversity. *III: The Integration of Organism and Environment. - IV: Adaptation to Abiotic Factors. - V: Adaptation to Habitat. - VI: Adaptation to Other Species. - VII: Adaptation Within the Species. - VIII: The Dynamics of Adaptation.* - III. The Organization of Life. *IX: The Molecular Basis of Life. - X: The Organization of Cells. - XI: Development. - XII: Body Plan.* - IV. Maintenance of the Individual. *XIII: Nutrition: Obtaining Mass and Energy. - XIV: Nutrition: Transport and Metabolism. - XV: Integrative Mechanisms. - XVI: Adaptive Response.* - V. The Continuance of Life. *XVII: Perpetuation of Kind.*

KÄMPFE, L., KITTEL, R. and KLAPPERSTÜCK, J., **Leitfaden der Anatomie der Wirbeltiere,** Veb Gustav Fischer Verlag, Jena, 1970, 336 pp., figs. and tables, DM 22.80.

Der Leitfaden hat sich viele Freunde erworben, so dass wiederum nach kurzer Zeit eine Neuauflage erforderlich wurde. Ganz offensichtlich hat sich die Gliederung nach Organsystemen und deren vergleichende Betrachtung innerhalb der Wirbeltierreihe bewährt. Dieses Prinzip wurde beibehalten unter Einarbeitung neuer Forschungsergebnisse und aussagekräftiger Abbildungen. Das Buch erfuhr eine bedeutsame Bereicherung durch Anfügen eines Kapitels "Zur Stammesgeschichte der Wirbeltiere", in dem die Hauptlinien der mutmasslichen Evolution der Wirbeltiere kurz dargestellt werden.

KATZ, B., **Nervi, muscoli e sinapsi,** Zanichelli, Bologna, 1971, XII+180 pp., figs. and tables, L. 3.600.

Questo libro è un testo moderno di neurofisiologia, destinato in primo luogo a studenti dei primi anni di università. Ma l'interesse intrinseco dell'argomento, la sua attualità di ricerca di frontiera, raccomandano il libro anche come strumento di lavoro per studenti di anni successivi, e anche per ricercatori, biofisici, elettrofisiologi, neurologi. Argomento del libro è la trasmissione dei segnali lungo i nervi, da cellula a cellula attraverso quei singolari tipi di contatti che sono le sinapsi, e infine, attraverso le giunzioni neuromuscolari, ai muscoli di cui dovranno provocare la contrazione. Regione periferica di quel vasto continente inesplorato che è il sistema nervoso, questo argomento comincia adesso a essere abbastanza conosciuto, almeno nelle grandi linee, e anche a livello sperimentale e quantitativo: di questo l'Autore non trascura di darci una sensazione diretta, attraverso numerosi esempi numerici.

CONTENTS. - *I: Elementi dell'organizzazione neuromuscolare. - II: Elettricità e neurofisiologia. - III: Alcune osservazioni sulle singole fibre nervose e muscolari. - IV: Il concetto di membrana. - V: L'avvio dell'impulso. - VI: La propagazione dell'impulso. - VII: Trasmissione degli impulsi all'interno delle cellule e attraverso le superfici-limite delle cellule. - VIII: La trasmissione degli impulsi dal nervo al muscolo. - IX: Natura quantica della trasmissione chimica. - X: Trasmissione dei segnali attraverso le sinapsi neuronali. - XI: Avvio della contrazione muscolare. - XII: Conclusione.*

KINGDON, J., **East African Mammals** (An Atlas of Evolution in Africa), VOLUME I, Academic Press, London, 1971, X+446 pp., figs., £12.00.

This is the first of a three volume work. It has an introduction to the East African environment but deals mainly with the primates. Mammalian evolution is the central theme; but the work aims to be a comprehensive inventory, textbook and atlas as well. This volume relates existing knowledge of geography, vegetation and anatomy to mammology and to the evolution of the species. The animals have economic, medical and veterinary importance to the countries in which they live and data on this is also included. Since East Africa is not a natural geographic region, the fauna are in many ways representative of the whole continent. The author's extensive and outstanding illustrations, nearly 100 maps and over 200 drawings (including detailed plates, dissections, skeletons and sketches), provide a rich source of visual material. *East African Mammals* presents a braod background for the student of mammology and much new information about species. This contribution to the understanding of evolution on the African continent will be of interest to zoologists, naturalists, veterinary and museum workers, primatologists and paleontologists.

KOJIMA, K. (ed.), **Mathematical Topics in Population Genetics** (Series: *Biomathematics*, No. 1), Springer-Verlag, Berlin, 1970, x+400 pp., figs. and tables, DM 68.

CONTENTS. - *I: Random Drift and the Shifting Balance Theory of Evolution*, by S. WRIGHT. - *II: Changes in Mean Fitness under Natural Selection*, by J. R. G. TURNER. - *III: Models and Analyses of Dispersal Patterns*, by R. H. RICHARDSON. - *IV: Avoidance and Rate of Inbreeding*, by C. C. COCKERHAM. - *V: Genetic Loads and the Cost of Natural Selection*, by J. F. CROW. - *VI: Stochastic Processes in Population Genetics, with Special Reference to Distribution of Gene Frequencies and Probability of Gene Fixation*, by M. KIMURA. - *VII: Theory of Limits to Selection with Line Crossing*, by W. G. HILL. - *VIII: A Theory of Limits in Artificial Selection with Many Linked Loci*, by A. ROBERTSON. - *IX: The Evolution of Dominance*, by J. A. SVED and O. MAYO. - *X: Survival of Mutant Genes as a Branching Process*, by H. E. SCHAFFER. - *XI: The Incomplete Binomial Distribution*, by C. C. LI. - *XII: Evolutionary Significance of Linkage and Epistasis*, by K. KOJIMA and R. C. LEWONTIN. - *XIII: Fitness and Optimization*, by R. LEVINS.

KRAUTH, L., **Die Philosophie Carnaps** (Series: *Library of Exact Philosophy*, No. 1), Springer-Verlag, Wien, 1970, x+234 pp., DM 46.

Rudolf Carnap gilt unbestritten als einer der eminentesten Vertreter des "logischen Empirismus". Hier werden nun zum ersten Mal alle wesentlichen Aspekte seiner Philosophie zusammenhängend dargestellt und kritisch analysiert, nach Möglichkeit vor einem breiteren philosophiegeschichtlichen Hintergrund: die Konstruktion semantischer Systeme, das Ringen um ein Sinnkriterium, die Auseinandersetzung mit der "Metaphysik", der Aufbau einer "induktiven" Logik usw. Manche der dabei zutage tretenden Probleme erweisen sich überraschenderweise als mögliche Berührungspunkte mit der Fragestellung der traditionellen Philosophie.

CONTENTS. - Einführung. - *I: Die Konstruktion exakter Sprachen anhand semantischer Systeme. - II: Das Problem des empiristischen Sinnkriteriums. - III: Die theoretischen Begriffe der Wissenschaft. - IV: Die Sinnlosigkeit der Metaphysik. - V: Carnaps "Induktive Logik" als eine Wahrscheinlichkeitslogik. - VI: Die grundsätzliche Auffassung der Philosophie bei Carnap. - VII: "Externe" und "interne" Fragen: eine problematische Lösung.*

LACADENA, J.-R., **Genética vegetal** (Fundamentos de su aplicación), 2nd ed., Agesa, Madrid, 1970, 430 pp., figs., 700 pesetas.

CONTENTS. - *I: Mejora genética de las plantas cultivadas: Generalidades. El material vegetal. - II: Mejora de plantas autógamas. Fundamentos genéticos y métodos. - III: Mejora de plantas alógamas: Fundamentos genéticos y métodos. - IV: Incompatibilidad. - V: Mejora de plantas de multiplicación asexual y plantas apomícticas: Fundamentos genéticos y métodos. - VI: Genética de la resistencia a enfermedades, plagas y condiciones ambientales adversas en las plantas cultivadas. - VII: Genética y mejora de la calidad de las plantas cultivadas. - VIII: Androesterilidad. - IX: Mutagénesis artificial. - X: Mejora conservadora. - XI: Aneuploidia. - XII: Poliploidía. - XIII: Introducción de la variación genética extraespecífica en las plantas (1). - XIV: Introducción de la variación genética extraespecífica en las plantas (2). - XV: Haploidía.*

LANG, S., **Introduction to Linear Algebra,** Addison-Wesley Publishing Company, Inc., London, 1970, VIII+180 pp., figs., £1.75.

This text is intended to serve as either a supplement to the calculus sequence or as an independent short course in the elements of linear algebra. The book will probably find its greatest use at the end of the freshman year, after the student has completed one-variable calculus, but it could be used before if it is desirable to introduce students to some algebra previous to the calculus. In that case, some of the exercises and examples taken from the calculus can be handled according to the judgment of the instructor. The author concentrates on the natural bases and has included excellent applications to geometry. The chapter on determinants is intended to be independent of the rest of the text. Each chapter includes a large selection of exercises.

CONTENTS. - *I: Vectors. - II: Vector Spaces. - III: Linear Equations and Bases. - IV: Linear Mappings. - V: Linear Maps and Matrices. - VI: Scalar Products and Orthogonality. - VII: Determinants.*

LARRAURI, I., **Dizionario di otorinolaringologia** (in cinque lingue), (Series: *Dictionnaires des Sciences Médicales*), Librairie Maloine S.A., Paris, 1971, 172 pp., 40 F.

L'autore ha compilato un dizionario dei termini di otorinolaringoiatria in cinque lingue (italiano, francese, inglese, tedesco e spagnolo) destinato così agli specialisti e agli studenti come ai traduttori di testi di medicina. L'opera, in cinque volumi, si propone di consentire una consultazione il più possibile agevole e rapida.

LARSON, D. B., **Quasars and Pulsars,** North Pacific Publishers, Portland, 1971, XII+180 pp., figs. and tables, $6.50.

In this book the author applies his new general physical theory to the problem of the quasars and associated phenomena that has baffled conventional theories, and comes up with the answers for which science has been searching: what these objects are, where they are, how they originated, how long a life span they have, what ultimately becomes of them, how their energy is generated, how the quasars are related to the pulsars, and so on.

CONTENTS. - *I: Introduction.* - *II: Design of the Universe.* - *III: Simple Linear Motion.* - *IV: Rotational Motion (Matter).* - *V: Destructive Limits.* - *VI: The Astronomical Scene.* - *VII: The 1959 Predictions.* - *VIII: Quasars: The General Picture.* - *IX: Quantitative Verification.* - *X: Some Further Details.* - *XI: The Pre-Quasars Situation.* - *XII: Supernovae and Pulsars.*

LEDBETTER, M. C. and PORTER, K. R., **Introduction to the Fine Structure of Plant Cells,** Springer-Verlag, Berlin, 1970, x+188 pp., figs. and tables, DM 54.

This atlas presents in over fifty full-page illustrations a survey of plant cells and their components as studied with the electron microscope. Each illustration is accompanied with an interpretive text to introduce the reader to features of cell fine structure. Further study is aided by a list of selected references to pertinent literature appended to each description. The areas covered are: General cell structure, fine structure of interphase cell and cell organelles, fine structure of dividing cells, the cell wall and plasmodesmata, vascular tissues, schlerenchyma and collenchyma, epidermal cells and varients, photosynthetic apparatus, cells with special inclusions, and germinative tissues. This book is designed to serve as a supplementary text for students of cell biology, or to introduce the research scientist or teacher of allied fields to this rapidly expanding field of botanical study.

LEVY, P., **Quelques aspects de la pensée d'un mathématicien,** Librairie Scientifique et Technique A. Blanchard, Paris, 1970, 222 pp., 34 F.

CONTENTS. - I. Autobiographie mathématique. *I: Mes études. Premiers travaux.* - *II: Analyse fonctionnelle et équations intégrales.* - *III: Calcul des probabilités. Notions générales. L'addition des variables aléatoires.* - *IV: Processus stochastiques. Mouvement brownien et processus de Markov.* - *V: Questions diverses.* - II. L'évolution de mes idées sur la philosophie. *I: Introduction.* - *II: Mon enfance.* - *III: Rencontre avec Descartes.* - *IV: L'évolution de mes idées après la lecture du Discours de la méthode.* - *V: Dieu existe-t-il?* - *VI: Rencontre avec Bergson.* - *VII: Les fondements du Calcul des probabilités.* - *VIII: Remarques diverses sur les fondements des mathématiques.*

LIMOGES, C., **La sélection naturelle** (Etude sur la première constitution d'un concept, 1837-1859), (Series: « *Galien* »), Presses Universitaires de France, Paris, 1970, 184 pp., 25 F.

CONTENTS. - *I: Introduction.* - *II: De la distribution géographique des organismes à la sélection naturelle.* - *III: Vraie et fausses identités.* - *IV: Le concept de sélection naturelle et l'origine des espèces.* - Conclusion - Appendices - Bibliographie.

LIPPARD, S. J. (ed.), **Progress in Inorganic Chemistry,** VOLUME 14, John Wiley & Sons Ltd., London, 1971, VI+442 pp., figs. and tables, £11.75.

CONTENTS. - *I: Studies of Dynamic Organometallic Compounds of the Transition Metals by Means of Nuclear Magnetic Resonance*, by K. VRIEZE and P. W. N. M. VAN LEEUWEN. - *II: Inorganic Electrosynthesis in Nonaqueous Solvents*, by B. L. LAUBE and C. D. SCHMULBACH. - *III: Electronic Spectroscopy of High-Temperature Open-Shell Polyatomic Molecules*, by D. M. GRUEN. - *IV: Single Crystal and Gas Phase Raman Spectroscopy in Inorganic Chemistry*, by G. A. OZIN. - *V: The Stereochemistry of Bis-Chelate Metal (II) Complexes*, by R. H. HOLM and M. J. O'CONNOR. - Author Index. - Subject Index. - Cumulative Index, Volumes 1-14.

LONGMORE, D., **Chirurgie et organes de rechange** (La pratique chirurgicale de l'avenir), (Series: *Techniques d'aujourd'hui*), Librairie Larousse, Paris, 1970, 192 pp., figs., 14,50 F.

CONTENTS. - *I: Le mécanisme de la vie. - II: Appareils extra-corporels. - III: Les membres artificiels. - IV: Implants mécaniques. - V: Homogreffes simples. - VI: Homogreffes complexes. - VII: Le défi de l'Homme.*

LUYTEN, W. J. (ed.), **White Dwarfs** (I.A.U. Symposium No. 42, held at St. Andrews, Fife, Scotland, 11-13 August 1970), D. Reidel Publishing Company, Dordrecht, 1971, x+164 pp., figs. and tables, Dfl. 34.

The Proceedings of the First International Conference on White Dwarfs, held from 11-13 August at the University of St. Andrews, brought together, for the first time in this field, twenty-seven astronomers, observational as well as theoretical in roughly equal numbers. The twenty-five papers presented reflect the same approximate participation. Since the book deals with the first and only conference ever held on the subject, it gives not only a completely up-to-date picture of the observational and theoretical aspects of White Dwarfs, but also describes to entire history of the field from 1918, its beginning, onward.

MEYLAN, S., **Bioélectricité** (Quelques problèmes), (Series: *Monographies de Physiologie Végétale*, No. 9), Masson & C.ie, Paris, 1971, 112 pp., figs. and tables, 60 F.

Si les traités de biologie ou de phytophysiologie mentionnent les réactions électriques qui manifestent l'irritabilité d'une cellule isolée ou d'une sensitive, ils ne font guère de place aux phénomènes de polarisation qui en sont l'origine. C'est précisément cet aspect moins connu de la bioélectricité végétale qui fait l'objet de la présente mise au point. Deux questions y sont plus particulièrement examinées: celle de la polarisation cellulaire, dont l'étude vient d'enregistrer des progrès décisifs; - celle des relations entre la polarité bioélectrique, la croissance et l'auxine, depuis longtemps discutée et pour laquelle est proposée une solution nouvelle. L'étude des mécanismes de polarisation des tissus végétaux fait le passage entre les phénomènes bioélectriques cellulaires et la polarité de divers organes; l'analyse des manifestations bioélectriques de la racine en établit la corrélation avec l'activité physiologique.

MIHALAS, D. (P. ROUTLY), **Galactic Astronomy,** W. H. Freeman and Company, Berkshire, 1968, XIV+258 pp., figs. and tables, £4.70.

The material presented in this text represents the content of a course on Galactic astronomy which the Author gave to undergraduate astronomy and physics students at Princeton University. This book is an attempt to gather information from scattered sources into a single book, and to treat both structure and dynamics on an equal basis.

CONTENTS. - *I: The Galaxy: An Overview. - II: Astronomical Terminology. - III: Physical Properties of the Stars. - IV: The Distribution of Stars in Space. - V: The Solar Motion. - VI: Statistical Parallaxes. - VII: The Velocity Ellipsoid and High-Velocity Stars. - VIII: Galactic Rotation and the Spiral Structure of Our Galaxy. - IX: Review of Mechanics. - X: The Goals of Stellar Dynamics. - XI: The Theory of Stellar Encounters. - XII: Galactic Dynamics. - XIII: Characteristics of Individual Orbits of Stars. - XIV: Star Clusters.*

Nouvelles données scientifiques sur le massif du Tsaratanana (Madagascar), (Mémoires O.R.S.T.O.M. No. 37), O.R.S.T.O.M., Paris, 1970, viii+244 pp., figs. and plates, No price stated.

Contents. - *I: Aperçu général sur le massif.* - *II: Buts de la mission Armée française/O.R.S.T.O.M. de novembre 1966.* - *III:* M. Descamps and D. Wintrebert, *Contribution à l'étude des Acrididae du massif du Tsaratanana* (Orth. Acridoidea). - *IV:* M. Ferragu, *Dinosius (Coleoptera Curculionidae) inédits récoltés dans le massif du Tsaratanana* (Madagascar Nord). - *V:* C. Girard, *Note sur quelques Coléoptères Elatérides de Madagascar.* - *VI:* J. Jarrige, *Contribution à l'étude des Coleoptera Brachelytra du massif du Tsaratanana* (Madagascar Nord). - *VII:* P. Basilewsky, *Contributions à l'étude des Coléoptères Carabiques de la région malgache, VI. Les Anchomeninae du massif du Tsaratanana* (Madagascar Nord). - *VIII:* A. Diakonoff, *Lepidoptera Tortricoidea from Tsaratanana Range* (North Madagascar). - *IX:* P. Viette, *Deux nouvelles Pyrales inédites récoltées dans le massif du Tsaratanana* (Madagascar Nord). - *X:* C. Herbulot, *Lepidoptera Geometridae du Tsaratanana* (Madagascar Nord). - *XI:* Y. De Lajonquière, *Lépidoptères Lasiocampidae récoltés dans le massif du Tsaratanana* (Madagascar Nord). - *XII:* P. Griveaud, *L'épidoptères Lymantriidae récoltés dans le massif du Tsaratanana* (Madagascar Nord). - *XIII:* P. Viette, *Noctuelles Trifides du massif du Tsaratanana* (Lep. Noctuidae). - *XIV:* P. Griveaud, *Lépidoptères Amatidae récoltés dans le massif du Tsaratanana* (Madagascar Nord). - *XV:* G. Bernardi, *Les Belenois antsianaka (Ward) de la région occidentale du Tsaratanana* (Lep. Pieridae). - *XVI:* J.-M. Klein, *Nouvelles Puces de Madagascar, récoltées dans le massif du Tsaratanana* (Siphonaptera). - *XVII:* R. Albignac, *Mammifères et Oiseaux du massif du Tsaratanana* (Madagascar Nord). - *XVIII:* M. M. Debray, *Contribution à l'inventaire des plantes médicinales du massif du Tsaratanana.* - *XIX:* R. Capuron, *Une nouvelle espèce d'Evodia récoltée dans le massif du Tsaratanana* (Rutacées).

PALOMBA, G., **Morfologia economica** (Series: *Sociologi ed Economisti*, No. 46), Unione Tipografico-Editrice Torinese, Torino, 1970, lii+582 pp., L. 7.000.

Contents. - I. La società arcaica. *I: La metafisica.* - *II: L'estetica.* - *III: La logica.* - *IV: L'economia.* - II. La civiltà cristiana. *V: La « Buona Novella ».* - *VI: Cristianesimo ed islamismo.* - *VII: Cattolicesimo, ortodossia e protestantesimo.* - *VIII: La città medievale.* - III. Dialettica della svalutazione. *IX: La « grande crisi ».* - *X: Verso l'economia borghese.* - *XI: L'interpretazione dei classici.* - *XII: La « piccola crisi ».* - IV. Speranze e timori del secolo XX. *XIII: Il materialismo dialettico.* - *XIV: Lo sviluppo economico.* - Appendice - Indice dei nomi storici, degli autori e dei testi anonimi - Indice per argomenti.

PARSONS, R. T. (ed.), **Windows on Africa** (A Symposium), E. J. Brill, Leiden, 1971, x+202 pp., tables, guilders 38.

Windows on Africa bring into a sharp focus a number of the situations and problems of African people in several parts of the subsaharan area of the continent. The material is presented by Africa and Western writers each of whom has had many years of working experience in Africa. The authors represent seven African countries and nine different Protestant denominations. The concerns dealt with in the book are present throughout Africa. The issues with which the writers deal are those with which the churches, other voluntary agencies and the Governments of Africa will have to struggle for many years to come.

PECKER, J.-C., **Space Observatories** (Series: *Astrophysics and Space Science Library*, No. 21), D. Reidel Publishing Company, Dordrecht, 1970, xii+120 pp., figs. and tables, Dfl. 35.

Contents. - I. The Raisons d'Être of Space Astronomy. *I: The Structure of the Atmospheric Layers.* - *II: The Opaque Wall of the Atmosphere.* - *III: Atmospheric Diffusion: Extinction and the Blue of the Sky.* - *IV: The Inhomogeneity of the Earth's Atmosphere: Clouds and Refraction.* - *V: The Particle Barrier.* - *VI: The Fusion of Meteorites.* - II. A Provisional and Partial Inventory of Some of the Information to be Acquired by Space Research. *I: Beyond Diffusion.* - *II: Beyond Turbulence.* - *III: Beyond Ultraviolet Opacity.* - *IV: Astronomy at Short Wavelengths.* - *V: Beyond Infrared Opacity: Towards the Infrared by Balloon.* - *VI: Beyond the Ionosphere: Towards Very Long Wavelengths by Eccentric Satellite.* - *VII: A Prospective Conclusion of Limited Perspective.*

PICARD, H., **Utilisation thérapeutique des oligo-éléments,** 2nd ed., Librairie Maloine S.A., Paris, 1971, 180 pp., 18 F.

Alors que nous ne connaissions parmi les oligo-éléments que les effets de l'Iode, l'auteur analyse minutieusement ceux de nombreux autres éléments dont l'intérêt est aussi appréciable. Il décrit en outre l'effet particulier de quelques associations synergiques. Cet ouvrage est présenté comme un précis d'utilisation destiné à permettre à tout thérapeute d'appliquer ces remèdes. Il se termine sur des considérations nouvelles sur la thérapeutique en général et sur celle des terrains pathologiques où nous étions souvent désarmés.

PIERRE-NOEL, L., **Manuel de santé publique,** Librairie Maloine S.A., Paris, 1970, 208 pp., 40 F.

Ce livre s'adresse à tous ceux — médecins, professions para-médicales, étudiants, organismes médico-sociaux nationaux et internationaux — qui, dans les pays en voie de développement, luttent pour promouvoir la santé. Il tend à combler une importante lacune, celle de l'enseignement de la Santé Publique, dans sa conception moderne et, surtout, adapté aux possibilités et aux besoins de ces pays.

PISTOR, M., **Les micro-injections de la Mésothérapie,** Librairie Maloine S.A., Paris, 1971, 256 pp., figs., 38 F.

CONTENTS. - *I: Définition.* - *II: Introduction.* - *III: Le cordonnier de Bray-Lu.* - *IV: Historique.* - *V: Le matériel.* - *VI: Technique d'utilisation de la Mésothérapie.* - *VII: La douleur.* - *VIII: La Mésothérapie en rhumatologie.* - *IX: La Mésothérapie en dermatologie.* - *X: La Mésothérapie dans les maladies de la nutrition.* - *XI: La Mésothérapie en pédiatrie: micro-vaccinations.* - *XII: La Mésothérapie en oto-rhino-laryngologie.* - *XIII: Mésothérapie et allergie.* - *XIV: La Mésothérapie en stomatologie.* - *XV: La Mésothérapie en gastro-entérologie.* - *XVI: La Mésothérapie dans les maladies du coeur et des vaisseaux sanguins.* - *XVII: La Mésothérapie en neurologie.* - *XVIII: La Mésothérapie en chirurgie.* - *XIX: La Mésothérapie en gynécologie.* - *XX: La Mésothérapie dans les maladies pulmonaires.* - *XXI: La Mésothérapie en microbiologie et virologie.* - *XXII: La Mésothérapie en ophtalmologie.* - *XXIII: La Mésothérapie en urologie et sexologie.* - *XXIV: Mésothérapie: procaine et vieillissement.* - *XXV: La Mésothérapie en médecine vétérinaire.* - *XXVI: Société Française de Mésothérapie.* - *XXVII: La Mésothérapie contre la pollution interne (abus médicamentaux).* - *XXVIII: Les inconvénients de la Mésothérapie.* - *XXIX: Conclusion.*

POINTEAU, R. and BOVASTRE, J., **Eléments de polarographie** (Théorie, technique instrumentale, applications analytiques), (Series: *Chimie Physique*), Masson & C.ie, Paris, 1970, VIII+396 pp., figs. and tables, 120 F.

CONTENTS. - *I: Les fondements de la méthode classique.* - *II: Technique instrumentale de la méthode classique.* - *III: Les maximums.* - *IV: Méthodes oscillographiques et impulsionnelles.* - *V: Eléments de la théorie des courbes $I = f(E)$ à potentiel constant.* - *VI: Éléments de la théorie des courbes $I = f(E)$ à potentiel variable.* - *VII: Phénomènes d'adsorption.* - *VIII: Résultats physico-chimiques en chimie minérale.* - *IX: Application de la polarographie à l'analyse minérale.* - *X: Application de la polarographie aux molécules organiques.* - *XI: Applications à la biochimie.* - *XII: Titrages ampérométriques.*

QUINTIN, M., **Electrochimie** (Series: *Euclide*), Presses Universitaires de France, Paris, 1970, 544 pp., figs. and tables, No price stated.

CONTENTS. - *I: Introduction sur la thermodynamique des solutions électrolytiques.* - *II: Théorie des actions interioniques. Solutions non perturbées.* - *III: Thermodynamique de la pile galvanique. Force électromotrice des piles. Potentiel d'électrode.* - *IV: Mesure des forces électromotrices et des potentiels.* - *V: Détermination de potentiels standards.* - *VI: Mesure des coefficients d'activité moyens des ions d'un électrolyte. Résultats expérimentaux. Confrontation avec la théorie.* - *VII: pH. Signification. Détermination électrométrique.* - *VIII: Titrages potentiométriques.* - *IX: Constante de dissociation. pK vrai, pK apparent. Détermination à partir de mesures de force électromotrice.* - *X: Conductance. Définitions.*

Théorie. - XI: Mesure de la conductance des électrolytes. - XII: Résultats expérimentaux des mesures de conductance des électrolytes. Confrontation avec la théorie. - XIII: Nombre de transport. - XIV: Conductibilité ionique. Mobilité des ions. - XV: Applications des mesures de conductibilité.

RAGUIN, E., **Pétrographie des roches plutoniques dans leur cadre géologique,** Masson & C.ie, Paris, 1970, 240 pp., figs. and tables, 54 F.

L'ouvrage comprend 8 chapitres de description des roches. Chaque chapitre, sauf celui des charnokites plus complexe, donne successivement une définition minéralogique des roches, une description de leurs structures, et la « géologie » de ces roches. Cette dernière rubrique, qui est la plus importante conformément à l'idée directrice ci-dessus, décrit le gisement des roches sur le terrain. Ce livre s'adresse aux étudiants des sciences géologiques, mais aussi aux spécialistes et au public éclairé. On s'est efforcé de le rendre accessible aux non-spécialistes, en définissant toujours les termes techniques de façon simple.

RAŽNJEVIĆ, K., **Tables et diagrammes termodynamiques,** Editions Eyrolles, Paris, 1970, 342 pp., tables, 110 F.

Ces tables et diagrammes sont destinés à tous ceux qui s'intéressent à la thermodynamique et à la technique de la chaleur. Dans ces 342 pages de tables et dans les 16 diagrammes qui les accompagnent, chacun trouvera toutes les données indispensables à l'exécution de calculs théoriques, aussi bien qu'à la solution de problèmes d'application pratique. Le classement des données n'a pas été effectué suivant les diverses catégories de grandeurs thermiques, mais selon l'état physique des corps étudiés: solides, liquides, vapeurs et gaz. Dans les tables, toutes les déterminations effectuées avec l'ancien système d'unités de mesures ont été converties en valeurs du nouveau système international d'unités MKSA. Cette transcription des données dans les deux systèmes — l'ancien et le nouveau — qui figurent côte à côte, présente un avantage évident pour les utilisateurs des tables. Les valeurs numériques ont été puisées aux sources les plus récentes; elles s'étendent jusqu'à la température de 1.000°C (1.273°K) et la pression de 1.000 atmosphères. A la fin de l'ouvrage, figurent des tableaux pour la conversion, d'un système d'unités à l'autre, d'un ensemble de grandeurs intervenant fréquemment en thermodynamique. En annexe, on trouve 16 diagrammes sous la forme de grands dépliants.

REICHLE, D. E. (ed.), **Analysis of Temperate Forest Ecosystems** (Series: *Ecological Studies*, Vol. 1), Springer-Verlag, Berlin, 1970, XII+304 pp., figs. and tables, DM 52.

CONTENTS. - I. Analysis of an Ecosystem. *I: Combination of Qualitative and Quantitative Approaches*, by F. BOURLIÈRE and M. HADLEY. - *II: Analysis of Ecosystems*, by F. E. SMITH. - *III: Studying the Effects of Environmental Factors on Ecosystems*, by D. W. GOODALL. - *IV: Phenology in Productivity Studies*, by H. LIETH. - II. Primary Producers. *V: Biomass and Productivity Models of Forest Canopies*, by H. A. I. MADGWICK. - *VI: A Synthesis of Studies by the Harvest Method: Primary Production Relations in the Temperate Deciduous Forests of Japan*, by T. SATOO. - *VII: Metabolism of Terrestrial Ecosystems by Gas Exchange Techniques: The Brookhaven Approach*, by G. M. WOODWELL and D. B. BOTKIN. - III. Consumer Organisms. *VIII: Insect Influences on the Forest Canopy*, by R. T. FRANKLIN. - *IX: Estimation of the Effects of Phytophagous Insects on Forest Production*, by P. M. RAFES. - *X: Secondary Production of Birds and Mammals*, by D. R. MCCULLOUGH. - IV. Decomposer Populations. *XI: Soil Flora: Studies of the Number and Activity of Microorganisms in Woodland Soils*, by L. STEUBING. - *XII: The Role of Soil Invertebrates in Turnover of Organic Matter and Nutrients*, by C. A. EDWARDS, D. E. REICHLE, and D. A. CROSSLEY JR. - V. Nutrient Cycling. *XIII: Micronutrients: Forest Ecology and Systems Analysis*, by J. A. C. FORTESCUE and G. G. MARTEN. - *XIV: Biological Cycling of Minerals in Temperate Deciduous Forests*, by P. DUVIGNEAUD and S. DENAEYER-DE SMET. - *XV: Carbon Cycle and Temperate Woodlands*, by J. S. OLSON. - VI. Hydrologic Cycles. *XVI: The Water Flux in Temperate Forests: Precipitation and Evapotranspiration*, by G. STANHILL. - *XVII: Measurement and Sampling of Outputs from Watersheds*, by D. J. NELSON. - *XVIII: Models of the Hydrologic Cycle*, by J. W. CURLIN.

REYMENT, R. A., **Introduction to Quantitative Paleoecology,** Elsevier Publishing Company, Amsterdam, 1971, XIV+226 pp., figs. and tables, Dfl. 60,00.

In this work the author deals with various examples of simple mathematical models applied to some problems of paleoecology. The book treats case histories, many analyzed from the literature, many more obtained from the author's original research. An important tool in quantitative paleoecology is the experimental approach, and the work includes many laboratory experiments devised on living organisms, models and the like to test hypotheses. An entire chapter is devoted to the analysis of orientational data, while others deal with the identification of environmental factors, the identification of population-dynamic effects in fossil associations, and spatial paleoecology, which gives an analysis of the ecologic influence of climatic and microclimatic origin.

ROCARD, J.-M., **Mécanique** (Enseignement supérieur, Premier cycle), Masson et C.ie, Paris, 1971, VI+268 pp., figs. and tables, 28 F.

Cet ouvrage est consacré aux principes et théorèmes généraux de la mécanique des particules. Il a été conçu pour servir d'instrument de travail à plusieurs catégories d'étudiants. Les futurs physiciens, au stade du premier cycle de l'enseignement supérieur, les futurs ingénieurs, au stade des premières années d'école ou des classes préparatoires, y trouveront un exposé des bases de la mécanique dite de Newton. D'autre part, on a mis l'accent sur les relations entre les diverses disciplines: mathématiques, électricité, thermodynamique... Dans cet esprit, on a introduit la notion d'opérateur (gradient, divergence, etc.) très utile non seulement pour les physiciens mais aussi pour les mathématiciens, ainsi que celle de « champs » afin de représenter l'action mutuelle des particules. De nombreuses « applications » sont traitées dans cet ouvrage pour initier les étudiants à la description des phénomènes aussi bien microscopiques que macroscopiques.

ROTBERG, R. I. (ed.), **Rebellion in Black Africa,** Oxford University Press, Oxford, 1971, XXIV+262 pp., maps, £1.40.

The half-dozen essays in this volume, a selection from the larger and longer work, *Protest and Power in Black Africa,* survey representative African protest movements during and after the era of colonialism. Covering the Sudan, Natal, Uganda, Malawi, Kenya, and the Congo, they range in time from 1880 to the present, and show the diversity of methods Africans have adopted to cope with or protest against the complex and intrusive strands of social change. While they are focused upon overt rebellions against European or post-European domination, they also explore related topics such as religious expression of discontent, and the emergence of pressure groups and political parties.

CONTENTS. - Introduction, by R. I. ROTBERG. - *I: The Sudanese Mahdiya,* by L. C. BROWN. - *II: The Zulu Disturbances in Natal,* by S. MARKS. - *III: The Nyabingi Cult of Southwestern Uganda,* by E. HOPKINS. - *IV: Psychological Stress and the Question of Identity: Chilembwe's Revolt Reconsidered,* by R. I. ROTBERG. - *V: The Gusii Rebels,* by A. WIPPER. - *VI: Rebellion and the Congo,* by M. C. YOUNG.

ROY, R. S. and LALIBERTE-ROBERT, L., **Travaux pratiques de microbiologie,** Librairie Maloine S.A., Paris, 1971, 220 pp., figs. and tables, 34 F.

Ce volume s'adresse à l'étudiant de niveau collégial, pré-universitaire ou universitaire dans le domaine des sciences biologiques, médicales et paramédicales. Il intéressera au premier chef l'étudiante infirmière. Les expériences décrites dans ce guide visent à donner à ceux qui l'utilisent des connaissances générales en microbiologie et à leur faire entrevoir les applications médicales de ces connaissances. Il leur permettra en premier lieu de s'initier aux manipulations stériles. Les expériences décrites sont brèves et regroupées par section bien qu'elles demeurent indépendantes les unes des autres. Chaque exercice est précédé de notes explicatives rappelant les points théoriques importants; ceci permettra à l'étudiant de situer l'expérience dans son contexte théorique et d'en interpréter les résultats.

SCHWEITZER, J., **Ordinateurs et comportement conscients?** (Series: *Initiation aux Nouveautés de la Science*), Dunod, Paris, 1970, XII+204 pp., figs., 29 F.

Jusqu'où iront l'informatique et l'automatique? En particulier, l'étude de « L'intelligence artificielle » et surtout la réalisation de « machines conscientes » nous aideront-elles à mieux cerner le problème de la conscience, cette connaissance au second degré qui permet de mieux « savoir ce que l'on sait »? Le présent ouvrage, tente de répondre à ces questions. L'auteur, à la fois ingénieur et philosophe, a voulu jeter les bases d'une théorie des machines parallèles essentiellement autonomes et conscientes et envisager les propriétés qu'auraient nécessairement ces mécanismes. L'analyse de leur comportement à partir des méthodes quantifiées pourrait alors apporter de très utiles enseignements sur le phénomène de la conscience.

CONTENTS. - *I: Présentation.* - I. Exposé théorique. *II: Qu'est-ce que savoir?* - *III: La conscience de soi.* - *IV: Les informations ambiguës.* - II. Dispositions envisageables. *V: Machines immobiles. Dispositions envisageables.* - *VI: Machines actives. Dispositions envisageables.* - *VII: Où en sommes-nous?* - III. Prolongement. *VIII: Comparaison machine-être vivant.* - *IX: Particularités psychologiques compatibles avec notre machine.*

SCIAMA, D. W., **Modern Cosmology,** Cambridge University Press, London, 1971, VIII+212 pp., figs., tables and plates, £3.60.

The exploration of the Universe, as conducted by physicists, astronomers and cosmologists, is one of the greatest intellectual adventures of the mid-twentieth century. This book tells the story of their achievements and the new insight gained into the structure, history, working and scale of our Universe. Dr Sciama describes the major components of the Universe as they are known today: the stars, galaxies, radio-galaxies and quasi-stellar objects. He discusses in detail the red shift of the lines in their optical spectra, which leads to the idea that the Universe is expanding. Theoretical discussion of the expanding Universe suggests the possibility that intergalactic space may contain a significant quantity of matter and be the seat of important physical activity. This possibility remains to be confirmed observationally; the issues involved are here thoroughly debated. In the second half of the book, Dr Sciama discusses the discovery and significance of the 3 °K cosmic microwave radiation, its relation to the hot big bang and the helium problem, to cosmic high energy processes and to questions of isotropy. Much of this work is new and brings the reader to the threshold of the cosmology of the future.

Science and Synthesis (An International Colloquium organized by Unesco on the Tenth Anniversary of the Death of Albert Einstein and Teilhard de Chardin), Springer-Verlag, Berlin, 1971, XVIII+206 pp., DM 38.50.

The Colloquium, organized by UNESCO on the tenth anniversary of the deaths, within a week of one another, of Albert Einstein and Teilhard de Chardin, brought together eminent scholars in a free discussion of the need to effect a synthesis, both of scientific knowledge and of the scientific and philosophical approaches to knowledge. The theme commemorates the common desire of two very different men for an all-embracing concept of the universe and their single-handed attempts to construct a cosmology for the modern world. The discussions range widely: synthesis in the physical sciences, synthesis as a method of organizing modern science, and the coordination of scientific disciplines and research planning at the national and international level. Starting out from physics and Einstein's search for a unified theory, the argument proceeds by way of the problems of synthesis in the life sciences to those of the human sciences.

SIMON, J.-C., **Introduction au fonctionnement des ordinateurs,** Masson & C.ie, Paris, 1970, VI+130 pp., figs. and tables, 16 F.

CONTENTS. - *I: Introduction au fonctionnement des machines à programme.* - *II: Les codes et leurs applications aux machines.* - *III: Transmission de l'information.* - *IV: Les opérations.* - *V: Les mémoires à accès aléatoire.* - *VI: Langage et organisation des machines. Un exemple: ORDI 2.*

STANILAND, A. C., **Patterns of Redundancy** (A Psychological Study), Cambridge University Press, London, 1966, VIII+216 pp., figs. and tables, £2.25.

A discussion of various formal measures of redundancy, and of their reliability. The concept of redundancy, meaning the unnecessary reiteration of words or statements, is much used in modern psychology. This book looks at the relations between technical and less precisely defined uses of words in problems of identification, discrimination, classification and recall, and it raises many theoretical problems beyond the immediate issues of quantification.

CONTENTS. - *I: Patterns of Experiment.* - *II: Information and Redundancy.* - *III: Problems of Performance.* - *IV: Production of Redundant Material.* - *V: Measures of Wastefulness and Over-Determination.* - *VI: Choice of Experimental Measures.* - *VII: Recognition and Discrimination Experiments Using D-Redundancy.* - *VIII: Information and Memory.* - *IX: Memorization as a Problem.* - *X: Noise and Translation.* - *XI: Concepts of Information.* - *XII: The Informational Model.* - *XIII: Schematization and Generalization.* - *XIV: Redundancy and Performance.* - *XV: Redundancy of Action.* - *XVI: Informational Analysis of Classification.* - *XVII: The Meaningfulness of Interactions.* - *XVIII: Redundancy and Interaction.* - *XIX: Probabilistic and Perceptual Structures.* - *XX: Information and Gestalt.* - *XXI: The Problem of Grain.*

STECKER, F. W., **Cosmic Gamma Rays,** Mono Book Corp., Baltimore, 1971, x+246 pp., figs. and tables, $12.50.

CONTENTS. - *I: Gamma-Ray Production from the Decay of Secondary Particles Produced in Cosmic-Ray Interactions.* - *II: Gamma-Ray Production from Cosmic Electron-Positron Annihilation.* - *III: Gamma-Ray Production from Cosmic Proton-Antiproton Interactions.* - *IV: Gamma-Ray Absorption Mechanisms.* - *V: The Form of the Cosmic Gamma-Ray Production Spectrum from Galactic Cosmic-Ray Interactions for Gamma-Ray Energies Less Than 1 GeV.* - *VI: The Form of the Cosmic Gamma-Ray Production Spectrum from Galactic Cosmic-Ray Interactions for Gamma-Ray Energies Greater Than 1 GeV: The Effect of the Decay of Nucleon Isobars and Hyperons on the Cosmic Gamma-Ray Spectrum.* - *VII: Equilibrium Spectra of Secondary Cosmic-Ray Positrons in the Galaxy and the Spectrum of Cosmic Gamma Rays Resulting from their Annihilation.* - *VIII: The Production of Gamma Rays in the Galaxy.* - *IX: Introduction to Relativistic Cosmology.* - *X: The Cosmic Gamma-Ray Spectrum from Secondary Particle Production Outside the Galaxy.* - *XI: Cosmic Gamma Rays from Extragalactic Proton-Antiproton Annihilations.* - *XII: Extragalactic Gamma Rays from Compton Interactions and Bremsstrahlung.* - *XIII: Gamma-Ray Absorption Processes at High Redshifts.* - *XIV: Possible Interpretation of Isotropic Gamma-Ray Observations.*

SYNGE, J. L., **Talking about Relativity,** North-Holland Publishing Company, Amsterdam, 1970, 194 pp., figs., Dfl. 18.00.

In previous books the author has discussed the basic concepts of the theory of relativity (general and special) for readers well equipped mathematically. The present book aims at a much wider circle of readers, including scientists working in other fields, students, the general public, and even philosophers and psychologists.

CONTENTS. - *I: Talking about Concepts.* - *II: Talking about Geometry.* - *III: Talking about Algebra.* - *IV: Variables, Operators, Functions.* - *V: Pontificating about Events.* - *VI: The Queen and the Captain of the Guard.* - *VII: Particles, World Lines and the Arrow of Time.* - *VIII: The Mary Jane and the Penelope.* - *IX: Time.* - *X: The Line-Element and Curvature of a Surface.* - *XI: The Line-Element and Curvature of a Space-Time.* - *XII: Tensors.* - *XIII: The Field Equations.* - *XIV: From General to Special.* - *XV: Particles in Collision.*

TETAU, M. and BERGERET, C., **L'organothérapie** (Diluée et dynamisée), Librairie Maloine S.A., Paris, 1971, 240 pp., 20 F.

CONTENTS. - *I: Préface.* - *II: Introduction et historique.* - *III: Préparation et contrôle des souches organothérapiques.* - *IV: Prescription. Mode d'administration et posologie des médicaments organothérapiques.* - *V: Organothérapie diluée et dynamisée et immunologie. Mode d'action.* - *VI: Maladies auto-entretenues et organothérapie diluée et dynamisée.* - I. Applications thérapeutiques de l'organothérapie diluée et dynamisée dans les diffé-

rents domaines de la pathologie. *I: Maladies de l'appareil cardio-vasculaire. - II: Troubles de l'appareil respiratoire. - III: Troubles de l'appareil hépato-biliare. - IV: Troubles de l'appareil rénal. - V: Troubles de l'appareil digestif. - VI: Troubles de l'appareil génito-urinaire. - VII: Endocrinologie. - VIII: Maladies de la nutrition. - IX: Appareil ostéo-articulaire. - X: Ophtalmologie. - XI: Oto-rhino-laryngologie. - XII: Dermatologie. - XIII: Affections du système nerveux. - XIV: Maladies psychosomatiques. - XV: Gérontologie. Fiche organo-clinique. Constitutions Organotropiques. - XVI: Conclusions et perspectives d'avenir de la méthode. - XVII: Observations cliniques.* - Répertoire clinique. - Liste des principaux organothérapiques.

TORTRAT, A., **Calcul des probabilités et introduction aux processus aléatoires,** Masson & C.ie, Paris, 1971, XIV+304 pp., 60 F.

Cet ouvrage tient compte de l'évolution assez rapide de l'enseignement des probabilités. Il se situe au niveau maîtrise, et, tout en reprenant de façon complète les éléments nécessaires de mesure et intégration, en rejette certains dans l'appendice I. L'accent est mis dans le chapitre II sur la construction de la mesure dans les espaces produits, et ses restrictions aux « bons » espaces de trajectoires, de façon à étudier au chapitre V (outre les processus de Poisson généraux), le mouvement brownien et les processus à accroissements indépendants sans théorie de la séparabilité. Le chapitre III, étoffé de très nombreux exemples, présente l'étude des lois dans R^d et de leurs transformées de Fourier sous l'aspect, essentiel pour les extensions, des propriétés des familles équitendues, et consacre un long paragraphe aux lois indéfiniment divisibles de R^d et aux critères de convergence vers ces lois, ce point de vue analytique adapté à la statistique complétant le chapitre V. Quelques éléments de théorie ergodique terminent le chapitre IV, consacré aux convergences des suites de variables aléatoires. L'appendice II est un exposé général des rapports entre mesure et topologie, de théorèmes fondamentaux qui devraient devenir classiques, l'appendice III étudie la convolution dans un demi-groupe (ou groupe) topologique.

TUSQUES, J., **Organisation nerveuse et sensorielle de l'homme,** Librairie Maloine S.A., Paris, 1971, 380 pp., figs., 58 F.

CONTENTS. - *I: Préface. - II: Définition. Notion de neurone. Développement général du système nerveux.* - I. Organisation nerveuse et sensorielle à l'étage rachidien. *III: Développement et anatomie générale. - IV: Etude analytique concrète des neurones à l'étage rachidien. Le protoneurone moteur. - V: Les protoneurones sensitifs. Propriocepteurs - Intérocepteurs. - VI: Préalable à l'étude des structures extéroceptives - La peau ou tégument. - VII: Retour aux structures neuro-sensorielles proprement dites. Les protoneurones sensitifs (fin) - Les extérocepteurs - Les nocicepteurs - Récapitulation des structures neuroniques de la moelle épinière.* - II. L'encéphale. *VIII: Développement de l'encéphale. - IX: Le tronc cérébral. - X: La formation réticulée du tronc cérébral. - XI: Le cervelet et le système cérébelleux. - XII: Le cerveau. - XIII: L'épiphyse.* - III. Méninges. Liquide céphalo-rachidien. Plexus choroïdes. Névroglie. Vascularisation du système nerveux central. *XIV: Méninges. - XV: Liquide céphalo-rachidien. - XVI: La névroglie. - XVII: Notions sur la vascularisation du système nerveux central.* - IV. L'appareil visuel. *XVIII: Définition. Développement. Vue d'ensemble. - XIX: La rétine optique. Les voies optiques. Sommaire histophysiologique.* - V. Notions sommaires sur l'appareil olfactif, l'appareil gustatif et l'appareil auditif. *XX: Notions sommaires sur l'appareil olfactif. - XXI: Notions sommaires sur l'appareil gustatif. - XXII: Notions sommaires sur l'appareil auditif (acoustique et vestibulaire).* - VI. Le système nerveux végétatif. *XXIII: Rappel des grandes lignes des structures nerveuses végétatives. - XXIV: Système orthosympathique et système parasympathique. - XXV: Ultrastructures et problème de la terminaison du système nerveux végétatif.*

TYMIENIECKA, A.-T. (ed.), **Analecta Husserliana** (The Yearbook of Phenomenological Research), VOLUME I, D. Reidel Publishing Company, Dordrecht, 1971, x+208 pp., Dfl. 50.

The wide spread of phenomenologically inspired work on an international scale calls for a direct communication among specialized scholars. *Analecta Husserliana,* to appear once a year, is, in response to it again taking up the aim Husserl pursued in his *Jahrbuch für Philosophie und phänomenologische Forschung*, to facilitate collaboration among scholars towards the advance of original work in phenomenology.

VALLÉE, R. L., **L'énergie électromagnétique matérielle et gravitationnelle** (Hypothèse d'existence des milieux énergétiques et d'une valeur limite supérieure du champ électrique), Masson & C.ie, Paris, 1971, VI+138 pp., figs. and plates, 50 F.

Comment se présente l'énergie dans l'univers? Qu'est-ce que la matière? Qu'est-ce que la gravitation? L'étude de l'énergie électromagnétique matérielle et gravitationnelle, à l'aide des équations de Maxwell et de l'hypothèse d'existence des milieux énergétiques, semble permettre de répondre, d'une façon satisfaisante, à ces trois questions et conduit à la découverte d'une limite supérieure du champ électrique qui, par la loi de matérialisation, marque la frontière entre l'énergie matière et l'énergie diffuse responsable de la gravitation et du rayonnement cosmique. Cet ouvrage, qui se distingue par la remise en cause de principes qui semblaient fermement établis, abandonne résolument les théories abstraites et leur formalisme mathématique hermétique, pour renouer avec l'ancienne tradition d'une connaissance concrète et pratique des phénomènes physiques. Ecrit, d'autre part, dans le langage de l'ingénieur, il explique le comportement des photons, des électrons, des neutrinos, du rayonnement cosmique, ainsi que l'origine électromagnétique de la gravitation, en parfaite harmonie avec les résultats expérimentaux.

VALNET, J., **Aromathérapie** (Traitement des maladies par les essences des plantes), 4th ed., Librairie Maloine S.A., Paris, 1971, 408 pp., plantes, 32 F.

CONTENTS. - *I: Avant-propos. - II: Essences et plantes étudiées. Nom français et correspondance botanique. Nom botanique et correspondance en français. - III: Considérations générales, cas cliniques, observations, propriétés générales des essences aromatiques. - VI: A) Les essences classées par propriétés principales. B) Principales indications des essences étudiées. - V: Etude particulière des essences: nom botanique et famille. -VI: Thérapeutique aromatique. Formulaire. - VII: Quelques formules de prescriptions en pratique. - VIII: Observations de malades. - IX: Quelques formules de vins et vinaigres médicinaux, élixirs, espèces. - X: Variétés d'essences artificielles de fruits utilisées dans l'alimentation. - XI: Comparaison des mesures anciennes et nouvelles. Récolte et conservation des plantes, leur emploi en infusions et décoctions. Définitions des divers modes de préparation des plantes. Usage en thérapeutique. - XII: Conclusions. - XIII: Termes médicaux utilisés dans cet ouvrage.*

VALNET, J., **Traitement des maladies par les légumes, les fruits et les céréales,** 2nd ed., Librairie Maloine S.A., Paris, 1971, 410 pp., 28 F.

Le Docteur Jean Valnet, sans faire table rase d'aucune médecine, est arrivé à cette conclusion que beaucoup de maux pourraient être guéris de la façon la plus simple: par une alimentation bien conçue, où les vertus thérapeutiques des plantes et de leurs graines ou de leurs fruits pourraient jouer au maximum. La connaissance qu'il a de ces plantes lui a permis de dresser un inventaire de toutes celles qui nous sont utiles dans tel ou tel cas.

VAUGHAN, P., **Pro e contro la pillola** (Series: *L'attualità scientifica*, No. 2), Feltrinelli Editore, Milano, 1971, x+300 pp., L. 2.300.

CONTENTS. - *I: Qualcosa alla frontiera dell'ignoto. - II: La strada per Shrewsbury. - III: L'audace impresa di Goody. - IV: Un'enorme quantità di ghiande. - V: Un alto indice di sospetto. - VI: Una serie di controlli. - VII: Una pillola per ogni donna. -VIII:L'esperimento continua.* - Appendice: *La pillola in Italia* di Ettore Cittadini.

VERNON, M. D., **Reading and its Difficulties** (A Psychological Study), Cambridge University Press, London, 1971, VIII+212 pp., figs., £3.20.

A scientific enquiry into the psychological basis of reading, and into dyslexia. The Author considers visual perception, reasoning, motivation, and outlines recent work in psycholinguistics in order to define the basic psychological processes involved in learning to read. She also discusses the problem of specific developmental dyslexia (i.e. marked reading disability apparently not caused by environmental factors, low intelligence or emotional maladjustment), and concludes that the basic deficiency may lie in the processes of conceptual reasoning necessary to form associations between sequences of printed and 'sounded' letters. A fundamental disfunction of the brain may also be involved. She stresses the importance of early recognition and remedial treatment of reading difficulties, since these may be alleviated, if not entirely overcome, by suitable treatment.

VIÉNOT, J.-C., SMIGIELSKI, P. and ROYER, H., **Holographie optique** (Développements, Applications), Dunod, Paris, 1971, XIV+218 pp., figs., 68 F.

L'holographie technique d'enregistrement de l'amplitude et de la phase d'une onde transportant une information tridimensionnelle a connu un développement rapide à la suite de l'avènement des lasers en tant que sources de lumière cohérente. Cet ouvrage, dont les auteurs ont été les premiers en France à réaliser des hologrammes et qui poursuivent actuellement des recherches en holographie, est une étude détaillée de cette technique. Dans une première partie, sont exposés les fondements de l'holographie et ses propriétés essentielles, présentés le traitement des aberrations, la reproduction du relief ainsi que les problèmes expérimentaux de la technique d'enregistrement et de reproduction. La seconde partie traite des diverses applications de l'holographie dans les sciences appliquées et dans l'industrie (cinéma et télévision en relief, spectroscopie, holographie en acoustique, en microondes...) ainsi que ses perspectives d'avenir. L'importante bibliographie qui complète ce volume représente un outil de travail précieux. De plus, une abondante illustration facilite la compréhension du lecteur. Ce livre intéressera tous ceux qui désirent suivre les progrès des applications du laser, mais aussi les spécialistes qui trouveront dans les annexes des explications plus élaborées.

WENDLING, E., **Problèmes de chimie minérale physique** (Maîtrises de chimie et de chimie physique), Dunod, Paris, 1970, XVIII+348 pp., figs., 48 F.

Cet ouvrage comporte quatre-vingt dix problèmes suivis de leur solution et classés en six chapitres. Le chapitre A propose des problèmes classiques de cristallochimie. Le chapitre B fait ressortir l'infinie variété de problèmes basés sur la considération des enthalpies réticulaires. Dans le chapitre C, sont proposés des problèmes précisant la nature de la liaison hydrogène ou soulignant l'interdépendance des notions d'énergie et de constantes de force de liaison. Le chapitre D est consacré au calcul de potentiels d'électrodes et de $\mathrm{pK_A}$. Deux théories originales y sont également abordées: celle de l'acidité en milieu fondu, selon Lux, Flood et Förland et celle des acides et bases durs et doux. Le chapitre E amène le lecteur à prévoir des coordinences en se basant sur la théorie électrostatique des complexes, des hybridations grâce à la méthode de Kimball, l'existence de molécules à déficit électronique ainsi que celle de liaisons σ courbes. Enfin, les problèmes du chapitre F font appel aux notions de séries spectrochimiques, nephélauxétiques, hyperchromiques, à celles d'énergie de stabilisation de champ cristallin, d'effet Jahn-Teller ainsi qu'à celle d'électronégativités optiques.

WEYL, H., **Raum, Zeit, Materie** (Vorlesungen über allgemeine Relativitätstheorie), Springer-Verlag, Berlin, 1970, VIII+338, pp., figs., DM 29.70.

CONTENTS. - *I: Der Euklidische Raum: seine mathematische Formalisierung und seine Rolle in der Physik. - II: Das metrische Kontinuum. - III: Relativität von Raum und Zeit. - IV: Allgemeine Relativitätstheorie.*

World Survey of Climatology, VOL. II: **General Climatology II** (H. FLOHN, ed.), Elsevier Publishing Company, Amsterdam, 1969, XII+266 pp., figs. and tables, Dfl. 95.00.

The first three volumes of the series World Survey of Climatology deal with the general aspects of Climatology, as an introduction to the following twelve volumes,

in which a detailed study of the climates of the various climatological provinces of the globe will be given. Volume 1 deals specifically with the basic properties of radiation and heat budget, giving the physical background of atmospheric processes together with the elements of climate, and their diurnal and annual courses. Volume 3 comprises reviews on the biological effects of climates on natural vegetation, agriculture, animals and man. Due to the rapid increase in the world population and its technological progress, the incorporation of a comprehensive review on artificial climates has proved to be necessary. The present volume contains reviews on the general and local circulation of the atmosphere and the physical processes at and above the surface of the earth — which can be considered as the fundamental climatogenetic factors — as well as reviews on the local climates created by these processes. Also covered are the remarkable world-wide climatic fluctuations during the history of the instrumental period (since approximately 1680).

YOURGRAU, W. and BRECK, A. D. (eds.), **Physics, Logic, and History** (Based on the First International Colloquium held at the University of Denver, May 16-20, 1966), Plenum Press, New York, 1970, xiv+336 pp., $14.00.

By mutual exploration of the ever-present links between logic, physical reality, and history, the contributors to this volume have attempted to remove the artificial barriers between these three disciplines. Internationally prominent physicists, logicians, and historians discuss such topical problems as the connection between technical development and economic history, knowledge and physical reality, non-quantal foundations of quantum mechanics, and the evolution of evolution.

Contents. - *I: A Realist View of Logic, Physics, and History*, by Sir K. Popper. - *II: Knowledge and Physical Reality*, by A. Mercier. - *III: The Science of History and Notions of Personality*, by H. Tennessen. - *IV: The Growth of a Theoretical Model: A Simple Case Study*, by H. Törnebohm. - *V: Existence*, by W. V. O. Quine. - *VI: The Problems of Unifying Cosmology with Microphysics*, by D. D. Ivanenko. - *VII: The Evolution of Evolution*, by J. V. Langmead Casserley. - *VIII: Plea for Pluralism in Philosophy and Physics*, by A. Naess. - *IX: On Semantic Information*, by J. Hintikka. - *X: Quantification and Ontological Commitment*, by C. Lejewski. - *XI: Possible Internal Subquantum Motions of Elementary Particles*, by J.-P. Vigier. - *XII: The Three Kings of Physics*, by G. Gamow. - *XIII: Scepticism and the Study of History*, by R. H. Popkin. - *XIV: Causation in History*, by R. S. Cohen. - *XV: Relativity Theory and Historicity of Physical Systems*, by H.-J. Treder. - *XVI: General Relativity as an Open Theory*, by H. Bondi. - *XVII: What is Russell's Theory of Descriptions?*, by D. Kaplan. - *XVIII: The Non-Quantal Foundations of Quantum Mechanics*, by A. Landé. - *XIX: Some Problems of the Connection between Technical Development and Economic History*, by G. Ránki. - *XX: Is Science Human?*, by H. Bondi.

ZEMANN, J., **Chimie cristalline** (Series: *Monographies Dunod*, No. 78), Dunod, Paris, 1970, xiv+162 pp., figs. and tables, 26 F.

Cette monographie présente d'abord les concepts élémentaires de la cristallographie, tels que: les symétries, les groupes d'espace, la maille, l'unité asymétrique, c'est-à-dire toutes les notions faisant partie du langage presque normalisé dans lequel on décrit actuellement les structures cristallines. Les concepts permettant de comprendre la stabilité et la cohésion des solides, tels que les rayons ioniques, atomiques, les types de liaisons ioniques, métalliques, covalentes, sont ensuite introduits et permettent de mieux comprendre les règles de Pauling et la notion de l'électronégativité. Les questions d'iso- et poly- morphie des solutions solides et de composés intermétalliques sont traitées et des chapitres spéciaux étudient les silicates, les hydrates et hydroxydes, ainsi que les sulfures.

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GENETICS AND THE FUTURE OF MAN

There is at the present time much interest in the problems of mankind, as a result of the great social changes in progress and the new demands imposed by the technological revolution. The social environment of man has removed him from the influence of natural evolution since his emergence. Man's genetic reserves are practically inexhaustible as a result of his consciousness, the social programme instilled during education has a leading role in the creation of personalityt An important task is to preserve the heredity of mankind, both from changes in the environmen. and from uninformed interference by eugenists who biologize the social categories of human personality. The man of the future will be, and is already being, created by social changes. The problem before human genetics is to overcome congenital diseases and other biologically harmful defects.

INTRODUCTION. — The main challenge facing human genetics is to what extent the existing genetic potential of man will provide an adequate basis for social progress in the future. Now and again in the past and also at the present time the view has been put forward that the human genetic endowment was inadequate, and hence that the genetic improvement of mankind was necessary. It gave rise to the teaching of eugenics aimed at the creation of a new and better man by means of genetics.

The problem of man is exceptionally complicated inasmuch as he is a social being, on the one hand, and evolved from the animal kingdom, on the other; therefore man is a result of interplay between the social environment and biological factors.

The significance of the biological structures for man's existence is evident from the fact that his genetic information is stored in his DNA molecules. All his biological characteristics including consciousness are coded by the hereditary structures. Continuous transmission from generation to generation of gradually evolved genetic molecules of DNA specific for the species *Homo sapiens* is an indispensable prerequisite of man's existence. Man rational furnishes a unique link in the evolution of life; no evidence available suggests the existence of other rational beings anywhere else. Dramatic advances in the space sciences have shown that the Earth, with its screening atmosphere protecting life from space radiation, its oceans and continents involved in the circulation of substances in the biosphere, is a real pearl of the solar system. It engendered life whose evolution is now a continuous, now a saltatory process. Among evolutionary saltations the major ones were the emergence of life out of inorganic matter and of consciousness related to the appearance of man.

The present and future possibilities of mankind rest with social changes in the world. The human growth pattern, however, is governed by biological principles. Human development, health, longevity, harmonious development of capacities, senescence, death - all these are programmed in the human DNA molecules. Consciousness has made man superior to all the other forms of life. Whether man will be able to protect his precious heredity from untoward effects is of vital importance to his future.

EVOLUTION OF LIFE AND MAN. — The progress of life on the Earth is governed by the laws of heredity, variability and natural selection. Expediency arising from the evolutionary requirements of the developmental patterns of species in particular environments is manifest in selection of objectively random mutations. Expediency is inconceivable without selection, in the course of which products of evolution combined with new qualities express unity of the organism and environment.

The emergence of the social environment has released man from the inevitable action of evolutionary laws. That man has overridden the effects of natural selection which consequently began to play a minor part in human populations has placed man beyond the influence of natural laws of evolution. Instead, his progress has become dependent upon social factors. The growth of a collective consciousness keeping the record of social progress has implemented the feedback so significant for his further progress.

This new attitude of man has not eliminated the role of heredity and variability. On the contrary, they are of paramount importance for the developmental pattern of an individual. The life pattern of any individual turns on heredity and variability, which makes investigations into general and medical human genetics so enormously important. Every man shares in the group peculiarities, so enquiries into the genetics of populations constitute another most important branch of contemporary human genetics.

Among the determinants of the characteristics of human populations is mutation pressure. This permanent genetic factor causes the origin and spread of a wide range of mutations in human populations. The general survey of mutation distribution in populations was initiated in 1928 by S. S. Chetverikov, who showed wild flies of the genus *Drosophila* to be heterozygous for a number of mutations. Six years later, in 1934, the phenomenon of the genetic load, i.e. widely spread deleterious defective genes, was discovered by N. P. Dubinin et al. It was shown that outwardly normal, viable, well-adapted *Drosophila* flies carried in a heterozygous state three or four lethal genes and several genes affecting viability. It became clear that a part of the progeny homozygous for lethal and semi-lethal genes was fated to die and another part to have hereditary malformations. Later on the role of dominant genic mutations, balanced polymorphism, nondisjunctions and chromosomal structural mutations was appreciated.

Much of the theoretical and experimental work on various animal and plant species has revealed that the mutation rate and coefficient of natural selection are correlated to the level of the genetic load. Selection eliminates an overwhelming majority of mutations in wild species. As regards man, due to drastic reduction of the effects of natural selection only a fraction of mutations contributes to the genetic load. An average of four per cent children are born with congenital diseases and malformations. If the present mutation rate is constant, the level of the genetic load will not be higher in an indefinite series of generations.

The neutral hereditary variability displayed by man does not enter into the genetic load. This is an important distinction of the genetics of human populations from other animal species. Such neutral heredity concerns blood groups, eye colour, external ear structure, form of nose, peculiarities of hair structure, the ability to taste phenylcarbamide etc., that is the whole diversity of biochemical, morphological, physiological and other characters which do not interfere with man's life under the existing circumstances.

Due to advances of medicine the bulk of the genetic load is decreasing. When radical treatment is found, the formerly deleterious gene passes into the class of neutrals. E.g., short-sightedness was pernicious for our ancestors. With the invention of spectacles this property became neutral.

Some people hold the view that, in protecting health of individuals, medicine interferes with the wholesome heredity of the species.

In fact, according to theoretical studies of the genetics of populations, the growth in concentration of negative mutations which are, to some extent, neutralized by medicine turns on the mutation pressure. Calculations show that, for an entirely neutral mutation, doubling of its concentration from 0.1 to 0.2 at the mutation frequency 1 per 100,000 gametes would take 25,000 years (the reproductive generation is assumed to be 25 years).

This proves that the idea of biological degradation of man through the origin of new mutations is in error. What is essential about the genetic structure of modern man is that it will essentially survive for a very long time (relatively long, of course). Meanwhile man is endowed with enormous personal plasticity both in his perception of social programme and his adaptation to immediate environmental effects.

THE GENETIC POTENTIAL OF MANKIND IS INEXHAUSTIBLE. — Mankind, the single species that it is, historically falls into over 30 races and a great number of populations. Throughout its existence from the neolithic period to the present time it ha srevealed tremendous genetic potential. Two tempestuous processes going on in modern human populations are growth of size and intermixture. The former will have reached its peak by the beginning of the XXIst century, the world population will have exceeded 10 thousand million, after which it will reach a plateau. Possible constraints to population size will be on the agenda then, causing numerous genetic problems. The second process, population intermixture, the contemporary stage of which began after the discovery of America by Columbus, is still expanding and after 2-5 millennia it will bring about a genetically unified human population. It too will raise serious problems for human genetics.

Analysis of the processes of natural mutagenesis, population size growth and intermixture enables one to ascertain that nothing threatens the genetic system of man. The level of the genetic load will not get appreciably higher in the millennia to come. The genetic system of man is undergoing negligible changes, with the result that the powerful potential of the genetic information of man will sustain him for a long time. The genetic information of man will be impaired, should a sharply increased mutagenic background develop in the habitat. Meanwhile the level of mutagenic factors in the biosphere, such as radiation, chemical agents etc. as well as the mutational process itself are to be controlled by man. Given all this, mankind has a virtually inexhaustible genetic potential which may serve as a ground for the progress of his social evolution.

PROGRAMME OF SOCIAL INHERITANCE. — Such a characteristic of the human species as consciousness, together with his biological organization, shows that the development of an individual man is governed by a close combination of two programmes. One is genetic information carried by a set of genes inherited from parents, the other is the social programme perceived through nurture. Through the social programme a man experiences the influence of the whole of human history crystallized in his contemporary social environments. Hence it is manipulation of the environment, the raising of living and cultural standards, that will immediately lead to the improvement of man's own self.

Consciousness presents a colossal advantage of the human growth pattern. It is the highest form of creative reflection of objective reality and self-knowledge, and it is specific to him alone. Consciousness gives man the possibility of unlimited self-improvement through interaction with other people and with the environment. In the development of man as a social being, though his biological development is based on the genetic programme, the inner incentive is social progress ensuing from the development of productive forces, rather than his genetic endowment. Historically, consciousness originates from and is influenced by social progress.

The future of man as a rational being turns entirely on his social progress, its qualitative and quantitative contents, providing that his genetic programme is valid for many millennia to come. The potentialities of material and the contents of spiritual culture, the different patterns of typified social behaviour, the standards of good and evil, patently furnish an impressive picture of social inheritance. All this is possible only because people are all different in biological terms; each is biologically unique, possessing a specific genetic constitution. It poses complicated problems in the course of nurture. The combined influence of the social and genetic programmes also makes every man spiritually unique. This is the supreme and irreplaceable moral and ethical foundation of man's existence. The moment we enter the path of biological or social standardization of people, mankind will be doomed. It is obvious that progressive social typification of man ensuing from common appeals of the social programme has nothing in common with standardization of man's spiritual world.

DEVELOPMENT OF POPULATION GENETIC SYSTEMS AND THE PROBLEMS OF SAFEGUARDING HUMAN GENETIC INFORMATION. — The genetic information of man realized in individual development is the greatest achievement of evolution; it represents the highest level of the structural - functional organization of living matter. Are we really confronted with the need for human genetic improvement? If we admit that the social progress of man is related to a consciousness capable of infinite self-development, then I believe that there is no such necessity. Besides, we do not know any real way of directing change of human genomes toward a desired end, for our knowledge of the involvement of the genetic component in the formation of personality is, in both qualitative and quantitative terms, quite inadequate.

We do not yet know how far the existing human potentialities for cultural development can bring man. No doubt if our children found themselves deprived of any information on contemporary culture immediately after birth, they would sink to the level of our ancestors tens of millennia ago. If culture and education had been available at that time, the progeny of those « primitive » people would have immediately ascended to our level.

Since our knowledge of the essential characteristics of human personality in terms of genetics is so very meagre, where should we be led by uninformed manipulation of the genetic programme of man? Attempts to improve his genetic system would no doubt damage the present unique biological organization distinctive of man. We must get a deep understanding of the genetics of man and the principles underlying the interplay between the genetic and social programmes during personality development to avoid fatal mistakes. Until that is achieved, nobody will be in a position to try on man any miracle of molecular or general genetics with the aim of influencing the genetic constitution of mankind as a biological species.

When the genetics of man attains a much higher level, the problem will arise how to describe the ideal man, better than we are? What techniques can be applied to an individual and to mankind as a whole? How can we prevent the possibility of employment of human populations as material for irresponsible breeding experiments like those performed with animals? How can we avoid the blunder of biological standardization of people, on the one hand, and neo-racism equipped with genetic engineering, on the other? When considering the problem of interference in the genetic system of man, we are confronted with grave genetic, ethical, sociological and philosophical problems. In the long run, all eugenic breeding methods proposed so far by different authors are inherently antihumane, and more often than not class-biased.

Rejecting eugenic proposals, we must pay due respect to studies of the biological problems of modern man by the methods of medical, general, molecular and biochemical, cellular and population genetics.

High priority should be given to the development of methods of diagnosis and treatment of congenital diseases and genetic preventive measures for them, bearing in mind the possibility of controlling the mutation process. Control over the mutation process would lead to a sharp reduction of the genetic load in human populations. To interfere in population processes and genetic characteristics of individuals with the aim of recovering normal heredity would be to utilize the most sophisticated achievements of molecular and general genetics. We must remember that in terms of biology every moment of our life is controlled by the genetic programme. It influences cardio-vascular diseases, cancer, disorders of old age, early death etc. The study of man in terms of the nature of his genetic structures, the genetics of his origin and his future and the possibilities of recovery from hereditary defects furnishes quite a new approach. It is no exaggeration to contend that, contrary to the older eugenics, this approach is the most promising of all those that have emerged throughout the history of civilization.

Significant for the understanding of the general problems of human genetics is the recognition that man as a biological species has not undergone any radical changes during the many millennia since his emergence. No less important is that sociogenesis and historical developments after his emergence have changed the genetic system of human populations greatly.

At the dawn of man's existence the isolation of populations and natural selection played an important part. The development of material and spiritual culture brought about many changes: populations grew in size, natural selection fell off, populations began to mix. All these processes unambiguously caused reduced expression of the genetic load, obstructed the way of genetic-automatic processes, prevented multiplication of occasional genes, created the conditions for a sharp increase of human genetic diversity and led to the manifestation of heterosis.

This beneficial process of improving conditions for the expression of the human genetic potential has been mounting, and now it has entered a new stage characterized by a tremendous growth of population size and by race and population mixtures.

Taking into consideration all the foregoing and proceeding from a realistic assessment of the role of mutation, it is obvious that eugenic arguments that mankind shows biological degradation are groundless. On the contrary, there is evidence of improvement of the genetic systems in human populations. These findings are of central importance for the understanding of the role of his genetic structures in the future of man.

Among the problems of applied human genetics the major ones should be taken to be protection of his genetic information and elimination of lesions ensuing from mutations. It should be noted that man's genetic endowment is absolutely intact and undamaged in all the races and populations in terms of population genetics. The genetic load carried by populations, however, is expressed in some individual people as physical and mental incapacity. They are victims of their population, of their kin. At the same time, every man has to face problems such as the normal operation of his genetic information during development and in the course of his life, longevity, and a wide range of genetically determined biological properties.

It should be stressed that in the evolution of life man as a biological species is exceedingly young. Typical representatives of *Homo sapiens* appeared only about a hundred millennia ago. During this time man has not got rid of any of his hereditary structures but improved the conditions for expression of his genetic potentialities. It is safe to say that human potentialities are inexhaustible. But this does not make the problems of genetics and the future of mankind any simpler. It needs to be stressed that they are tremendous challenges. They are not restricted to genetics alone, but require knowledge of most recent advances and approaches of general biology, sociology, ethics and philosophy as well as all the achievements of material and spiritual culture. Rising above the incidence of natural laws of evolution man is to look deeply into the mechanics of his genetic information, become its master, exercise science-based protective control over it, creating all the necessary condition for wholesome heredity to flourish and for the development of all his faculties. The advance of human genetics, bringing us ever nearer to a full understanding of human biology, lays a heavy social responsibility on geneticists. These advances may have grave social consequences, for the boundless social evolution of men requires that all aspects of his astonishing genetic endowment be preserved and further developed.

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IS THE THEORY OF EVOLUTION DIFFERENT?

I - THE CENTRAL CORE OF THE THEORY

This is the first part of a two part paper in which I attempt to analyse modern evolutionary theory in the light of claims which have been made about its nature by a number of philosophers. In this part I begin by sketching what is a fairly widely accepted view of scientific theories (in general). I point out that such theories are axiomatic systems containing laws, referring to observable and unobservable kinds of entities, and having features leading to their own improvement. Then I introduce what I call the « Evolutionary Theory Core » — this is the part of biology more commonly known as « population genetics ». I argue that, contrary to the claims of J. J. C. Smart, this part of biological theory fits, in a fairly exact manner, the general model of scientific theories which has already been sketched. In particular, I argue that one has an axiomatic system containing laws, that reference is made to observable and unobservable entities, and I show how this part of biology contained within itself the seeds of its own improvement.

Many philosophers have argued that biology is a science of a quite different nature from physics and chemistry [1]. In this paper I want to consider the argument of three such philosophers, all of whom base their cases for the supposed peculiarity of biology on differences which they believe to exist between the theory of evolution and theories in the physical sciences. I shall argue that these three — J. J. C. Smart, Morton Beckner, and T. A. Goudge [2] — fail to make their cases, and I shall also argue that despite a certain looseness, evolutionary theory is fundamentally similar to the theories of physics and chemistry [3]. In order to carry out my plan I shall consider the three critics of evolutionary theory in the above order; but before turning to them, so that the reader might have a firm grasp of the crucial points at issue, I shall start the paper with a brief resumé of the major features of physical theories. As I begin this task, I must emphasize that throughout the paper my major interest will be that of providing a positive analysis of evolutionary theory, rather than merely that of refuting my opponents — consequently, where necessary, I shall not hesitate to go beyond the strict limits of the critics' arguments in order to draw attention to points of likeness between evolutionary theory and other scientific theories.

1. Theories in the Physical Sciences. — Not everyone agrees on the nature of theories in the physical sciences; but most people (like myself) think that where such theories are fully developed, they are 'axiomatic' systems, or, as they are sometimes called, 'hypothetico-deductive' systems [4, 5]. What this means has, perhaps, best been explained by R. B. Braithwaite. He writes:

> A scientific system consists of a set of hypotheses which form a deductive system; that is, which is arranged in such a way that from some of the hypotheses as premisses all the other hypotheses logically follow. The propositions in a deductive system may be considered as being arranged in an order of levels, the hypotheses at the highest level being those which occur only as premisses in the system, those at the lowest level being those which occur only as conclusions in the system, and those at intermediate levels being those which occur as conclusions of deductions from higher-level hypotheses and which serve as premisses for deductions to lower-level hypotheses. (Braithwaite, 1953, 12).

In order to avoid possible future confusion, a couple of points raised by this passage must be dealt with at once. First, it should be noted that it is not being claimed that no evidence at all can be offered in support of the hypotheses which act as premises in a hypothetico-deductive system — there may indeed be a great deal of direct evidence for the premises. Rather, the point is that in a particular system certain hypotheses are taken as basic — that is, they are not justified within the system. Secondly, a point which is closely associated with the first point is that although certain hypotheses may be used as premises within one particular system, they may quite possibly be derived in another system. Similarly, derived hypotheses in one system may be premises in another (possibly later) system. As physical (and chemical) theories are developed, one quite often finds this juggling of hypotheses to create the most desirable system.

There are many different aspects of hypothetico-deductive systems that one could discuss, but here I shall just pick out for brief attention three features which seem to be fairly widely shared by the theories of physics and and chemistry.

a. Physical theories contain laws. — The first significant feature of physical theories that I want to mention is that the hypotheses of physical theories are of two kinds. First, one finds *a priori* propositions, in particular, propositions which express logical and mathematical truths. Secondly, one finds *a posteriori* propositions, that is to say, propositions which are in some sense about the world of experience. These latter propositions are general propositions, in other words they are not about particular individual things, but are instead about the relationships which hold between sets of things. Since they are *a posteriori*, they are not logically necessary, and so their denial is not contradictory; however, their truth does seem to be more than a matter of mere chance — in some sense the empirical hypotheses of physical theories seem to tell us about relationships which *must* occur given certain combinations of members of certain classes of things. It is customary to refer to the empirical necessity of the *a posteriori* propositions of physical theories as 'nomic' necessity. These *a posteriori* propositions are said to be 'law-like', or are just plainly called 'laws' [6].

b. Physical theories refer to entities of different kinds. — A second fairly common feature of physical theories is that there seems to be a significant difference in the entities referred to in the higher level hypotheses and the entities talked about in the lower-level hypotheses. In particular, although the entities referred to at the lower-levels are fairly obviously objects of immediate experience — metals, gases, prisms, pendulums, light configurations on screens, and so on — the entities referred at the upper-levels are usually anything but the sorts of things which one could experience directly. At the upper-levels one finds talk of things like 'electrons' and 'protons', things whose very nature seems to preclude the possibility of one's ever coming into contact with them directly through the senses. The existence of these entities, usually called 'theoretical entities', has to be inferred indirectly. Physicists and chemists employ them in their theories and (usually) are prepared to accept their existence, because propositions about such entities enable one to predict the occurrence of other phenomena, which other phenomena are directly observable. By using theoretical entities, one can greatly increase the predictive power of a theory.

Now clearly, in theories which do contain reference to both theoretical and non-theoretical (*i.e.* directly observable) entities, there must at some point occur a change in the language, from talk about theoretical entities to talk about non-theoretical entities. What one must have are translation rules which enable one to go from talk of one kind of entity to talk of another kind, since otherwise, one could not deduce the lower-level hypotheses (with no references to theoretical entities) from the higher level hypotheses (with references to theoretical entities). These translation rules are often called 'bridge' principles. A good example of such a principle occurs in gas theory where Boyle's law is explained by reference to the behaviour of molecules. At some point in the derivation of Boyle's law one uses the bridge hypothesis that the mean kinetic energy of the molecules of a fixed mass of gas does not alter so long as the temperature of the gas remains the same. One has here a bridge between the theoretical (the molecules) and the non-theoretical (the temperature of the gas) [7].

c. Physical theories are self-correcting. — The third and final feature of physical theories that I want to highlight is the fact that although the essential characteristic of such theories seems to be that they unify all our knowledge in a particular field — they draw together into one body all kinds of apparently disconnected experiences and diverse explanations — they also very frequently act as correctives to our knowledge. Almost paradoxically, quite often through the devising of a particular theory the very things which spurred the making of the theory are themselves shown to be not quite correct. The classic example of this corrective aspect of theories is shown by the relationship between Kepler's laws and Newtonian Mechanics. Newtonian Mechanics shows that the regularities spotted by Kepler are not just isolated phenomena, but are rather part of a much wider order of things. However, in doing this, it also shows that Kepler's Laws are not quite accurate, and more importantly, it shows why they are not accurate and provides substitutes which are closer to the truth. Thus, here and elsewhere, it seems that physical theories unify our knowledge, provide directions for the explorations of new fields, and help us to improve the information which we have already gathered.

Many more things could be said about physical theories; but they are not my main concern in this paper. What I have tried to do has been to pinpoint some (but by no means all) of the major features of physical theories, and on the basis of this, I hope shortly to be able to compare such theories with evolutionary theory. Consequently, brief though the discussion has been, let us now leave physical theories and turn to biological considerations.

2. THE CORE OF EVOLUTIONARY THEORY. — The theory of evolution which I want to examine in this paper is that which is accepted by nearly every modern biologist and which is called the 'neo-Darwinian' or the 'synthetic' theory of evolution. It is the theory which is discussed from various viewpoints in such important works as J. S. Huxley's *Evolution: The Modern Synthesis*, G. G. Simpson's *The Major Features of Evolution*, and, more recently, E. Mayr's *Animal Species and Evolution.* I shall not attempt an analysis of the whole theory in this section, but shall here concentrate on but one part, and I shall be concerned to defend it against a general criticism levelled by J. J. C. Smart against all biological theories.

It is Smart's claim that biological (and psychological) theories must differ from the theories of physics and chemistry because the former, unlike the latter, can never be hypothetico-deductive or axiomatic systems. He writes:

> Writers who have tried to axiomatise biological and psychological theories seem to me to be barking up the same gum tree as would a man who tried to produce the first, second, and third laws of electronics, or of bridge building ... The writers who have tried to axiomatise biology or psychology have wrongly thought of biology or psychology as a science of much the same logical character as physics, just as chemistry is ... [However] the important analogy is not between biology and the physical sciences but between biology and the technologies such as electronics. (Smart, 1963, 52).

Hence, for Smart, since evolutionary theory is obviously a biological theory, it must have a structure radically different from any theory to be found in physics and chemistry.

I think that Smart is quite wrong in this claim, and I intend to show in this section that at least one part of evolutionary theory does have an axiomatic structure. Indeed, I intend to go even farther, and to show that this part shares with physical and chemical theories all the other features which I discussed in the last section. The reader might think it strange that I should restrict my discussion to but part of evolutionary theory; however, I have two reasons for so doing. In the first place, enough of the theory will be covered in this section to show Smart's claim to be false. The amount covered can stand in its own right as an area of biology which has been axiomatized. In the second place, the part I shall be covering is, I believe, in many respects the most important part of the theory of evolution. It is the part which is *logically* prior to the rest of the theory (*i.e.* the rest of the theory presupposes knowledge of it), and because I place so much importance upon this part (which I shall refer to as the 'core' of evolutionary theory), I want to consider it separately from the rest of the theory. The other parts of the theory, and the relationship of these parts to the core of evolutionary theory, will be the subject of the next section of this paper.

What I call the 'core' of evolutionary theory (the 'ETC' for short) is that area of biology which is today usually referred to as 'population genetics' [8]. To start the discussion about the ETC (*i.e.* population genetics) it is necessary to understand that there is one entity around which the theory revolves, namely the 'gene' [9]. The gene, it is argued, is a thing with many properties, but by definition it is considered to be the thing which is ultimately responsible for physical characteristics — the gene it is which makes us what we are, and which makes each one of us different from every other organism but similar to many. Genes are the ultimate causes of arms, legs, eyes, teeth, noses, brown hair, black hair, curly hair, straight hair, no hair, black skin, white skin, smooth skin, rough skin, and so on, and so on. Because the gene is responsible for these different characteristics, it is in this sense often referred to as the 'unit of function'. However, although this is the way in which the gene is usually defined, and although as we shall see shortly this definition gives rise to many vital bridge principles in the ETC, it is customary when presenting the ETC to concentrate on another aspect of the gene, namely the gene *qua* unit of inheritance. It is argued that the gene is the thing which is passed on from one generation of organisms to the next, thus ensuring the continuity of life and also ensuring that (roughly speaking) like things are born of like. Humans beget humans, for example, and not giraffes.

The way in which the gene acts as a unit of inheritance is expressed in two basic hypotheses known as Mendel's Laws. These can be stated as follows.

The First Law (also known as the 'law of segregation'). Each individual (in particular, each sexual individual) possesses a set of genes, which genes usually retain their individuality from generation to generation. The set of genes is divided into two, each gene of one half having a corresponding mate in the other half. When the sex-cells are formed, one and only one of each pair is incorporated into any particular sex-cell, and it is a matter of chance which one of the pair is so incorporated. (*n.b.* Organisms are the product of the fusion of ne sex-cell ofrom each parent).

The Second Law (also known as the 'law of independent assortment'). When more than one pair of genes is considered, each pair segregates independently of all other pairs (*i.e.* if one member of a particular pair is incorporated into a sex-cell, this in no way influences the choice of which other genes will be incorporated into that sex-cell) [10].

From these two laws, together with a number of fairly elementary propositions of mathematics, one can deduce many other propositions about the way in which genes will be inherited [11]; but the most important, is the proposition known as the 'Hardy-Weinberg Law'. It is easiest to understand this law in the simplified special case where one considers just genes which occur in a particular pair of a particular kind of organism, and where one assumes that there can be only two kinds of gene (known as 'alleles') in the pair (say A and a). The law then states that given a large population of these organisms, given that they mate with each other at random, and given also that there are no external forces disrupting the proportions of the genes, then after the first generation, no matter what the original distribution of the genes, the proportions will assume discoverable ratios, and from the first generation on, the ratio of the two kinds of gene to each other will remain *constant* for every generation. In particular, if the ratio of A genes to a genes is $\mathrm{p}:\mathrm{q}$, then all generations after the first will be divided as follows.

$$\mathrm{p}^2 AA : \quad 2\mathrm{pq}\ Aa : \quad \mathrm{q}^2 aa$$

(an AA is an organism with two A genes, and so on). Since $\mathrm{p} + \mathrm{q} = 1$, the ratio of A genes remains always as $\mathrm{p}:\mathrm{q}$. Obviously it is possible to generalize this law for more alleles, and, by using Mendel's Second Law, to extend the law over other genes possessed by an organism.

The importance of the Hardy-Weinberg Law should not be underestimated — in particular, one should not be seduced into concluding that because the law apparently says that if nothing happens (*i.e.* if there are no external forces disrupting gene ratios) then nothing happens (*i.e.* gene ratios remain constant), it is little more than a truism. The law is no more a truism than is Newton's First Law, which tells us that things stay much as they are unless something alters them. Indeed, in a sense the Hardy-Weinberg Law is important for the same reasons as is Newton's First Law. Because nothing changes without a cause, evolutionists are able to introduce into their system propositions which specify certain understood causes, and then they can deduce the effects that these causes have on the gene ratios of certain specified populations.

There seem to be two major causes of gene-ratio-change which are introduced into ETC. These are *mutation* and *selection* [12]. First, it is argued that although genes are usually very stable, remaining unchanged from generation to generation, sometimes they change (to all intents and purposes instantaneously) from one form to another. This change is called 'mutation', and if the gene changes back to its original form, it is called 'back-mutation'. Although it is a basic assumption of the ETC that one cannot tell exactly when a particular gene will mutate, it is assumed that given a large set of similar genes it is possible to assign a calculable figure to the overall percentage of genes that will mutate in a particular way in a given generation. Given this assumption, one can readily calculate changes of gene frequency (due to mutation) from one generation to the next. Thus, suppose gene A_1 mutates to A_2 with a frequency of u per generation, and that the back-mutation rate is v. Then, if the frequencies of A_1 and A_2 genes in a particular generation are p_o and q_o, the change of gene frequency in one generation is easily found, being up_o-vq_o.

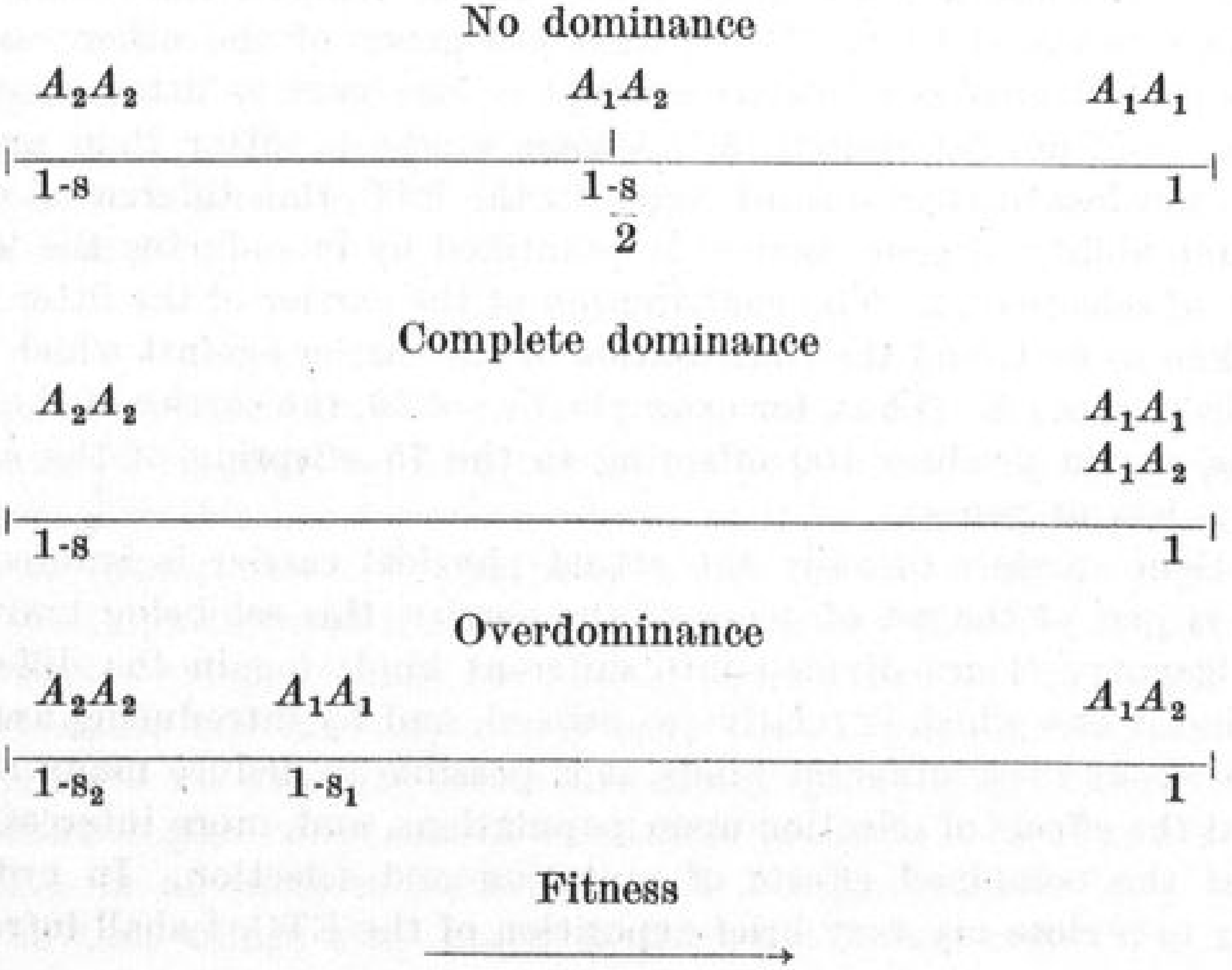
The second cause of gene-ratio-change is selection. It is argued that not all the gene carriers in a certain population will reproduce with equal success, and that thus through this differential reproductive rate, gene ratios will be altered from generation to generation. Those genes whose carriers are more successful at reproduction than other carriers, are said to be 'fitter' than the genes of the other carriers. Note that 'fitness' is a relative concept — one gene is 'fitter' than another, it is not 'absolutely fit'. A gene might be fitter than another gene, but less fit than a third gene. In the ETC, this differential reproductive ability of gene carriers is quantified by introducing the 'coefficient of selection', s. The contribution of the carrier of the fitter genes is taken to be 1, and the contribution of the carrier against which there is selection is 1-s. (Thus, for example if $s = .25$, the carrier of the fitter genes, would produce 100 offspring to the 75 offspring of the carrier of the less fit genes).

Gene carriers (usually the actual physical carrier is ignored and talk is just of the set of genes of the carrier, this set being known as the 'genotype') are divided into different kinds (again the difference in kind is one which is relative to others), and by introducing assumptions about these different kinds, it is possible to deduce many results about the effects of selection upon populations, and, more interestingly, about the combined effects of mutation and selection. In order to bring to a close my very brief exposition of the ETC, I shall introduce the major kinds of genotype and then indicate some of the important results which follow deductively from assumptions about these types.

So that the discussion can be simplified let us suppose that in a particular set of alleles there are just two members, A_1 and A_2. This means that there are three possible genotypes, A_1A_1, A_1A_2, and A_2A_2. Let us also assume that the coefficient of selection against A_2A_2 is s. Then it would seem that there are three possible situations. First, relative to A_1A_2, A_1A_1 may be completely fit and A_2A_2 might have an inbetween fitness (assume for simplicity that the coefficient of selection against A_1A_2 is ½ — it could, of course, be a greater or smaller fraction of s). In this case we say that neither A_1 nor A_2 is ‘dominant’ over each other. Secondly, A_1A_2 (relative to A_2A_2) might be completely fit and so also might be A_1A_2. In this case, where A_1A_2 behaves as if it were A_1A_1, we say that gene A_1 is ‘dominant’ over A_1, and A_2 is ‘recessive’ to A_1. (It is most important to note that at this stage we have said nothing at all about the physical characteristics of the carriers of the different genotypes — all the talk so far is at the genetic level). Finally, relative to A_2A_2 it might be that it is A_1A_2 which is completely fit, and that A_1A_1 is less fit than A_1A_2 also (although A_1A_1 may or may not be less fit than A_1A_2). Here we have a case of what is called ‘over-dominance’ where the ‘heterozygote’ (*i.e.* genotype with different alleles) is fitter than either ‘homozygote’ (*i.e.* genotype with the same alleles). Possibly the following diagram will make the three possible situations somewhat clearer.

Fig. 1.

Degrees of dominance with respect to fitness.



Now, as was said earlier, given the Hardy-Weinberg Law and the assumptions about mutation, selection, and degrees of dominance with respect to fitness, many results about gene ratio changes in populations can be deduced. Rather than to attempt to give a comprehensive survey of such results, let me mention just a couple of questions where one can give some answers, together with some numerical examples as illustration [13].

i) *How many generations are required to bring about a particular change of gene frequency?* — The answer to this question obviously depends on rates of mutation, intensity of selection and degrees of dominance. For simplicity, let us assume that we have a completely recessive lethal gene (*i.e.* a gene where the homozygote always dies without reproducing). It can be deduced that if the initial frequency of the gene is q_o and the required frequency is q_t, then if there is no mutation, the number of generations required (t) is given by the equation

$$t = \frac{1}{q_t} - \frac{1}{q_o}$$

Thus, for example, if the initial frequency is 1 in 10 and the desired frequency is 1 in 100

$$t = \frac{1}{1/100} - \frac{1}{1/10} = 90$$

Note that many more generations would be required to go from 1 in 100 to 1 in 1000, for then

$$t = \frac{1}{1/1000} - \frac{1}{1/100} = 900$$

ii) *How do mutation and selection balance each other?* — Like the answer to the last question, the answer to this question also depends on rates of mutation, intensity of selection and degrees of dominance. First assume that we have a recessive gene with frequency q, mutation rate to it u, and selection coefficient against it s. (Assume no back mutation). The equilibrium condition can be deduced as being (approximately)

$$q = \sqrt{\frac{u}{s}}$$

By way of illustration, assume that we have a mutation rate to a lethal recessive gene of 1 in 10,000. Then

$$q = \sqrt{\frac{1/10{,}000}{1}} = \frac{1}{100}$$

That is, the population will stabilize with 1 in 100 of the genes being this particular gene.
For a gene with no dominance we get (approximately)

$$q = \frac{u}{s}$$

With the same figures as above, this would yield

$$q = \frac{1/10{,}000}{1} = \frac{1}{10{,}000}$$

Finally let us consider a completely dominant gene with frequency 1-q and mutation rate to it being v. At equilibrium we have

$$q(1\text{-}q) = \frac{v}{s} \quad \text{(approximately)}$$

Assume that $v = \frac{1}{10{,}000}$ and that we have fairly severe selection against the gene, say $s = \frac{1}{2}$

$$\text{then } q(1\text{-}q) = \frac{1}{5000}$$

Since q is nearly zero, we can simplify to

$$q = \frac{1}{5000}$$

In other words, under the specified conditions, even when selection is only semi-lethal for a dominant gene, a stable population will contain only twice the percentage of the dominant gene than it would for a completely lethal gene without dominance, and the percentage of the dominant gene would be only 1/50 of the percentage of a fully lethal recessive gene.

These are obviously fairly elementary results in the ETC. More complex results can be derived from those assumptions which we have already introduced, and yet more complex results can be deduced when further assumptions, for example about rates of dispersal of populations, are introduced. However, I think that enough ground has been covered to indicate to the reader the nature of the ETC. This therefore, leaves but one more thing to do before we turn to consider Smart's claim about biological theories. So far, both the assumptions and conclusions which we have examined of the ETC talk mainly about genes and their various properties. What finally is needed is a set of hypotheses to enable us to transfer from talk about genes to talk about observable physical organisms and their various characteristics. It is clear that population geneticists do assume such hypotheses, since once having derived their results about gene frequencies, they then go on immediately to talk about physical characteristics. For example, they use hypotheses such as that mentioned earlier, namely that genes are the causes of physical characteristics, and they also use other hypotheses to the effect that mutation changes physical characteristics, as well as various hypotheses about the consequences of different degrees of dominance. These latter hypotheses include the obvious ones that a completely dominant gene makes the carrier of the heterozygote look like the homozygote of the dominant gene, and that in cases of non-dominance, the heterozygote is usually a blend of the characteristics of the homozygotes. It might be thought that these hypotheses are so obviously true that they do not really need stating — one might think that 'dominance' could mean only one thing and that therefore the hypotheses are unnecessary, or at best, analytic. However, this seems to be an over-simplified view of the situation. Logically, one could translate the genetic propositions (*i. e.* those propositions which talk only of genes and not of physical characteristics) of the ETC into different propositions about observable characteristics — these latter propositions would, no doubt, normally be false; but they would not necessarily involve inconsistent usage of terms like 'dominance'. For example, it is consistent with the genetic propositions of the ETC to say that in cases of complete dominance, the heterozygote's carrier is quite different from either homozygote's carrier. All that the genetic part requires is that the heterozygote be as fit as the homozygote for the dominant gene — their carriers could have quite different characteristics. It is clear that this possibility is not realized (or, at least, only on very rare occasions); but since it could always be realized, it becomes apparent that the translation rules are both needed and nonanalytic.

With the aid of the translation rules, biologists can use the ETC to explain and predict a wide range of phenomena. For example, the results that I gave above point to the perhaps not entirely obvious fact that if a gene is recessive and mutating at a fairly high frequency into a population, then it can maintain itself at a relatively high ratio, even though the selection against it is very severe, indeed, even if homozygotes for the gene always die before reproducing. This explains why in populations, for example human populations, certain types of genetic oddity keep reappearing, even though this oddity is fatal to its carrier. The results also show that where certain genetic characteristics are considered undesirable, it might not be that easy to eliminate them, particularly if their causes are recessive genes. For example, suppose one considered albinism in humans so undesirable that one forebad albinoes to reproduce. Their present frequency is about 1 in 20,000, and even if one ignores mutation entirely, with about 25 years to a generation, since albinism is recessive, it would take nearly 1500 years to reduce their frequency to 1 in 40,000 [14].

Rather than list explanatory successes of the ETC, let us now return to Smart's criticism of biological theories, in order to see how well the criticism fairs against the theory which we have just been discussing — the theory which, although it can stand on its own, I shall later show plays a crucial role in the overall structure of evolutionary theory. Smart's claim was that biological theories cannot take on an axiomatic form, or at least, his claim was that they cannot *legitimately* take on such a form. Does this claim seem to hold true in the case of the theory we just looked at? Shortly we shall have to go into the whole problem in somewhat more depth, but it seems clear that *prima facie* Smart's claim fails in this case — that is to say, we are here presented with a biological theory which is axiomatic in form.

In order to see that the ETC is in fact axiomatic let us remember that in an axiomatic scientific theory (*i.e.* a theory which is a hypothetico-deductive system) one finds a number of the propositions of the theory which act as premises, and from these, all the other propositions of the theory are deduced. That the ETC is a theory of this kind is clear. As we have seen, one starts with a number of propositions which are used as premises, in particular Mendel's Laws, certain assumptions about selection and mutation, other assumptions about degrees of dominance, assumptions about the links between genes and physical characteristics, and so on and so on, and then from these propositions (together with truths of mathematics and logic) all the other propositions of the theory are deduced. Thus for example, one deduces the Hardy-Weinberg Law, propositions about the time required for selection to be effective, propositions about the balance between mutation and selection, and (with the help of the translation assumptions) propositions about how long selection will take to be effective in eliminating certain physical characteristics. All in all, I would suggest that the ETC presents us with what seems to be a paradigmatic example of an axiomatic system.

Nevertheless, even though Smart could not very well deny that the ETC has at least the outward appearance of an axiomatic system, the chances still are that he would argue that the ETC is a non-genuine type of *scientific* axiomatic system. The reason why Smart would continue to think this, would be not so much because the ETC does not have the required logical structure, but because he would want to claim that biologists still lack another of the essential aspects of the theories of physics and chemistry. This aspect is one which we discussed in the first section, namely the requirement that the non-analytic propositions of physical theories be in some sense 'necessary' or 'law-like'. Smart argues that any attempt to axiomatize biology is bound to fail, since biology has (and can have) no laws of its own. He writes, « Not only do I deny the existence of emergent laws and properties, but I even deny that in biology and psychology there are laws in the strict sense at all ». (Smart, 1963, 52) Smart, of course, does not deny that there are things which biologists call 'laws'; but he does not think that these are really in any sense necessary. He believes them to be mere accidental generalizations, and hence he concludes that all biological theories must remain different from physical theories.

This charge of Smart's is extremely serious and will have to be answered; but before trying to meet the challenge, let us briefly recall the two preliminary points which I made earlier about hypothetico-deductive systems. In recalling these points we shall underline the similarity between the ETC and physical theories, and we shall also prepare the way for a refutation of Smart's arguments. The first point was that although a particular proposition may be being used as a premise in a hypothetico-deductive system, this does not necessarily mean that there is no direct evidence for the proposition — it merely means that, within the system, it is being taken as basic: the thing which is argued *from* rather than *to*. The second point was that « once a premise » does not necessarily entail « always a premise ». Sometimes, as a theory is developed, premises become derived conclusions or derived conclusions become premises.

Now, applying these two points specifically to the ETC, we find that they both hold. Although something like Mendel's First Law is used as a premise in the ETC, this does not imply that there is no direct evidence for the Law. Indeed, as we shall see, there is a great deal of evidence; but within the ETC the Law is not inferred from this evidence. Again, as far as the second point is concerned, logically it would be quite possible for evolutionists to revise the ETC, and for them to use other propositions as basic (of course, such a revised version might not necessarily get all the results that the present version can). One could, I suppose, use the Hardy-Weinberg Law as a premise. Actually, it is interesting to note that at least one earlier version of the evolutionary theory that we are discussing did not take all the premises of the ETC as basic. Instead, one of the premises was derived. In the ETC, the first claims about selection are assumed without proof — the existence of selection is given rather than proved from other premises. However, when Charles Darwin presented his version of evolutionary theory, he thought it necessary to derive his first propositions about selection, rather than assume them as premises. He wrote:

> Can it ... be thought improbable, seeing that variations useful to man have undoubtedly occurred, that other variations useful in some way to each being in the great and complex battle of life, should occur in the course of many successive generations. If such do occur, can we doubt (remembering that many more individuals are born than can possibly survive) that individuals having

> any advantage, however slight, over others, would have the best chance of surviving and of procreating their kind? On the other hand, we may feel sure that any variation in the least degree injurious would be rigidly destroyed. This preservation of favourable individual difference and variations, and the destruction of those which are injurious, I have called Natural Selection, or the Survival of the Fittest. (Darwin, 1959, 164).

Here, Darwin is clearly arguing to natural selection from propositions about variation and the fact that more are born than can survive. Moreover, study of *The Origin of Species* reveals that this latter proposition is in turn derived from other propositions [15]. However, in contemporary versions of evolutionary theory, the existence of selection seems to be introduced straight into the premises.

Now both of the similarities between the ETC and physical theories which we have just been discussing, point to the fact that there can be, and indeed there is, a great deal of evidence for the premises of the ETC. Keeping this fact in mind, let us now turn our attention to a consideration of the three features which I earlier found to be associated with physical theories, and let us begin by looking at Smart's claim about the absence of laws in biology.

a) *Does the ETC contain laws?* — In the light of the kind of evidence which we have mentioned in the last few paragraphs, that is, that a great deal of empirical support can and does exist for the premises of the ETC, does Smart's claim seem to be well taken? I would suggest that it does not, and that in fact, the non-analytic propositions which we encounter in the ETC are fundamentally similar to the non-analytic propositions of physical theories [16]. Consider, for example, Mendel's Laws. These are certainly not propositions of logic or mathematics, because logically it is quite conceivable that they be false. For example, instead of each offspring getting half of its units of inheritance from each parent (as Mendel's First Law specifies) it is logically possible that it get 3/4 of its units from the male parent and only 1/4 from the female parent. However, although they are non-analytic, these two propositions do seem to be like the non-analytic propositions of physical theories. They are general propositions, talking not just about one particular gene in one particular organism of one particular species, but rather about all the genes of all organisms of all species. Moreover, their truth seems to be more than just a matter of mere contingency. Since Mendel first formulated his Laws a 100 years ago, although his evidence was just that of a batch of pea-plants, they have been found to hold true almost universally throughout the organic world. From man to viruses, from oak-trees to sea-weed, the Laws hold. Furthermore, cytological evidence, the way for example that chromosomes multiply, seems to provide a physical interpretation for the Laws. Consequently, because the Laws have such a wide range of applications, and because the Laws seem to complement so strongly the physical activities of the cell, it seems reasonable to suppose, as biologists do in fact suppose, that even in unexamined instances, Mendel's Laws are obeyed. However, to suppose this, is to do no more than to assume that in some sense the Laws *must* hold — in other words, since the Laws are not logically necessary, it would seem that on the wide basis of discovered evidence it is justifiable to hold Mendel's Laws to be nomically necessary.

A similar conclusion seems to apply to other premises of the ETC. For example, as we saw, the assumptions which link talk of genes to talk of physical characteristics are non-analytic; but these propositions also are general (*e.g. whenever* a particular recessive gene occurs, say the gene for human albinism, it will only show its effects at the physical level when it occurs twice in the genotype). In addition, as in the case of Mendel's Laws, there is a terrifically wide range of evidence pointing to the fact that the units of inheritance and the units causing physical characteristics are the same. Indeed, there is so much evidence that it seems reasonable to assume that these links are not just a matter of chance, but that in some sense they *must* hold. Hence, in these examples I have just discussed, we can conclude that we have here propositions similar to the non-analytic propositions of physical theories. Furthermore, it seems reasonable to draw the same conclusion both both in the cases of the non-analytic propositions which are derived in the ETC, and in the cases of propositions, like those introducing selection, which are premises in the ETC but conclusions in other systems. Because of the way in which it follows from Mendel's First Law, the Hardy-Weinberg Law seems to be something which holds because it *must*, rather than something which holds just by chance. The same seems to be true for premises about selection, which Darwin justified by deriving from other propositions. These other propositions, for example, « all species of organic beings tend to increase in numbers if unchecked », are clearly law-like, and thus they pass their necessity on to the propositions about selection.

No more need be said in defence of the claim that the non-analytic propositions of the ETC are law-like. They seem to have as much right to the name 'law' as do the non-analytic propositions of physics and chemistry. In view of this, I suggest that we can end our concern with Smart. He has been shown to be wrong, because the ETC really is a genuine scientific axiomatic system made up of genuine laws. Nothing which has been covered so far can justify drawing a strong dividing line between the theories of physics and chemistry and the ETC. In view of this fact, let us conclude our examination of the ETC by going beyond the strict limits of Smart's discussion, and by seeing if the ETC shares with physical theories the other two features which I mentioned in the first section [17].

b) *Does the ETC refer to theoretical entities?* — We saw in the first section that the entities to which physical theories refer seem, roughly speaking, to be of two kinds — theoretical, like electrons and protons, and non-theoretical, like metals and gases. I pointed out that talk of the theoretical entities occurs in the higher-level propositions of the theories, and talk of the non-theoretical entities occurs in the lower-level propositions, and at some point in the theories, translation rules or bridge principles are needed to go from one kind of talk to the other kind. It is obvious that in the ETC we find an exactly similar kind of situation. The gene is certainly not an object of direct experience — one cannot see it or feel it or taste it or sense it in any other way. Even with the aid of light microscopes it is doubtful to what extent it can be said that the gene can be experienced. Perhaps strips of DNA, which is now supposed to be the carrier of genetic information, can be 'seen' with the aid of electron microscopes; but for information about all the various properties that the gene of the ETC is supposed to possess, we have to seek indirectly, inferring these properties from the macroscopical characteristics of organisms. On the other hand, although the gene, which is the major concern of the higher-level hypotheses of the ETC, is theoretical, the same cannot be said for many of the things talked about by the lower-level propositions of the ETC. Here we find talk of arms and legs, straight hair, brown hair, lack of hair, and so on — obviously all things which can be observed directly, and which are thus non-theoretical. Hence, it would seem that in the ETC we have reference to entities of both classes of object, theoretical and non-theoretical. Moreover, as my exposition of the ETC clearly showed, we have bridge principles enabling us to go from one kind of talk to the other — principles like the one which enables us to go from propositions about dominance as it was defined at the genetic level, to the effects of such dominance at the level of physical characteristics. Consequently, with respect to the theoretical-non-theoretical dichotomy, the ETC does not differ from the theories of physics and chemistry.

c) *Is the ETC self-correcting?* — The third and final feature of physical theories which I picked out as being of special interest was that although such theories act mainly to unify our knowledge and to point to new fields worth investigating, they also act as correctives to what we may previously have held, even to the extent of showing that the things which spurred the building of a theory may not be quite correct. In the development of the ETC a perfect example of this correctives aspect of theories has occurred, namely the discovery of 'genetic drift' (or, as it is sometimes called after its discoverer, the 'Sewell Wright effect'). As we have seen, one of the basic assumptions of the ETC (an assumption moreover which spurred the development of the theory) is that genetic change is brought about by selection.

Further, it is assumed that selection can affect the rate and direction in which this genetic change occurs. In the development of the theory it has been shown that under certain circumstances this assumption might be violated. (Sewell Wright, 1931) In particular, it has been possible to deduce propositions showing that in small populations the genetic change might occur in a direction which is *opposite* to the direction pointed by selection. (It is assumed, of course, that there are no other factors, like mutation, bringing about this change). The reason for this genetic drift is that apparently when selective forces are fairly low, the deviation which always occurs from absolutely random interbreeding might be sufficient to counteract completely the effects of the selective forces. Hence, it would seem that even in cases where it might be expected to, selection does not always bring about genetic change. Clearly this result is something which modified one of the basic assumptions behind the ETC, and, I suggest, bears out my contention that this theory, like the theories of physics, acts to correct as well as unify and direct [18].

At this point, I think that further argument in support of the case I have been making in this section would be redundant. It is clear that in many essential respects the ETC is no different from the theories of the physical sciences. It shares with them the same kind of structure, the same kind of propositions, refers to entities in the same kinds of classes, and functions in the same way to improve our knowledge. In view of all of this, let us therefore go on to the next topic to be discussed.

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NOTES

[1] Other than those I shall be discussing in this paper, three typical examples are Scriven (1958), Manser (1965), and Barker (1970). Many biologists also argue in this way, for example Simpson (1963), Mayr (1969), and Longuet-Higgins (1970).

[2] The pertinent references are Smart (1963), Beckner (1959), and Goudge (1961).

[3] The arguments which I shall be considering in this paper are not the only ones which have recently been made, specifically designed to show that evolutionary theory is somehow different. Both Manser and Barker argue that the theory is different mainly because neither believe that the theory is very much good. I try to tackle their objections in Ruse (1969a) and Ruse (1971a).

[4] Disagreements with the hypothetico-deductive characterisation of scientific theories can be found, for example, in Hanson (1958), Achinstein (1969), Humphreys (1968).

[5] One should, perhaps, draw a distinction between 'axiomatic' systems, and 'hypothetico-deductive' systems. The latter term is usually reserved exclusively for empirical theories, whereas the former term can also apply to mathematical and logical systems. Since, in this paper, I shall be concerned solely with empirical theories, I shall use the two terms interchangeably.

[6] The clearest discussion of laws is to be found in Nagel (1961).

[7] Again, an excellent discussion of theoretical entities and bridge principles can be found in Nagel (1961). Recently, the theoretical — non-theoretical distinction has come under heavy fire. It has been noted, with some justice, that terms like 'theoretical' and 'observable' can mean many things to different people, and might not always be mutually exclusive. For example, does one 'observe' the moon when one uses a telescope — if not, is the moon, therefore, a theoretical entity? However, even the critics of the distinction, specifically Spector (1967) and Achinstein (1969), admit that useful lines of demarcation can be drawn, so long as one specifies the exact basis of the dividing lines. Since I think

that for most people an ‘observable’ entity is one which can be seen either by the naked eye or by a light telescope or microscope, this is what I shall mean by ‘observable’ in this paper. All other entities are ‘non-observable’ or ‘theoretical’. I discuss the theoretical-non-theoretical distinction as it applies to biology in Ruse (1972).

[8] There are many books about population genetics. A good introduction is Li (1955). Much of the following exposition is based on this and Falconer (1961).

[9] I discuss the nature of the gene, in some detail, in Ruse (1971b).

[10] Both of Mendel's Laws as I have stated them have exceptions — for example, where sex chromosomes are concerned, genes are often unpaired, and more particularly, the Second Law needs drastic modification, since it does not hold for genes on the same chromosome. In this paper, because it does not affect the main thread of my argument, I shall ignore the counter-instances to the laws, remarking only that the failure to hold exactly is a property of just about every ‘law’ of physics and chemistry also. (See Scriven 1961). I consider the problem of the counter-instances to Mendel's Laws in Ruse (1970a).

[11] It must be confessed that this ‘deduction’ is not always of a rigour that would satisfy a formal logician. As in the case of the truth of Mendel's Laws, I would point out that such rigour is usually missing in the physical sciences, and again, I would refer the reader to Ruse (1970a) for a more detailed discussion of this point.

[12] Migration is a third important cause of gene-ratio-change. I shall ignore this here, since whilst its inclusion would not affect the points I want to make, such an inclusion would complicate the discussion considerably.

[13] I shall be giving some more results of population genetics later in the paper.

[14] This result and others can be found in the early chapters of Falconer (1960).

[15] In Ruse (1971a), I give a detailed analysis of Darwin's argument, trying to show its hypothetico-deductive nature.

[16] The ground covered in the next few paragraphs is tackled in much greater depth in Ruse (1970a).

[17] In leaving Smart, it is interesting to speculate on the reason why he has so mistaken a view of biology. I suspect the real reason is that his knowledge of biology begins and ends with the work of J. H. Woodger. Smart refers to Woodger in his attack on the possibility of axiomatizing biological theories, remarking that Woodger's efforts have a « very odd look » (Smart, 1963, t0). Did Smart but know it, most biologists would agree.

[18] In fairness, it must be admitted that whilst some evolutionists have eagerly embraced the notion of genetic drift and ascribe to it a fairly significant evolutionary role (*e.g.* Huxley, 1942, and Carter, 1950), others have denied its existence, or at least, have denied its evolutionary significance (*e.g.* Fisher and Ford, 1948). The general consensus seems to be that drift does exist, that it does play a role in evolution, but that its importance is not quite as overwhelming as some early devotees claimed (see Dobzhansky, 1951, Simpson, 1953, and Smith, 1958).

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MEMORIA, MORFOGENESI E COMPORTAMENTO

La moderna letteratura scientifica ha portato all'impiego del vocabolo « memoria » anche fuori della sfera psicologica: si parla così, ad esempio, di memoria genetica, in certi contesti, ed in altri di memoria di un calcolatore elettronico. L'autore saggia la possibilità e l'opportunità di una trattazione formale unitaria di tutti questi comportamenti e della struttura dei sistemi fisici che li supportano. Suggerisce pertanto di considerare la memoria come la traccia di uno stato passato di un sistema, traccia persistente in esso e condizionante il suo comportamento successivo. Ne deriva una visione storica che, nell'ambito della fenomenologia biologica, mostra il comportamento come immediato prolungamento della morfogenesi ed agente morfogenetico esso stesso, a misura della traccia che può lasciare nell'organismo in azione. Questa impostazione porta ad abbandonare molta vecchia, farraginosa terminologia degli psicologi e degli embriologi. Negli ultimi paragrafi vengono discussi: le diverse strategie seguite, da gruppi di esseri viventi diversamente organizzati, per canalizzare l'informazione necessaria allo sviluppo ed al comportamento; le caratteristiche strutturali e funzionali dei subsistemi sede di memorie specializzate ed infine le prospettive aperte a questo tipo di analisi.

1. Vi sono molte specie di fenomeni naturali la cui interpretazione e comprensione è stata ritardata notevolmente dal fatto che gli studiosi non si trovavano d'accordo sulle rispettive competenze in proposito; così che questi fenomeni sono rimasti troppo a lungo accantonati in una sorta di « terra di nessuno », per esserne al più tolti, di tanto in tanto, solo per essere affrontati con i mezzi meno idonei.

Questo è il caso, ad esempio, della *memoria*.

Il contributo dato dalla psicologia filosofica all'interpretazione di questo fenomeno è facilmente riassumibile in pochi cenni: si pensi all'associazionismo, si pensi a Bergson[1].

Quanto alla psicologia sperimentale, è sufficientemente noto che cosa essa ci abbia insegnato, ma è noto anche come difficilmente ci si possa accontentare delle classiche e pur, sotto certi aspetti, ammirevoli esperienze di Ebbinghaus[2] e di quant'altre sono state di seguito eseguite nella stessa direzione.

Un grande passo in avanti è stato compiuto quando nuove istanze, tra cui soprattutto la realizzazione di dispositivi tecnici di memoria, hanno fatto capire che, tradizionalmente, ci si era occupati solo di ciò che possiamo convenire di chiamare memoria psicologica, e non di memoria in genere.

Di memoria possiamo infatti parlare anche in altri contesti; l'unica precauzione da prendere, in tali casi, sarà quella di ricordare che stiamo parlando non di memoria psicologica, ma di memoria *tout court*, in un'accezione che *include* l'uso che del termine si fa in psicologia.

[1] In Aristotele troviamo passi degni della massima attenzione, in cui sono tra l'altro sottolineate sia la natura dinamica del processo di memorizzazione, e soprattutto del ricordo, sia la necessità di un substrato materiale per la memoria (*De Mem., passim*). Di H. BERGSON va citato il saggio *Matière et mémoire*, Paris, 1896.

[2] EBBINGHAUS, H. (1885): *Über das Gedächtnis*. Si veda poi, tra i contemporanei, B. J. UNDERWOOD (di cui ad es., *Forgetting* in Sci.Amer. 210 : 91-99 (1964)).

2. Un ragionamento simile a questo dovette fare Rignano[1] quando, mezzo secolo fa, prese a parlare di memoria a proposito dell'ereditarietà dei caratteri, prendendo alcuni fenomeni della psicologia a *modello* di fenomeni della genetica e delle scienze dello sviluppo.

Ai nostri giorni i biologi hanno meno riluttanza di un tempo a parlare in termini di modelli ed alcuni sono giunti anzi a dare, alla nascente modellistica, un po' di disciplina logica e di consapevolezza epistemologica insieme (si veda ad esempio Stahl, 1967)[2].

Oggi possiamo dire, con espressione pulita ma forse un po' pedante, che il punto di vista di Rignano era giustificato dall'effettiva esistenza, tra le due classi di fenomeni ch'egli considerava, di una corrispondenza tale da rispettare l'invarianza per determinate proprietà: memoria psicologica e memoria ereditaria (genetica) hanno cioè delle proprietà formali (strutturali) comuni, e ciò giustifica il prendere l'una a modello dell'altra.

3. Si può essere tentati, a questo punto, di veder come si possa caratterizzare meglio simile invarianza; nelle pagine che seguono cercherò di mostrare che questa caratterizzazione è possibile e, cosa non meno importante, fruttuosa.

Per ogni discorso - è meglio precisare subito - possiamo adoperare forme differenti di linguaggio, ma assai generalmente ve n'è una che risulta, nel contesto contingente, più espressiva delle altre (fors'anche perchè chi la usa ne è maggiormente padrone): per queste considerazioni sulla memoria, svolte ad un livello insieme funzionale e formale, ritengo opportuno servirmi del linguaggio della cibernetica ed in particolare di quello della teoria dell'informazione.

I problemi relativi alla memoria possono infatti riguardarsi, come dirò più avanti, come problemi di *inscrizione* di informazioni in un determinato substrato materiale e di *lettura* delle informazioni precedentemente inscritte.

Qui ci conviene comunque lasciare momentaneamente sospeso il discorso generale e le considerazioni semantiche per delineare schematicamente un paio di situazioni concrete.

Il primo esempio è attinto alla sfera del comportamento, il secondo a quella della morfogenesi.

4. Supponiamo di chiedere ad un passante l'itinerario che porta da un punto ad un altro di una città. Il nostro interlocutore potrà dirci come dobbiamo comportarci ad ogni incrocio oppure potrà suggerirci di seguire la linea di un certo tram (fig. 1a-b).

Nel primo caso abbiamo ricevuto istruzioni relative alle scelte da operare ad ogni successiva alternativa, nel secondo un'unica istruzione che rimanda ad una regolarità ambientale da seguire.

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[1] RIGNANO, E. (1922): *La memoria biologica*, Bologna, Zanichelli.

[2] STAHL, W. R. (1967): *The role of models in theoretical biology*, in *Progress in theoretical biology* (Snell, F.M. ed.), I : 165-218, Acad. Press.

Questa seconda istruzione si potrebbe anche definire un *criterio di utilizzazione* per un'informazione che verrà successivamente ricevuta *dall'esterno*: la linea del tram è stata infatti stabilita operando ad ogni incrocio la stessa identica scelta che deve operare, nel suo cammino, chi deve seguire quel percorso.

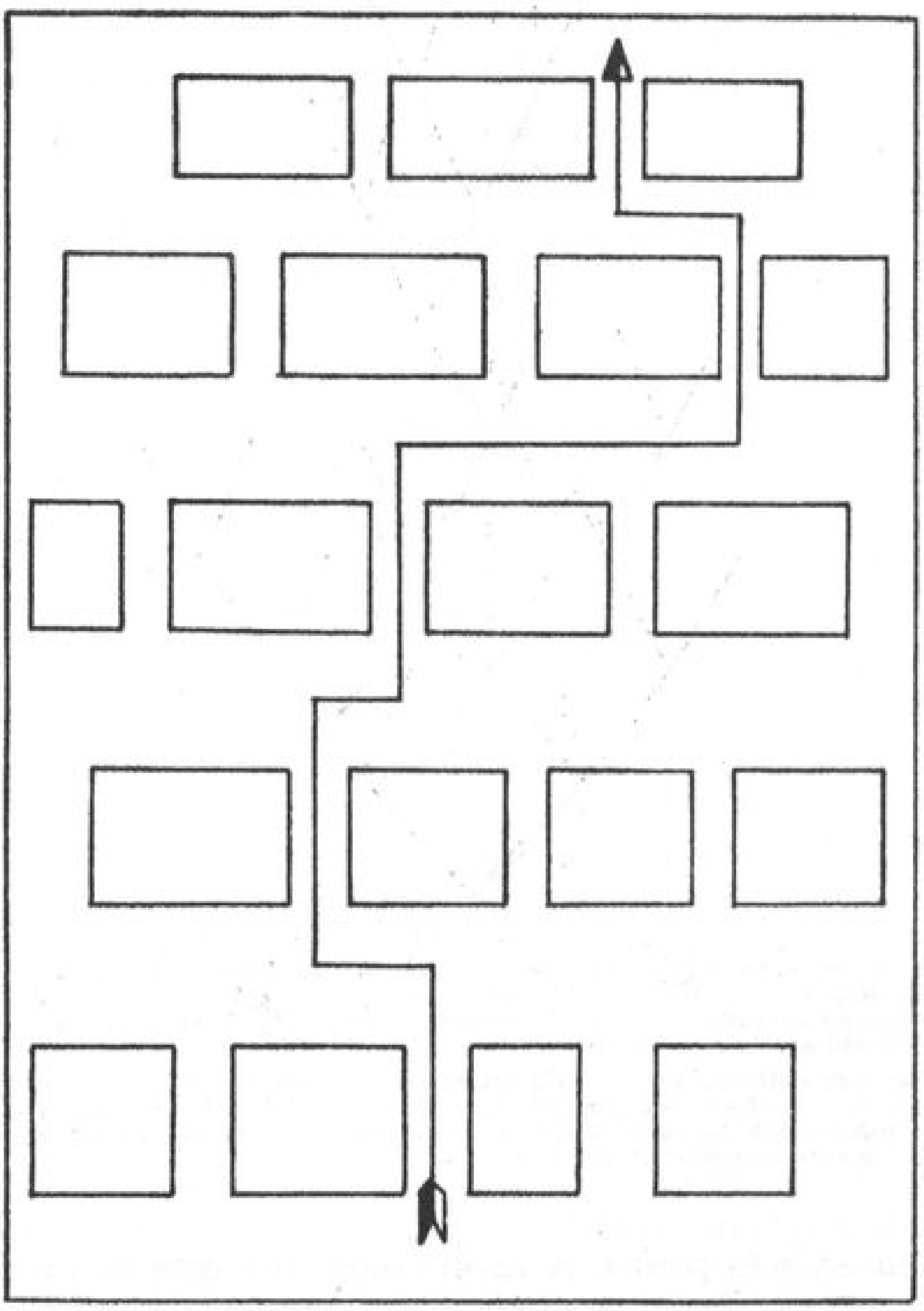

Fig. 1a

Ora, chi dispone di un'istruzione particolareggiata si pone, ad ogni incrocio, una domanda del tipo: - devo andare a destra o a sinistra?[1] - e la risposta è data dalle istruzioni ordinatamente inscritte in memoria; chi ha ricevuto, invece, un'istruzione del secondo tipo, trasforma la medesima domanda in una del tipo seguente: - da che parte va il tram? a destra o a sinistra? - e la risposta è data, in questo caso, dall'osservazione, cioè da informazione assunta estemporaneamente dal mondo esterno.

Questo esempio chiarisce uno dei punti focali della questione:

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[1] **Per usare alternative binarie.**

infatti, nella situazione in cui si disponga di un'ordinata sequenza di informazioni particolareggiate è possibile senz'altro operare le scelte che ci si presentano; nel caso alternativo, invece, ciò non sarà possibile senza l'ulteriore assunzione di informazione esterna.

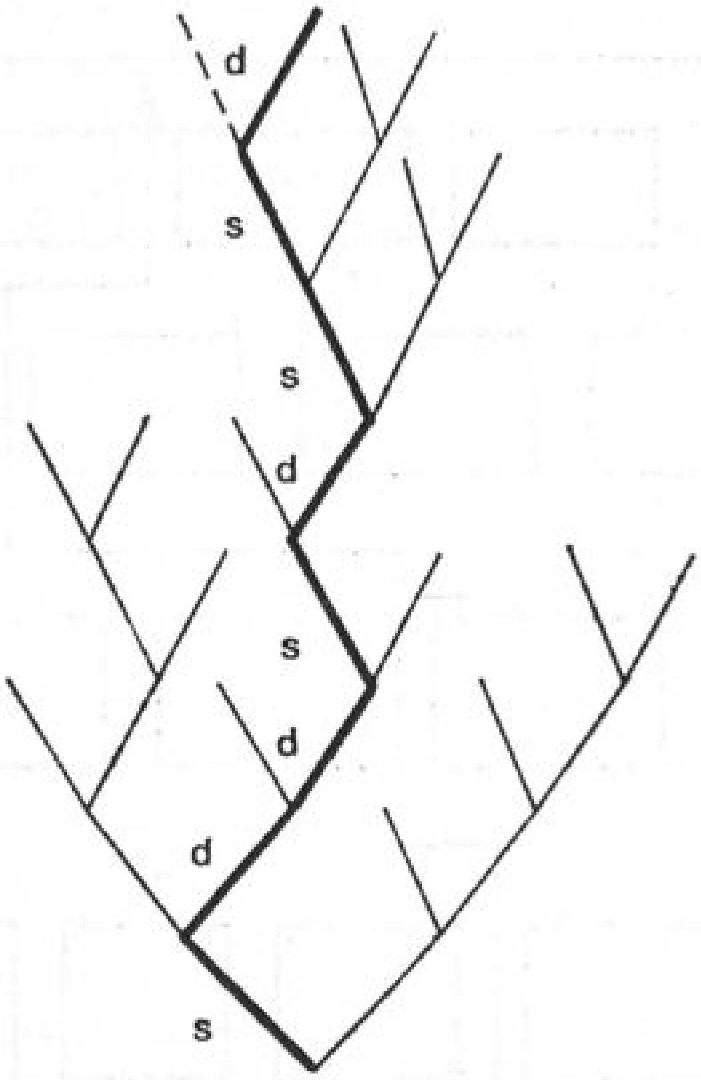

Fig. 1b

Nel percorrere l'itinerario indicato in (a) dalla freccia, incontriamo 8 bivi successivi, in corrispondenza dei quali dobbiamo scegliere tra l'andare a sinistra (S) o a destra (D) per poter complessivamente discriminare tra tutti gli itinerari possibili. Ciò equivale a compiere, in un albero di scelte binarie (b), le scelte indicate in grassetto.

In following the route indicated in (a) by the arrow, we encounter 8 successive turnings, corresponding to which we have to choose between turning left (S) or right (D) in order to make an overall discrimination between all the possible routes. This amounts to making, in a sequence of binary choices (b), the choices indicated in heavy type.

Ciò non esclude però che, in un adeguato sistema di riferimento, l'istruzione di tipo globale abbia un contenuto d'informazione non nullo. Essa permette infatti di discriminare, nell'afferenza sensoriale, tra tutte le possibili fonti esterne di informazione, quella avente valore operativo; e ciò corrisponde al montaggio di un sistema atto a selezionare (ed eventualmente a ricercare e ad amplificare) gli stimoli in arrivo.

La natura di un'istruzione « globale », che rimanda ad una regolarità esterna da utilizzare, è perciò *morfogenetica s.l.* in quanto ha come risultato l'allestimento o la rielaborazione di una struttura.

5. Ci si può allora domandare se nei casi in cui vien data un'istruzione « occasionale » esista o meno, nel sistema, qualcosa di equivalente all'organizzazione strutturale e funzionale realizzata nell'altro caso mediante quel che ho definito un « criterio di utilizzazione ».

C'è da pensare di sì: un segnale in arrivo ha diverso valore, per diversi organismi, in relazione alla loro identità specifica ed alla loro storia individuale, un valore correlato, cioè, alla loro *struttura* (od organizzazione) nel momento considerato.

È evidente, ad esempio, come molti segnali in arrivo dall'ambiente siano diversamente utilizzati da un uomo o da un insetto, da un animale affamato o da un animale sazio, da un individuo sessualmente maturo o da un individuo sessualmente immaturo.

E rilevare il valore funzionale della struttura di un sistema equivale a dire che in questo esistono, in un certo senso, dei circuiti stabiliti, dei criteri selettivi già predisposti, realizzati in parte mediante i processi morfogenetici *s.str.* in parte secondo quanto appreso in precedenza.

Queste considerazioni ci fanno spostare il discorso dalla sfera del comportamento a quella della morfogenesi.

6. Gli embriologi parlano frequentemente del « valore prospettico » di una porzione di uovo o di embrione o dei « territori presuntivi » da cui ci si attende deriveranno determinati organi; essi ci descrivono poi vari meccanismi di controllo sullo sviluppo, meccanismi dei quali una serie di classiche esperienze, condotte soprattutto sugli anfibi o sul riccio di mare, ci illustra le proprietà.

Ma anche qui, similmente a quanto dicevo a proposito delle consuete ricerche psicologiche sulla memoria, l'impostazione tradizionale dell'indagine è in parte insoddisfacente e viziata da una terminologia vaga e legata a deboli e contingenti filosofie biologiche: proviamo a sostituirla con un'altra, più chiara, anche questa volta presa a prestito dalla teoria dell'informazione.

Possiamo infatti descrivere un processo morfogenetico, così come qualsiasi altro comportamento, come l'attuazione di un *programma*, e questo, in fondo, non è altro che una sequenza di informazioni memorizzate. Nei diversi casi, nel sistema potrà esistere o meno un dispositivo di *verifica* del grado di attuazione del programma, dispositivo capace di intervenire, mediante un *feed-back*, facendo riprendere il comportamento da un punto conveniente del suo antecedente decorso (si pensi ai fenomeni di rigenerazione, v. fig. 2).

Per svolgere le nostre considerazioni ci conviene partire da un fatto assai comune nell'embriogenesi, cioè dal diverso destino di due cellule, o comunque di due porzioni di uno stesso embrione, che differiscano ad un dato istante per il solo fatto di occupare due posizioni diverse nell'architettura dell'embrione.

È quanto è stato possibile mettere in rilievo mediante accurate ed intelligenti manipolazioni di blastomeri e di mezzi embrioni a diversi stadi di sviluppo[1].

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[1] Per una parziale sintesi di quanto è stato compiuto in proposito su due differenti gruppi zoologici si vedano, ad esempio: HARRISON, R. G. (1969): *Organization and development of the embryo*, Yale University Press. — HÖRSTADIUS, S. (1939): *The mechanics of sea-urchin development, studied by operative methods*, Biol. Rev., 14 : 132-79.

È quanto possiamo descrivere mediante il concetto, felicemente illustrato da Wolpert[1], di *informazione posizionale*.

Ma noi possiamo compiere le nostre indagini su materiale appartenente a specie differenti o a stadi differenti di sviluppo o ad organi e territori embrionali, infine, differenti; e l'embriologia causale ci insegna che il destino della porzione di embrione che consideriamo dipenderà, in certi casi, più dalla sua posizione *attuale* che da quella occupata in un istante *precedente*, mentre in altri casi sarà vero il contrario.

A questo punto non dovrebbe costarci molto riparlare in termini di memoria e dire che, in determinati embrioni ed in determinati stadi di sviluppo, si conserva una memoria dell'informazione posizionale precedente, mentre in altri contesti questa viene annullata dall'informazione posizionale sopraggiunta: e ciò equivale a dire che si è avuta una parziale cancellazione della memoria stessa.

Che cosa può significare, dunque, in questa accezione, *memoria*?

Memoria viene ad essere semplicemente *la traccia di uno stato precedente* del sistema, traccia che persiste in esso e ne condiziona il comportamento successivo.

7. Una definizione di questo tipo risulta soddisfacente, a guardar bene, anche nel caso della memoria di cui si occupano gli studiosi di psicologia e di etologia.

Sia nei fatti di apprendimento che in quelli morfogenetici *s.str.*, infatti, possiamo abbastanza agevolmente far nostra una visione *storica* in cui gli eventi (*transizioni* tra stati differenti del sistema, *esperienze* dell'individuo) condizionano il comportamento del sistema in un istante successivo a misura appunto della traccia che essi lasciano dietro di sè; e questa traccia la chiameremo senz'altro *memoria inscritta*.

Non è peraltro inutile distinguere due diversi sistemi di riferimento che dànno un diverso significato ad una traccia mnemica: memoria è, da un lato, informazione immagazzinata in modo funzionale, utilizzabile nell'ambito del sistema (individuo) in cui è inscritta; dall'altro essa è documento, per lo storico che osserva dall'esterno, di un evento occorso: certi fatti, come ad esempio una cancellatura, un'omissione, hanno ben diverso significato nei due contesti.

Definizioni di questa sorta prescindono, comunque, da un gran numero di determinazioni, soprattutto da quelle relative alla natura fisica del sistema capace di memoria. Ma ciò, si badi, non è povertà descrittiva, bensì atteggiamento che invita a non circoscrivere *a priori* la nostra attenzione a determinate classi di oggetti che si ritengono acriticamente l'unica possibile sede delle proprietà che studiamo.

Ciò che deve essere comunque dato per acquisito è l'impossibilità di concepire una memoria slegata da ogni substrato materiale.

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[1] WOLPERT, L. (1969): *Positional information and the spatial pattern of cellular differentiation*, J.Theor.Biol.: 25 : 1-48.

8. Naturalmente, la natura fisica di un dispositivo di memoria non è indifferente. Se le voci di un dizionario fossero incise su un nastro magnetico, anzichè essere stampate sulle pagine di un volume, il tempo medio di reperimento di una di esse sarebbe notevolmente diverso; e sarebbe ancora diverso a seconda che noi usassimo un unico nastro o più nastri distinti (ed ordinati).

Si apre qui un vasto ed importante discorso sugli aspetti topologici e su quelli dimensionali dei dispositivi di memoria, discorso che ci porterebbe lontano e che, del resto, sotto molti aspetti, è ancora tutto da fare.

L'unico aspetto che, a mio avviso, vale la pena di illustrare in questa sede è quello relativo alla possibile separazione, nell'ambito di un sistema, di un compartimento riservato alla memoria di informazioni che è particolarmente importante sottrarre ad una cancellazione accidentale e che è insieme necessario poter riutilizzare in determinati momenti successivi: si pensi al DNA di una cellula, si pensi ai libri, si pensi ai nastri magnetici......

Tutto il sistema è evidentemente sede di memoria, nel senso sopra illustrato, ma si tratta di memoria per cui non esistono specifici problemi di conservazione e di regolare riutilizzazione: ogni volta che disponiamo invece di qualche schema riproducibile, suscettibile di applicazioni ulteriori, ci conviene affidarlo ad un compartimento più conservativo, ad un subsistema specializzato, appunto, per una memoria più duratura ed organizzata.

Non affronterò in questa sede il problema dell'ubicazione di questa memoria: è certo comunque che attualmente ne sappiamo abbastanza per quanto concerne la memoria genetica[1], ma molto meno per quanto si riferisce alla memoria di cui parla lo psicologo[2].

Quel che più mi interessa, in questa sede, è sottolineare come abbia tranquillamente senso parlare della memoria di una molecola o di un qualsiasi dispositivo meccanico o elettrico multi- o anche solo bistabile: si pensi ad un flip-flop, il cui comportamento dipende da due semplici fattori: lo stato precedente (condizionante in quanto memorizzato) e l'arrivo (diciamo dall'ambiente esterno) di un impulso di corrente. Un dispositivo siffatto è in qualche misura l'equivalente elettrico del *dispositivo elementare binario di memoria*[3].

9. Questa schematizzazione ci rende agevole trattare della memoria con il linguaggio della teoria dell'informazione anche sotto il profilo quantitativo. Con tutti i limiti di ogni stima dell'informazione,

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[1] In questa nota ispirata ai concetti della teoria dell'informazione è doveroso accennare ai dispositivi di regolazione della lettura (erogazione) dell'informazione genetica (operone di Jacob e Monod, etc.).

[2] E ciò si deve ammettere nonostante i dati sperimentali raccolti in questi anni a riguardo del ruolo di varie molecole (RNA, polipeptidi) nei processi di memoria. Si veda in proposito il bel volume *Biology of memory* (edd. K. H. PRIBRAM e D. E. BROADBENT, Acad. Press, 1970).

[3] Non constavano forse di sistemi di flip-flop le memorie di certi vecchi calcolatori?

un calcolo è possibile anche a proposito di informazioni memorizzate.

Qui è però necessario ricordare due diversi punti di vista in cui ci possiamo porre nell'eseguire misure di informazione: possiamo occuparci infatti del *contenuto* di informazione di un sistema oppure delle caratteristiche quantitative della *trasmissione* di informazione da un sistema ad un altro.

Qualcosa di analogo è possibile discriminare in materia di memoria, distinguendo tre momenti differenti del processo mnemico.

Abbiamo infatti il *farsi* della memoria, l'inscriversi cioè, nella struttura dell'organismo, delle tracce dei suoi stati successivi.

Possiamo occuparci poi della *struttura* di un sistema (poniamo di un organismo) in quanto già realizzata ed articolata, come risultante di tutta la sua storia antecedente.

Possiamo infine vedere in che misura la struttura posseduta da un organismo in un dato istante ne condiziona il *comportamento*, qual'è, in altri termini, l'incidenza della traccia memorizzata degli stati antecedenti sul comportamento attuale del sistema in istudio.

Questi tre momenti possono chiamarsi il primo scrittura ed il terzo lettura di un dato informativo, mentre il secondo rappresenta una sezione istantanea del processo dinamico di memorizzazione.

10. Impostando così la problematica, è agevole cogliere una zona d'incontro tra discipline tradizionalmente tanto lontane ed apparentemente tanto diverse come quelle dello sviluppo e quelle del comportamento.

Esse possono infatti apparire come la visione di uno stesso oggetto in due diverse dimensioni: la morfogenesi è storia, la storia degli stati successivi del sistema; il comportamento è cronaca, la cronaca in cui si prolunga una storia che si estende fino ad un istante fa, cronaca che, dialetticamente mediata con i suoi precedenti, si farà essa stessa storia tra un istante.

Salvi i casi limite, tuttavia, è praticamente vantaggioso considerare separatamente i due punti di vista, e ciò anche perchè in etologia ed in morfogenetica diversi sono i sistemi di riferimento che rendono possibili stime quantitative dell'informazione in gioco; stime, queste, attraverso le quali si possono identificare problemi di ottimizzazione risolvibili mediante adeguate strategie.

11. Senonchè, in certi casi è veramente difficile stabilire dove finisca l'aspetto morfogenetico del fenomeno e dove inizi invece quello etologico; le due categorie perdono anzi ogni significato in comportamenti quale l'aggregazione degli Acrasini.

In questi organismi, la morfogenesi del corpo fruttifero è l'espressione ultima di un movimento orientato di una popolazione di amebe in risposta ad uno stimolo chimico (figg. 3 e 4).

Il fenomeno, se da un lato ci ricorda i « riconoscimenti di superficie » che si hanno tra cellula e cellula, non è peraltro diverso dalla formazione di una fila di formiche per azione di un feromone. In entrambi i casi si assiste alla realizzazione di una struttura più complessa per l'operare di una semplice legge compositiva su elementi più semplici, dal punto di vista dei quali il fenomeno appare come un movimento orientato in risposta ad una regolarità ambientale di natura chimica.

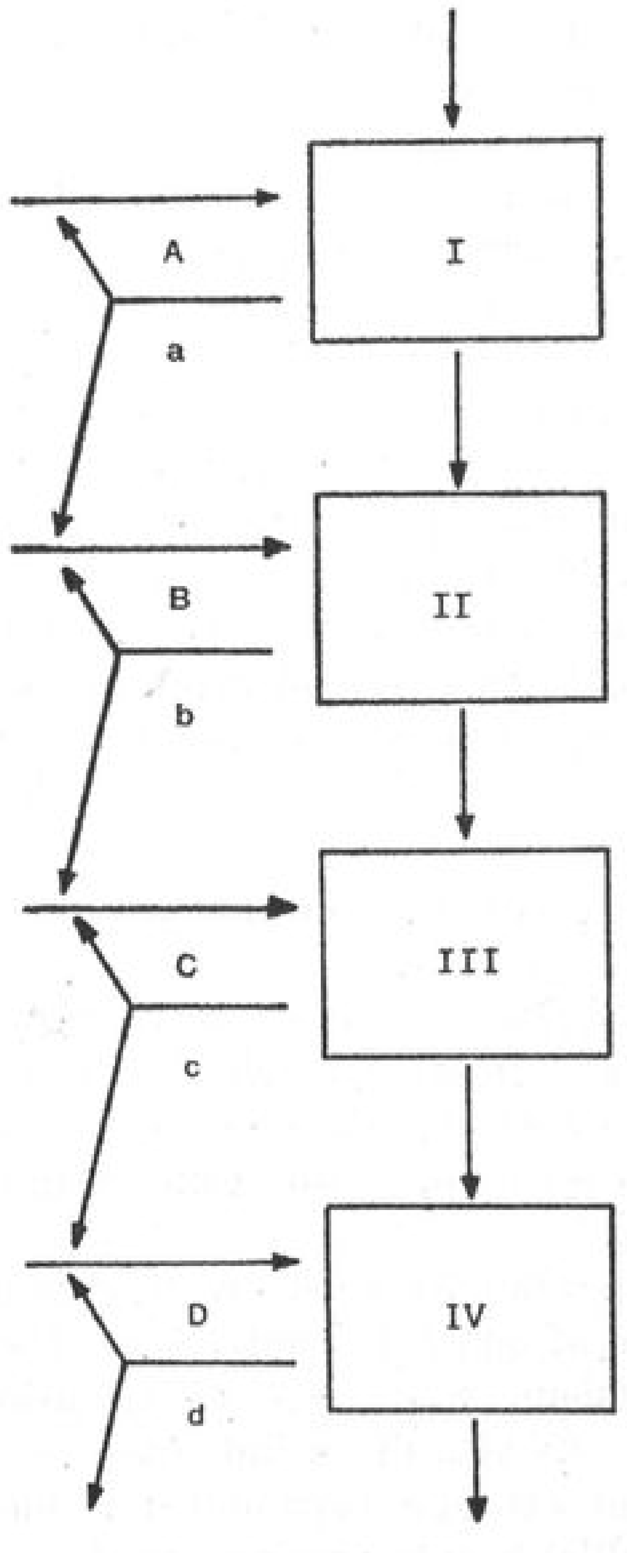

Fig. 2

Le celle (I....IV) rappresentano stati successivi del sistema; le frecce in entrata (A....D) l'informazione necessaria per il passaggio da uno stato al seguente; le frecce in uscita rappresentano (a....d) i dispositivi che verificano l'attuazione del programma, decidendo se farne proseguire il decorso o determinare la ripresa del funzionamento a partire da un'istruzione precedente.

The cells (I....IV) represent successive states of the system; the incoming arrows (A....D), the information necessary for the passage from one state to the following one; the outgoing arrows (a....d), the devices for checking how the program is carried out, by deciding whether to make it continue its course or to cause it to resume its functioning, starting from a previous instruction.

Movimenti morfogenetici avvengono, d'altro canto, anche nell'ontogenesi di organismi superiori: basterà citare la migrazione delle cellule della cresta neurale (Weston, 1970)[1] e quella di cellule del telencefalo dei Tetrapodi (Clairambault e Derer, 1969)[2].

12. Riprendendo comunque, più in generale, il discorso sulla memoria, cercherò di chiarire in quale senso si possa dire che ogni sistema (che può essere per noi un organismo, un individuo) è quale lo ha fatto la sua storia o - si può anche dire - è la sua storia stessa.

In realtà, la storia che sta alle spalle di un essere vivente ha due dimensioni, l'una individuale, l'altra abbracciante invece l'insieme delle generazioni antecedenti.

La memoria che un individuo porta con sè, pertanto, è insieme memoria della specie e memoria individuale, l'una innestandosi sulla prima come suo necessario prolungamento.

Ed il decorso delle esperienze della specie è strettamente parallelo a quello delle esperienze dell'individuo: le prime sorgono con le mutazioni, attraversano un processo selettivo ed entrano - se favorevoli - nella « memoria della specie », cioè nella memoria genetica; le seconde sorgono dalla contingenza della vita individuale ed entrano, più o meno stabilmente, nella memoria dell'individuo.

Molto più in là il parallelismo non può venire spinto, tuttavia, essendo diverso il substrato (popolazioni, subunità (ad es. cellule, molecole) di un individuo) su cui i due processi selettivi rispettivamente agiscono.

13. Memoria è comunque incanalamento del comportamento, restrizione di gradi di libertà.

Questa considerazione, che nasce dallo studio del comportamento individuale, ci invita a riflettere anche su alcuni problemi evolutivi.

Quanto abbiamo memorizzato durante la nostra vita individuale, ci si può chiedere ad esempio, è opportuno trasmetterlo tutto ai nostri discendenti?

Questo è in fondo il problema dell'ereditarietà dei caratteri acquisiti, indebitamente legato al nome di Lamarck (v. Omodeo, 1960)[3].

Una breve riflessione ci convince, al riguardo, che molte determinazioni contingenti della vita di un individuo sono irrilevanti per ogni altro individuo, sono « troppo personali »; il buon educatore non si limita a riproporre all'allievo la propria esperienza, ma cerca di sviluppare in lui un'autonoma capacità di giudizio e di scelta tra le alternative che gli si presenteranno.

Che non vi sia prova dell'ereditarietà dei caratteri acquisiti, pertanto, è un fatto che non deve sorprenderci, malgrado i vantaggi contingenti che si possono ipotizzare. Sappiamo d'altra parte che presso animali sociali come l'uomo avviene una trasmissione extragenetica, *culturale*, di patterns sperimentati ed acquisiti nelle generazioni antecedenti.

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[1] WESTON, G. A. (1970): *The migration and differentiation of neural crest cells*, Adv. Morphogen., VIII : 41-114.

[2] CLAIRAMBAULT, P. et DERER, P. (1969): *Les migrations cellulaires au cours du développement du télencéphale des Tétrapodes*, Année Biol., VIII : 629-656.

[3] OMODEO, P. (1960): *Darwin e l'ereditarietà dei caratteri acquisiti*, « Scientia », LIV : 22-31.

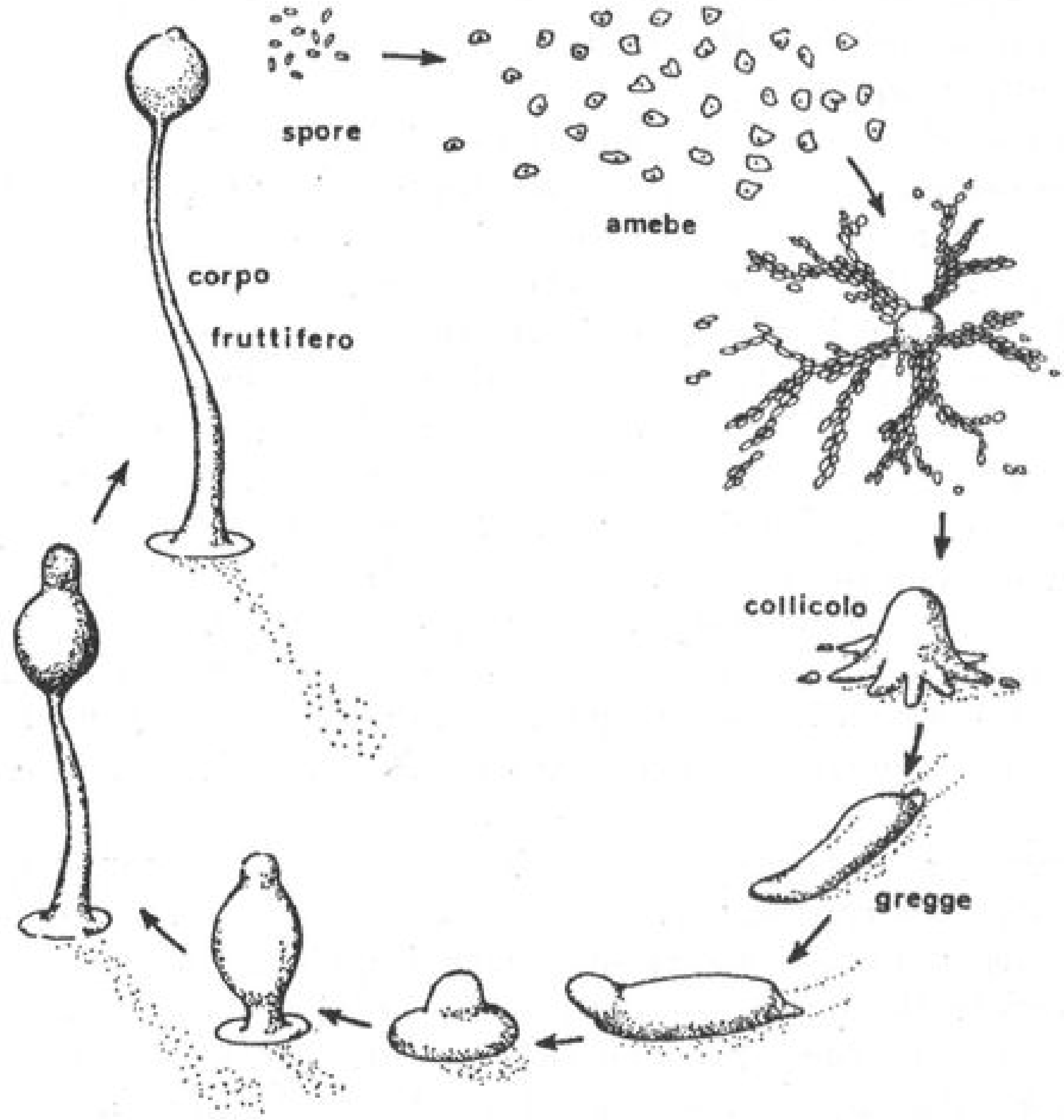

Fig. 3

Ciclo biologico dell'Acrasino Dictyostelium discoideum. *Le amebe (in alto a destra) confluiscono attratte chimicamente da una cellula fondatrice; formato il « collicolo », si mettono in moto in forma di « gregge » che, dopo un certo tempo, si arresta formando il corpo fruttifero. Le spore che si differenziano su questo riaprono il ciclo (sec. vari A.A., da Omodeo 1970).*

The biological cycle of the Acrasine Dyctyostelium discoideum. *The amoebas (upper right) flow together, chemically attracted by a founder cell; having formed the « colliculus », they start to move in the form of a « flock » which, after a certain time, stops and forms the fruit-body. The spores that are differentiated on the latter reopen the cycle (acc. various authors, from Omodeo,* 1970).

Ma la filogenesi - ed è questa una nuova importante questione - non è tanto storia di individui quanto storia di « modelli », di norme di reazione: le contingenze della vita dell'individuo non sono rilevanti, riproponendosi *ex-novo* ad ogni generazione.

Dobbiamo pertanto interpretare con prudenza le serie ortogenetiche classiche (quali quella dei cavalli, quella dei titanoteri o quella degli elefanti) ed i parallelismi e le convergenze evolutive: a volte, infatti, potremo trovarci di fronte ad autentiche « scelte obbligate », altre volte non avremo di fronte altro che scontate *regolarità a posteriori*.

14. Un altro aspetto generale da considerare, sotto il profilo filogenetico, riguarda la canalizzazione dell'informazione necessaria allo sviluppo ed al comportamento, problema che, dal punto di vista embriologico e fenogenetico, fu molto dibattuto da Waddington[1].

In organismi di diverso livello evolutivo (Procarioti, Metazoi, Primati) possiamo infatti notare diverse strategie di memorizzazione di tale informazione.

I due schemi fondamentali sono riconducibili alle alternative mostrate nell'esempio del pedone che chiede la via: si può dare, infatti, una memorizzazione puntuale delle informazioni necessarie, oppure un criterio di utilizzazione che diverrà operativo a misura dell'integrazione che verrà dall'informazione attingibile all'esterno.

Una memorizzazione del primo tipo associa un ovvio vantaggio ad un ovvio limite: essa è cioè più sicura, in quanto la disponibilità dei dati memorizzati dipende solo dall'integrità dell'organismo che la possiede e che la può utilizzare; non può d'altro canto superare limiti dimensionali assegnati, sia perchè gli *elementi conservativi* di memoria sono in certa misura compartimentati e rappresentano perciò solo una porzione limitata del sistema, sia perchè, oltre certi valori numerici delle unità di memoria, sorgono insormontabili problemi di utilizzazione e di ottimizzazione, secondo quanto ha dimostrato Bremermann (1967)[2].

Questo problema non esiste più nel caso della memorizzazione di «norme di reazione»; in tal caso non è prevista puntualmente l'attuazione di un comportamento, poichè questo, tracciato a grandi linee, verrà specificato dalle concrete determinazioni del contesto esterno.

L'arrivo, da questo, di una sufficiente informazione esterna integrativa, è certamente più o meno aleatorio, ma si tratta di un rischio che gli organismi hanno dovuto e saputo accettare per realizzare le strutture e esprimere i comportamenti più sofisticati che incontriamo, ad esempio, in un Vertebrato. Il pattern esterno da acquisire dovrà comunque esistere ad una scala dimensionale adeguata alle capacità sensoriali dell'organismo[3].

Tra i più clamorosi esempi dell'importanza dei processi morfogenetici di questo tipo, mi limito qui solo a ricordare come l'architettura del SNC di un Vertebrato superiore sia tracciata geneticamente solo per grandi linee (v. ancora Bremermann 1967) e come il regolare montaggio di questo sistema sia condizionato dalle esperienze, soprattutto quelle precocissime, dell'individuo (si pensi all'*imprinting* ed alle classiche ricerche condotte in proposito da Conrad Lorenz).

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[1] Mi limito qui a citare due noti libri di C. H. WADDINGTON: *The strategy of the genes* (Allen & Unwin, London, 1957) e *New patterns in Genetics and Development* (Columbia University Press, New York & London, 1962).

[2] BREMERMANN, H. (1967) : *Quantitative aspects of goal-seeking self-organizing systems*, in *Progress in theoretical biology* (Snell, F.M. ed.), I : 59-77.

[3] La presenza e l'attività di altri organismi possono naturalmente fornire molte regolarità utilizzabili in differenti contesti: cosa che era negata, evidentemente, ai primi organismi comparsi sulla Terra.

Le due diverse canalizzazioni della memoria necessaria alla morfogenesi ed al comportamento hanno anche, evidentemente, un diverso costo, sia in termini di dimensioni relative del *compartimento-memoria*, sia in termini energetici o materiali: biomassa o numero di individui eliminati attraverso processi selettivi, perchè « realizzati male o non a tempo ».

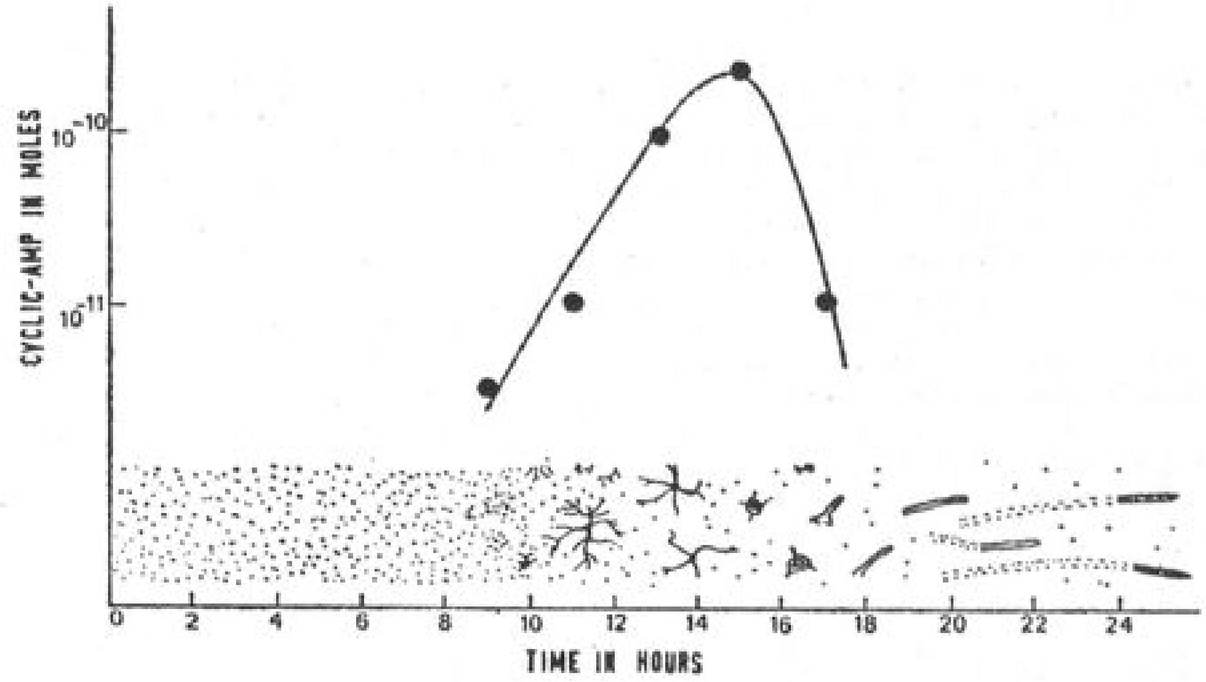

Fig. 4

La curva in alto indica le variazioni del feromone (*chemoattrattore*) (*acraeasina, cioè AMP-ciclico*) *prodotto dal* Dictyostelium *nelle varie fasi della formazione del corpo fruttifero. In basso sono rappresentate le fasi dei movimenti morfogenetici che portano alla formazione del corpo fruttifero. Il fenomeno può essere visto nello spazio come fascio di movimenti coordinati delle singole cellule, nel tempo invece come divenire di una forma attraverso stati successivi* (da Bonner et al., 1969).

The upper curve indicates the variations in the pheromone (*chemo-attractor*) (*acrasine, i.e., cyclic-AMP*) *produced by the* Dyctyostelium *in the various phases of the formation of the fruit-body. Below are shown the phases in the morphogenetic movements that lead to the formation of the fruit-body. The phenomenon may be seen in space as a band of co-ordinated movements of the individual cells, in time as the growth of a form through successive states* (from Bonner et al., 1969).

15. Per concludere questa nota, posso accennare a due direzioni in cui ritengo si dovrà estendere lo studio per trarre maggior profitto dall'approccio modellistico qui suggerito.

Da un lato si dovrà cercare la definizione delle restrizioni d'ordine topologico e d'ordine dimensionale cui deve sottostare un dispositivo di memoria per soddisfare a certi requisiti di utilizzabilità o di stabilità di contenuti. Simili ricerche ci condurranno a definire meglio il campo in cui svolgere le nostre ricerche sperimentali per localizzare e caratterizzare i dispositivi di memoria impiegati dagli organismi.

D'altro lato, la definizione di memoria come stato di un sistema in cui perdura traccia delle transizioni dagli stati precedentemente occupati invita ad una trattazione meccanico-statistica dei sistemi in oggetto: il problema che qui ci si presenta è perciò quello, facile a definirsi ma tutt'altro che facile da risolversi, di trovare soddisfacenti definizioni degli stati e delle transizioni che caratterizzano una cellula o un organismo nel loro divenire.

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MEMORY, MORPHOGENESIS AND BEHAVIOUR

Modern scientific literature has led to the use of the word « memory » also outside the psychological sphere: for example, one hears speak of genetic memory, in certain contexts, and also of the memory of an electronic computer. The author discusses the possibility of an unitary formal treatment of all these behaviours and of the structure of the physical systems that underlie them. He therefore suggests considering memory as the trace of a past state of a system: a trace persisting in it and conditioning its subsequent behaviour. Hence he derives a historical view which, within the framework of biological phenomenology, shows behaviour as an immediate prolongation of morphogenesis and as a morphogenetic agent in itself, to the extent of the trace that it can leave in the organism in action. This presentation of the subject leads to the rejection of much of the old confused terminology employed by psychologists and embryologists. In the last few sections of the work, the author discusses: the various strategies followed by differently organised groups of living beings, in order to canalise the information necessary for development and behaviour; the structural and functional characteristics of subsystems that are the sites of specialised memories and the prospects open to this type of analysis.

1. There are many kinds of natural phenomena, the interpretation and understanding of which have been considerably delayed by the fact that the experts were not in agreement on the respective competences in question, with the result that these phenomena have been set aside for too long in a sort of « no man's land », from which, at the most, they have from time to time been taken, only to be approached with the least suitable methods.

This is the case, for example, with *memory*.

The contribution made by philosophical psychology to the interpretation of this phenomenon may easily be summed up in a few words: one has only to think of associationism, and of Bergson[1].

As regards experimental psychology, it is sufficiently known what it has taught us, but it is also known that we can hardly be content with the classical - and even, under certain aspects, admirable - experiments carried out by Ebbinghaus[2] and with those carried out later in the same direction.

A great step forward was taken when new achievements - above all, the construction of technical memory devices - made it clear that, traditionally, we had been concerned merely with what we may agree to call psychological memory, and not memory in general.

In fact, we may also speak of memory in other contexts; the only precaution to be taken, in these cases, will be to remember that we are speaking not of psychological memory, but of memory, and nothing more, in a meaning that *includes* the use that is made of the term in psychology.

2. A reasoning such as this must have been adopted by Rignano[3] when, half a century ago, he spoke of memory with respect to the hereditary nature

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[1] In Aristotle we find passages deserving the greatest attention, in which are emphasised, among other things, both the dynamic nature of the memorising process, and above all of remembering, and the necessity for a material substratum for memory (*De Mem., passim*). Mention may be made of H. Bergson's essay *Matière et mémoire*, Paris, 1896.

[2] Ebbinghaus, H. (1885): *Uber das Gedächtnis*. See also, among the contemporaries, B. J. Underwood (e.g. *Forgetting*, in Sci.Amer. 210 : 91-99 (1964)).

[3] Rignano, E. (1922): *La memoria biologica*, Bologna, Zanichelli.

of characteristics, taking certain phenomena of psychology as a *model* of phenomena of genetics and the sciences of development.

Nowadays, biologists are less reluctant than hitherto to speak in terms of models, and some have even reached the point of bestowing upon the nascent modellism a certain logical discipline and epistemological awareness (see, for example, Stahl, 1967).[1]

Today we can say, neatly but perhaps rather pedantically, that Rignano's point of view was justified by the actual existence, between the two classes of phenomena that he considered, of a correspondence such as to respect their invariance for determined properties: that is to say, psychological memory and hereditary (genetic) memory have common formal (structural) properties, and this justifies taking one as a model of the other.

3. At this point, an attempt may be made to see how a similar invariance may be characterised better; in the pages that follow, I shall try to show that this characterisation is possible and - what is no less important - profitable.

For every discourse - and it is best to state this precisely at once - we can adopt different forms of language, but generally there is one of them, in the contingent context, that is seen to be more expressive than the others (perhaps also because whoever uses it has greater mastery of it): for these considerations on memory, presented at both a functional and a formal level, I consider it appropriate to make use of the language of cybernetics and, in particular, of that of the theory of information.

In fact, the problems relative to memory may be regarded, as I shall mention later, as problems of *inscription* of information in a determined material substratum and of the *reading* of the information previously inscribed.

In any case, at this point it is as well to break off momentarily from a general discussion and semantic considerations, in order to give a schematic outline of a couple of concrete situations.

The first example is drawn from the sphere of behaviour, the second from that of morphogenesis.

4. Let us suppose that we ask a passer-by the way to a certain place in a town. He may tell us what to do at each turning and cross-roads or else he may suggest following the route of a certain tram (Fig. 1).

In the first case we have received instructions about the choices to be made as each successive alternative is encountered; in the second case, a single instruction referring to a regular directional rule to be followed.

This second instruction might also be defined as a *utilisation criterion* for a piece of information that will be successively received *from outside*: in fact, the tram route has been fixed and, at every turning or cross-road, makes the same identical choice that must be made, as he proceeds, by whoever has to follow that route.

Now, whoever has been given a detailed set of instructions has to ask himself, at each turning, a question such as « Do I have to go left or right? »[2], and the answer is given by the series of instructions inscribed in his memory; but whoever has received an instruction of the second type transforms that question into one of the following type: « Which way does the tram go, left or right? », and in this case the answer is given by observation: that is, by information obtained extemporaneously from the outside world.

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[1] STAHL, W. R. (1967): *The role of models in theoretical biology*, in *Progress in theoretical biology* (Snell, F.M., ed.), I : 165-218, Acad. Press.

[2] To use binary alternatives.

This example clarifies one of the focal points of the question: in fact, in the situation in which an ordered sequence of detailed bits of information is provided, it is certainly possible to make the choices that are presented; in the alternative case, on the other hand, this will not be possible without the additional provision of external information.

However, this does not exclude the possibility that, in an adequate reference system, the « overall » type instruction may have a non-null information content. In fact, it allows the discrimination, in sensorial afference, among all the possible external sources of information, of that having an operative value; and this corresponds to the assembly of a system capable of selecting (and, where necessary, of seeking and amplifying) the stimuli being received.

The nature of an « overall » instruction, which refers to an external regularity to be utilised, is therefore *morphogenetic* (in the broad sense), since its result is the setting-up or re-processing of a structure.

5. It may then be asked whether, in the cases in which an « occasional » instruction is given, there exists or not, in the system, something equivalent to the structural and functional organisation achieved in the other case by means of what I have defined as a « utilisation criterion ».

There is reason to suppose that it does exist: an arriving signal has a different value, for different organisms, in relation to their specific identity and their individual history - that is, a value correlated with their *structure* (or organisation) at the moment considered.

It is obvious, for example, that many signals arriving from the environment are used differently by a man or by an insect, by a hungry animal or by a satiated animal, by a sexually mature individual or a sexually immature individual.

Emphasizing the functional value of the structure of a system amounts to saying that, in a certain sense, there exist in this system established circuits and pre-ordered selective criteria, created partly through the morphogenetic processes (in a strict sense) and partly according to what has been learned previously.

These considerations cause us to shift the discussion from the sphere of behaviour to that of morphogenesis.

6. Embryologists frequently speak of the « prospectic value » of a portion of egg or embryo, or of the « presumptive territories » from which determined organs are expected to derive; they then describe various mechanisms controlling development - mechanisms whose properties have been demonstrated by a series of classical experiments, chiefly carried out on the amphibians or on the sea-urchin.

But even here, similarly to what I said about the usual psychological studies on memory, the traditional formulation of the research is to a certain extent unsatisfactory and is marred by a vague terminology associated with weak and contingent biological philosophies: let us try to replace it with another, clearer terminology, once again borrowed from the theory of information.

In fact, we can describe a morphogenetic process, just like any other behaviour, as the carrying-out of a *program*, and this is basically nothing but a sequence of memorised pieces of information. In the different cases, there may or may not exist in the system a device for *checking* the degree to which the program is carried out: a device capable of intervening, by means of a feed-back, causing the behaviour to be resumed from a convenient point of its previous course (take, for example, the phenomena of regeneration, Fig. 2).

In order to follow up our observations, it will be as well to start from a very common fact in embryogenesis: that is, from the different destiny of two cells or, at any rate, of two portions of one and the same embryo, which differ, at a given instant, solely in the fact that they occupy two different positions in the architecture of the embryo.

It has been possible to show this by means of careful and shrewd manipulations of blastomeres and half-embryos at various stages of development[1].

It is what we can describe by means of the concept - neatly illustrated by Wolpert[2] - of *positional information*.

But we can carry out our studies on material belonging to different species or to different stages of development or to different embryonal organs and regions; and causal embryology teaches us that the destiny of the portion of embryo that we are considering will depend, in certain cases, more on its *present* position than on that occupied at a *previous* instant, while in other cases the contrary will be true.

At this point it should not cost us much to speak once again in terms of memory, and to say that, in particular embryos and at particular stages of development, a memory of the previous positional information is retained, whereas in other contexts this is cancelled by the positional information that has arrived: this amounts to saying that there has been a partial cancellation of the memory itself.

What, then, can be the meaning of *memory*, in this sense?

Memory is merely *the trace of a previous state* of the system - a trace that persists in it and conditions its subsequent behaviour.

7. A definition of this type is found to be satisfactory, on closer inspection, also in the case of memory with which experts in psychology and ethology are concerned.

In fact, both in the morphogenetic facts (in the strict sense) and in those of learning, we can fairly easily make ours a *historical* view in which the events (*transitions* between different states of the system, *experiences* of the individual) condition the behaviour of the system at a subsequent instant to the extent of the trace that they leave behind them; and this trace we shall certainly call *inscribed memory*.

However, it is not unprofitable to distinguish two different systems of reference that give a different significance to a mnemic trace: memory is, on the one hand, information stored in a functional way, usable within the system (individual) in which it is inscribed; on the other hand, it is a document - for the historian that observes from outside - of an event that has occurred: certain facts, such as a cancellation or an omission, have very different meanings in the two contexts.

Definitions of this sort, however, leave out of account a great number of determinations, especially those relative to the physical nature of the system capable of memory. It should be noted that this does not indicate inadequacy of description but rather an attitude that invites us not to limit *a priori* our attention to particular classes of object that are acritically considered the only possible site of the properties that we are studying.

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[1] For a partial synthesis of what has been achieved in this respect, on two different zoological groups, see, for example: HARRISON, R. G. (1969): *Organization and development of the embryo*, Yale University Press; HÖRSTADIUS, S. (1939): *The mechanics of sea-urchin development, studied by operative methods*, Biol. Rev., 14 : 132-179.

[2] WOLPERT, L. (1969): *Positional information and the spatial pattern of cellular differentiation*, J.Theor.Biol., 25 : 1-48.

At any rate, what must be taken as recognised is the impossibility of conceiving a memory unassociated with any material substratum.

8. Naturally, the physical nature of a memory device is not indifferent. If the entries in a dictionary were recorded on a magnetic tape instead of being printed on the pages of a volume, the average time required to look up any one of them would be very different; and it would again be different according as to whether we used a single tape or several separate tapes (arranged in order).

This opens up an extensive and important discussion on the topological and dimensional aspects of memory devices - a discussion that would lead us far away and in any case, under many aspects, has not yet been achieved to any extent.

In my opinion, the only aspect that is worth illustrating here is that relative to the possible separation, within a system, of a compartment reserved for the information memory that it is particularly important to keep from accidental cancellation and that, at the same time, it is necessary to be able to use again at particular subsequent moments: take, for example, the DNA of a cell, books, magnetic tapes.....

The whole system is obviously a seat of memory, in the sense described above, but this is memory for which there exist no specific problems of conservation and regular re-use; on the other hand, every time we have some reproducible scheme, susceptible of further applications, it will be as well to entrust it to a more conservative compartment, a specialised subsystem, in fact, for a more lasting and organised memory.

In the present work, I shall not deal with the problem of the siting of this memory: at any rate, it is certain that we now know enough about it as regards genetic memory[1], but far less as regards the memory to which the psychologist refers[2].

Here, what interests me more is to emphasize how it makes perfectly reasonable sense to speak of the memory of a molecule or of any multi-stable - or even only bi-stable - mechanical or electrical device: take, for example, a flip-flop, the behaviour of which depends on two simple factors: the previous state (conditioning, since it is memorised) and the arrival (e.g. from outside) of a current pulse. Such a device is, to a certain extent, the electrical equivalent of the *elememtary binary memory device*[3].

9. This schematic approach makes it easy to speak about memory in the language of the theory of information, even from the quantitative aspect. With all the limitations of any assessment of information, a calculation is possible also with regard to memorised pieces of information.

Here, however, it is necessary to mention two different points of view that we can adopt in carrying out information measurements: in fact, we can consider either the information *content* of a system or else the quantitative characteristics of the *transmission* of information from one system to another.

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[1] In this note, prompted by the concepts of information theory, mention should be made of devices for controlling the reading (delivery) of genetic information (the work of Jacob and Monod, etc.).

[2] This must be admitted, in spite of the experimental data recently collected in regard of the role of various molecules (RNA, polypeptides) in the memory processes. See, in this respect, the excellent volume *Biology of memory* (ed. K. H. Pribram and D. E. Broadbent, Acad. Press, 1970).

[3] Did not the memories of certain old calculators possibly consist of flip-flop systems?

In the matter of memory, it is possible to discriminate something similar, distinguishing three different phases in the mnemic process.

In fact, we have the *creation* of memory: that is, the inscribing, in the structure of the organism, of the traces of its successive states.

Or we can concern ourselves with the *structure* of a system (of an organism, let us say), since it is already constructed and organised, as a result of its whole previous history.

Finally, we can see to what extent the structure possessed by an organism at a given instant conditions its *behaviour*: in other words, what is the effect of the memorised trace of the previous stages on the present behaviour of the system being studied.

The first of these phases may be called the writing and the third the reading of a piece of information, while the second represents an instantaneous section of the dynamic memorisation process.

10. By presenting the problems in this way, it is easy to find an area of encounter among disciplines that are traditionally so far removed and apparently so different from one another, such as those of development and those of behaviour.

In fact, they may appear as a view of one and the same object in two different dimensions: morphogenesis is history, the history of the successive states of the system; behaviour is current fact, into which is prolonged a history that extended up to a moment ago, and which, dialectically mediated with its previous instants, will itself become history within another moment.

However, with the exception of the borderline cases, it is advantageous, in practice, to consider the two points of view separately, also because, in ethology and in morphogenetics, there are different systems of reference that make it possible to make quantitative assessments of the information in question - assessments through which it is possible to identify optimisation problems to be solved by adequate strategies.

11. Nevertheless, in certain cases it is really difficult to establish where the morphogenetic aspect of the phenomenon ends and where the ethological aspects begins; indeed, the two categories become meaningless in behaviours such as the aggregation of the cellular slime-moulds (Acrasina).

In these organisms, the morphogenesis of the fruit-body is the final expression of the orientated movement of a population of amoebas in response to a chemical stimulus (Figs. 3 and 4).

If, on the one hand, the phenomenon reminds us of the « surface recognitions » that exist between one cell and another, it is no different from the formation of a line of ants through the action of a pheromone. In both cases, what is occurring is the building-up of a more complex structure through which a simple compositional law operates on simpler elements, from the point of view of which the phenomenon seems like a movement orientated in response to an environmental regularity of a chemical nature.

On the other hand, morphogenetic movements also occur in the ontogenesis of higher organisms: it will be sufficient to mention the migration of the cells of the neural crest (Weston, 1970)[1] and that of cells of the telencephalon of the Tetrapods (Clairambault and Derer, 1969)[2].

12. However, in resuming the discussion on memory in more general terms, I shall try to explain in what sense it may be said that each system (which, for us, may be an organism, an individual) is what its history has made it, or - it may also be said - is its own history.

In reality, the history behind a living being has two dimensions: one individual, the other embracing the sum total of the previous generations.

Consequently, the memory that an individual carries with it is a combination of memory of the species and individual memory, the latter grafting itself onto the former as its necessary prolungation.

And the course of the experiences of the species runs closely parallel with that of the experiences of the individual: the former arise with the mutations, go through a selective process and - if favourable - enter the «memory of the species», or the genetic memory; the latter arise from the contingency of the life of the individual and enter, more or less stably, its memory.

The parallelism cannot be pushed much further, however, since the two selective processes act upon a different substratum (populations, subunits - e.g. cells, molecules - of an individual).

13. In any case, memory is a channelling of behaviour, a restriction of degrees of freedom.

This consideration, which is born from a study of individual behaviour, leads us to reflect also upon certain evolutional problems.

Regarding what we have memorised during our life as an individual, we may ask ourselves, for example, whether it is opportune to transmit it all to our descendents.

Basically, this is the problem of the hereditary nature of acquired characteristics, improperly associated with the name of Lamarck (see Omodeo, 1960)[3].

In this respect, brief reflection convinces us that many contingent factors determining the life of an individual are irrelevant for any other individual, being « too personal »; a good teacher does not confine himself to re-presenting his own experience to the pupil, but seeks to develop in him an autonomous capacity for judgement and choice among the alternatives that are offered to him.

Hence, that there is no proof of the hereditary nature of the acquired characteristics is a fact that should not surprise us, in spite of the contingent advantages that may be suggested. On the other hand, we know that among social animals, such as man, there occurs an extra-genetic *cultural* transmission of patterns acquired and tried out in the previous generations.

But phylogenesis - and this is a new and important question - is not so much the history of individuals as the history of « models », of rules of reaction: the contingencies of the life of the individual are not relevant but are re-presented *ex novo* to each generation.

We must therefore take care in interpreting the classical orthogenetic series (such as that of the horses, the titanotheres or the elephants) and the evolutional parallelisms and convergences; in fact, at times, we may find ourselves faced with actual « compulsory choices », at other times we shall merely be faced with expected *a posteriori regularities*.

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[1] WESTON, G. A. (1970): *The migration and differentiation of neural crest cells*, Adv. Morphogen., VIII : 41-114.

[2] CLAIRAMBAULT, P. and DERER, P. (1969): *Les migrations cellulaires au cours du développement du télencéphale des Tétrapodes*, Année Biol., VIII : 629-656.

[3] OMODEO, P. (1960): *Darwin e l'ereditarietà dei caratteri acquisiti*, « Scientia », LIV : 22-31.

14. Another general aspect to be considered, from the phylogenetic point of view, is that of the canalisation of the information necessary to development and behaviour: a problem that was much debated by Waddington[1] from the embryological and phenogenetic aspects.

In fact, in organisms of different evolutional level (Procariota, Metazoa, Primates) we can note different strategies for the memorisation of this information.

The two fundamental schemes may be reduced to the alternatives shown in the example of the pedestrian asking the way: in fact, what may be given is either a precise memorisation of the necessary information or else a working criterion that will become operative in accordance with the integration coming from the information obtainable from outside.

A memorisation of the first type combines an obvious advantage with an obvious limitation: it is more certain, since the availability of the memorised data depends only on the integrity of the organism that possesses it and can utilise it; on the other hand, it cannot go beyond assigned dimensional limits, both because the *conservative elements* of memory are, to a certain extent, compartments and therefore represent only a limited portion of the system, and because, in addition to certain numerical values of the memory units, there arise unsurmountable problems of utilisation and optimisation, as has been shown by Bremermann (1967)[2].

This problem no longer exists in the case of the memorisation of « reaction rules »; in this case, there is no precise expectation that a behaviour will be put into practice, since the latter, broadly outlined, will be specified by the concrete determinations of the exterior context.

The arrival, from this exterior context, of sufficient integrative external information is certainly more or less random, but this is a risk that organisms have had to and are able to accept in order to achieve the structures and express the more sophisticated behaviour that we find, for example, in a Vertebrate. The exterior pattern to be acquired will, however, have to exist on a dimensional scale suited to the sensorial capacities of the organism[3].

Among the more striking examples of the importance of morphogenetic processes of this type, I merely confine myself here to mentioning that the architecture of the C.N.S. of a higher vertebrate is traced genetically only in its broad outlines (see again Bremermann, 1967) and that the regular assembly of this system is conditioned by the experiences of the individual, especially the very early ones (take, for example, *imprinting* and the classical studies carried out on this subject by Conrad Lorenz).

The two different canalisations of the memory necessary for morphogenesis and for behaviour obviously represent a different cost, both in terms of relative dimensions of the *memory-compartment* and in terms of energy and material:

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[1] I confine myself here to citing two well-known books by C. H. WADDINGTON: *The strategy of the genes* (Allen and Unwin, London, 1957) and *New patterns in Genetics and Development* (Columbia University Press, New York and London, 1962).

[2] BREMERMANN, H. (1967): *Quantitative aspects of goal-seeking self-organizing systems*, in *Progress in theoretical biology* (Snell, F. M. ed.) I : 59-77.

[3] The presence and activity of other organisms may naturally provide many regularities usable in different contexts: something that was evidently denied to the earliest organisms that appeared on the Earth.

the biomass or number of individuals eliminated through selective processes, because they were « created wrongly or untimely ».

15. To conclude this note, I mention two directions in which I consider that studies should be extended in order to derive greater profit from the modellistic approach suggested here.

On the one hand, a definition must be sought for the restrictions of a topological and dimensional order to which a memory device has to submit in order to satisfy certain requirements of the usability or stability of the content. Similar studies will lead us to a better definition of the field in which to carry out our experimental research for localising and characterising the memory devices used by the organisms.

On the other hand, the definition of memory as the state of a system in which there persists a trace of the transitions from the previously occupied states invites a mechanico-statistical treatment of the systems in question: the problem with which we are faced - easy to define but anything but easy to solve - is that of finding satisfactory definitions of the states and transitions that characterise a cell or an organism in its growth.

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A. MINELLI

METODO SCIENTIFICO E LEGITTIMAZIONE DELLA SOCIOLOGIA

La sociologia, si dice spesso, è una scienza giovane. Ma, si dice ancora, è poi una scienza? Può esserlo? La disputa è antica e oggi, per chi ha preso posizione per l'una tesi (sociologia come scienza inter pares*) o per l'altra (sociologia come filosofia della società, intuizione sociale ecc.) è una disputa per certi versi scontata. In realtà anche fra i sostenitori della seconda tesi, talvolta è presente l'ambizione a definire la propria metodologia quale procedimento « scientifico », pur nella necessità di aggiustamenti* ad hoc, *di concezioni* sui generis *della scienza stessa... A nostro avviso la scienza non ha alternative che concedano le stesse garanzie di oggettività e di comunicabilità. Essa si caratterizza per l'assunzione di un suo metodo generale (da cui le scienze particolari si differenziano solo per le diverse tecniche di ricerca) e la sociologia è una scienza se assume questo metodo. Ora, le difficoltà che si presentano in tale assunzione, sono spesso pseudo-difficoltà, ritenute reali, inoltre, solo in una prospettiva che non riesce ad accettare che esse si siano presentate e si presentino in ogni altra scienza, e che la metodologia attuale ne renda possibile la reinterpretazione e quindi la risoluzione. Le uniche difficoltà reali sono altresì collegate, in sociologia, non già con l'utilizzabilità del metodo scientifico, ma con la complessità del suo oggetto e con i suoi rapporti con la società studiata.*

1. CONOSCENZA ESISTENZIALE E CONOSCENZA SCIENTIFICA. — Necessita anzitutto chiarire cosa si intende in questo saggio per conoscenza scientifica. Essa, a nostro avviso, va distinta da ogni incontro con la realtà che persegua fini tecnici o politici: anche se questi processi possono essere interpretati come forme di conoscenza non si tratta però di conoscenza scientifica, in quanto quest'ultima si caratterizza per il perseguimento non già di stati desiderati, ma di comprensione del reale attraverso l'impiego di un metodo.

In altri termini conoscere è possibile in molti modi, conoscere una nuova realtà sociale uscente da una rivoluzione è possibile anche senza aver conosciuto la vecchia: cambiare, disgregare, costruire è insomma certamente conoscere. Ma non necessariamente è un conoscere scientifico: lo sarà solo se lo si sarà ottenuto con il *metodo* della scienza.

In un senso generalizzato, di recente un antropologo ha definito *riconoscimento* l'interiorizzazione della *realtà data*, processo questo essenzialmente passivo che si attua nella socializzazione e si stabilizza nel conformismo, e *conoscenza* ogni invenzione di nuove realtà e valori [1].

Si vede bene, in questa presa di posizione, il primato che si intende attribuire, nel processo conoscitivo, alla prassi o comunque all'attività innovativa.

Il metodo scientifico consta altresì essenzialmente di procedimenti logico-sperimentali tesi a « verificare » le ipotesi o le teorie oppure, nella versione cara al Popper, tesi a « falsificarle » in modo tale che, finché non vi si riesce, esse si vengono a collocare in quella che il Kuhn definisce la « scienza normale » alla quale « una particolare comunità scientifica, per un certo periodo di tempo, riconosce la capacità di costituire il fondamento della sua prassi ulteriore », [2] in quanto fornisce *paradigmi* capaci di dare risposta a una certa classe di domande e spesso a certi bisogni applicativi.

Come si vede non si insiste qui coll'attribuire alla conoscenza scientifica un carattere più o meno genuino di altri, ma si è solo ristretto, per un indispensabile bisogno di chiarezza, l'ambito delle conoscenze scientificamente reperite a quello dominato dal metodo scientifico logico-sperimentale. Ciò non toglie che sia possibile costruire, innovare, in definitiva conoscere anche senza la scienza: l'intuizione, la sensibilità, le capacità di sintesi dell'individuo, scienziato o no, sono gli strumenti di gran parte della nostra conoscenza quotidiana, e sono anche spesso serviti da base di partenza per ricerche e studi altamente scientifici. Si intende solo dire, in conclusione, che la scienza è un modo di conoscere la realtà fondato sul metodo logico-sperimentale, mentre ogni altro conoscere, migliore o peggiore a seconda delle convenienze personali o sociali, dei gusti e delle ideologie, non si può far risalire ad esso. Non è raro infatti assistere all'ostentazione da parte di alcuni, di una « scientificità » del proprio lavoro dovuta esclusivamente al prestigio diffuso di quell'aggettivo (come fu, secondo Sorel, per Karl Marx), mentre nella realtà della loro opera non pagano il necessario prezzo di tale blasone adottando, insieme al nome, anche il *metodo* della scienza.

2. NEO-POSITIVISMO, PRAGMATISMO E RIVALUTAZIONE DELLO SCIENZIATO. — Alla base di questa concezione della scienza, che attribuisce il suo principale segno di riconoscimento al metodo di verifica delle ipotesi, si può reperire facilmente un'impostazione neopositivistica o logico-empirica (che per certi aspetti rinnova principi reperibili già in Galilei), ma che peraltro non è qui seguita fino alle sue estreme conseguenze. Ciò che intendiamo utilizzare della impostazione neopositivistica è, anzitutto, la sua decisa indisponibilità all'inserimento nel sistema scientifico di elementi metafisici. Ma già qui lasciarsi trascinare da questo, peraltro accettabile, presupposto fino alle sue conseguenze ultime potrebbe significare approdare da un lato a un rinnovato « fisicalismo » che vorrebbe ridurre la scienza a un insieme di proposizioni quali fatti fisici, oppure dall'altro al solipsismo di Wittgenstein, che contrasta col carattere intersoggettivo che le conoscenze scientifiche possiedono di fatto. A nostro avviso infatti se è certamente inammissibile, ed anzi impossibile, inserire nelle proposizioni della scienza elementi metafisici, non è però detto che riflessioni extra-empiriche non siano presenti nello studio del metodo scientifico. Anzi, tutta la dottrina del metodo è, in definitiva, extra-empirica poiché se così non fosse non potrebbe darsi altro che una realtà fisica che pensa se stessa. Quelle che per la scuola neopositivistica ortodossa sono « proposizioni prive di senso » lo sono anche a nostro avviso, solo se non è possibile darne una

versione empirica, ma non già intendendo il confronto, fra proposizione e fatto, in termini di proposizioni indecomponibili (Wittgenstein) oppure di identità logica fra proposizione-fatto e fatto fisico (Neurath), bensì di constatazione della corrispondenza fra le previsioni enunciate in base a concetti empirici da un lato e gli oggetti empirici dall'altro, anche di natura complessa (contra Wittgenstein) e definendo il procedimento di verifica non tanto logico (contra Neurath) quanto metodologico ossia dotato di una convenzionalità non tanto formale nè, ovviamente, empirica (che sarebbe una contraddizione in termini) bensì, per così dire, processuale. All'atto della verifica infatti si nota (non «si presuppone») un accordo psicologico fra gli osservatori intorno al «dato» che conferma o smentisce la previsione in modo intersoggettivo.

A questo livello si realizza un congiungimento fra convenzione e empiricità che non può risolversi qualitativamente, in una delle due componenti, ma in un processo che non va difeso *a priori*, ma constatato nella pratica.

Il concetto di «disposizione presente» (che sarà ripreso più avanti) che noi attribuiamo ad una cellula di riprodursi, non è ontologico, ma metodologico: ciò sta ad indicare che il termine «disposizione presente» non è né vero né falso nel momento in cui lo si esprime, né è riconducibile a tale stato mediante calcoli logici, ma è solo un concetto metodologico costruito per *rendere possibile* la verifica della legge della riproduzione e la classificazione degli oggetti capaci di riproduzione e non già *per venire*, da questa, verificato.

Secondo Neurath «il sociologo è completamente libero nella sua ricerca di leggi. L'unica condizione che deve sempre rispettare è quella di riferirsi, nelle sue previsioni, a strutture determinate spazio-temporalmente» [3].

Noi intendiamo dunque questa «libertà» dello scienziato anche nello assumere concetti non evidenziabili empiricamente, ma ponibili metodologicamente per rendere possibili le spiegazioni e le previsioni. Mentre è chiaro che ontologicizzando la «disposizione presente» la si ricondurrebbe all'*entelechia* e quindi alla metafisica deduttiva, è del pari palese che solo attribuendo metodologicamente un nome alla «causa» provvisoria e ipotetica di un certo fatto, si rende possibile continuare la ricerca per trovare connessioni più precise ed empiriche.

Da tutto questo si avverte come per noi è indispensabile concepire la conoscenza scientifica quale processo interattivo che vede nel ricercatore e nel mondo esterno le due realtà che la rendono possibile in base alla capacità del primo di assumere convenzioni metodologiche per l'elaborazione dei concetti e per la stima della verifica. È infatti nel passaggio dal simbolico all'empirico che è crollata l'impostazione dei neopositivisti antidualistici, nel rifiuto di concedere allo scienziato la facoltà di tramite che invece noi gli attribuiamo. Lo stesso Weinberg, che più in là si sforza di trovare una nuova teoria del linguaggio che tenga fede alle vedute neopositivistiche fondamentali, riconosce che « la difficoltà della lingua formale risiede nel fatto che entro tale lingua è impossibile esprimere la relazione fra le proposizioni protocollari del linguaggio oggettivo e i dati empirici a cui queste proposizioni si riferiscono » [4]. Egli propone poi di introdurre il termine « dato » di esperienza. Ora, non si vede come si possa concepire il dato se non come tipico prodotto dell'attività concettualizzatrice (astrazione) dello scienziato, non metafisica, almeno a questo livello, ma metodologica. Un altro punto fondamentale su cui concordiamo col neopositivismo, e lo si è già accennato implicitamente, è il ricorso alla previsione quale passo caratterizzante della scienza. Una volta sciolto il nodo della impossibilità di una verifica intersoggettiva e qualora si conceda allo scienziato di costituirsi in produttore autonomo di convenzioni, (altrettanto quanto la realtà empirica è autonoma nel produrre oggetti) la previsione ridiviene effettivamente uno strumento validissimo di conferma o smentita delle leggi scientifiche. Anche qui però un fisicalismo estremista parrebbe, paradossalmente, attribuire alle leggi quel carattere ontologico che rivestivano le leggi astronomiche per il Galilei [5], mentre a nostro avviso val la pena di fermarsi al punto in cui si afferma che una legge è uno strumento metodologico per spiegare e prevedere. Anche su questo punto si tornerà più avanti, ma qui è già possibile peraltro escludere, da quanto detto, una impostazione rigidamente pragmatistica della previsione. La previsione va intesa qui non già come perseguimento dell'utile, ma di una verità empirica la cui enunciazione è oggettivata per mezzo di connotazioni convenzionali. Mentre infatti la acquisizione di qualche realtà desiderata è possibile al limite anche con procedimenti del tipo « per tentativi ed errori » ossia saltando a piè pari la spiegazione dei fatti, per noi è indispensabile tenere presente che scopo concomitante della previsione è di garantire o confutare la spiegazione dei fatti, intesa come riconduzione degli stessi a leggi generali. Un sociologo, Emile Durkheim, ha acutamente descritto questo tema: « ...supposons qu'on ait persuadé à un homme d'esprit fruste qu'un mal physique dont il est atteint, est dû à des esprits malins que se sont introduits dans son corps (le cas est d'ailleurs fréquent dans les sociétés primitives). On lui fait absorber une substance très désagréable, mais dont on lui assure qu'elle mettra en fuite les esprits qui le tourmentent. Il le croit et il guérit. Le résultat est bien le résultat escompté; c'est même celui qui « convient ». L'idée n'en était pas moins fausse » [6].

È chiaro che una scienza intesa esclusivamente quale sistema di previsioni verificate che forniscano utilità, una scienza che, in altri termini, non si preoccupasse di spiegare i fatti che assume quale campo di studio, si riterrebbe soddisfatta del risultato ottenuto, ritenendolo nel contempo spiegato definitivamente per il fatto stesso di essere approdata all'utilità che intende quale massimo valore.

È inoltre da supporre che questa considerazione critica valga bene anche per il neopositivismo estremista: infatti, se una previsione regge al confronto dei fatti (a parte la difficoltà già vista del « confronto ») in quanto accordantesi con i fatti empirici semplici e non contraddittoria e coerente sul piano logico, il neopositivista estremista si riterrà soddisfatto, ritenendo di già *spiegato* il fenomeno preso in esame. In realtà la mentalità scientifica non si risolve in questo accordarsi logico e atomico: valori diversi spingeranno altri scienziati a « falsificare » la legge appena reperita, e se nuove ricerche verranno portate a buon termine, nuove spiegazioni saranno accolte, ad onta della loro contraddittorietà e non coerenza col «sapere consolidato» o non riconducibilità a fatti atomici.

Una scienza intesa quale fonte di spiegazioni da porre aldilà di ogni limite di falsificabilità, proseguirebbe in altre parole sulla strada della ricerca in merito all'oggetto già spiegato in una certa versione, e riuscirebbe a dare spiegazioni più valide e insieme previsioni, quanto meno, altrettanto certe, e probabilmente estendibili ad un maggiore numero di casi.

È al proposito di questo compito della scienza che, infine può ancor oggi venire accolto un altro cardine della epistemologia neopositivistica, sebbene ancora con qualche cautela: il programma unificatore della scienza. È in effetti a questo particolare riguardo che, a nostro avviso, va visto il risultato più genuinamente accettabile della logica della scienza: lo studio delle interconnessioni possibili fra leggi di diversi ambiti scientifici, allo scopo di rendere più precise le spiegazioni e, quindi, le previsioni.

Ma anche questa opera di per sé evidentemente imponentesi nel futuro integrato del mondo scientifico e tecnologico, non dovrebbe pretendere di assurgere a unico e definitivo inglobamento dello scibile e del reale, tenendo invece presente che la scienza non è la sola realtà fornita di senso, ma solo quella forma di conoscenza che paga la sua generalità e la sua efficacia, non già *negando l'esistenza* di altri aspetti e di altre modalità conoscitive dell'uomo, ma solo *trascurandoli metodologicamente*. Essi, in altri termini, non sono *assolutamente* « privi di senso», ma unicamente estranei al senso scientifico.

3. La metodologia come strumento generale dell'intuizione individuale. — È necessaria inoltre, anche per chi si appresta a scegliere una « via alla sociologia », una scelta di fondo fra la sociologia come scienza e la sociologia che si avvalga prevalentemente della intuizione, della partecipazione, della psicanalisi, della metodologia storicista ecc.

La prima si fonda sulla convinzione che anche la sociologia è una scienza empirica, anche se quasi sempre ha a che fare con una empiricità indiretta, ossia mediata dagli oggetti (gli individui o i gruppi) e non risolventesi in essi, ma nelle strutture e nei processi (fatti) cui essi danno luogo, pur sempre percepibili mediante indicatori empirici.

Dalla scelta epistemologica discende una scelta metodologica. Se la sociologia è scienza empirica, non può che attuare il metodo tipico delle scienze empiriche.

I metodologi possono allora rivelare sempre più la loro insostituibile funzione orientatrice, specialmente nell'estrema prolificità della scienza odierna, anche perché non sempre gli scienziati hanno una precisa consapevolezza metodologica, nonostante nella loro opera concreta rivelino, agli occhi di un osservatore, una uniformità di procedure ben evidente.

Quando C. Whright Mills [7] riporta le *opinioni* di fisici come Kusch e Bridgman circa la loro convinzione che non esista un metodo scientifico e ricorda come il processo creativo, per William S. Beck, « sia così intimamente legato alla struttura emozionale dell'individuo che esso si presta male alla generalizzazione », commette a nostro parere un errore di valutazione della metodologia, infatti:

1) scopo della metodologia non è l'imporre allo scienziato un modo di procedere; scopo del metodologo è di scoprire il metodo effettivamente seguito, anche inconsapevolmente, dallo scienziato nella ricerca, al fine di studiarne la struttura logica e affinarne gli aspetti teorici e tecnici a tutto vantaggio dello scienziato stesso;

2) Mills prende per buone le affermazioni dei fisici da lui citati, ossia ripone nelle loro *opinioni soggettive* la fiducia circa il loro procedere creativo emozionale... dimenticando, ad esempio, le solide basi di matematica metodologica che i corsi accademici e gli studi personali conferiscono al fisico. Le « regole » metodologiche spesso sono ignorate dallo scienziato poiché egli si muove in una tradizione ormai permeata dai criteri della scienza teorico-sperimentale. D'altra parte, ripetiamo, la loro esplicitazione può essere di estrema utilità poiché, per riprendere il pensiero del Kuhn [8] in riferimento alla consapevolezza metodologica degli scienziati « se mai essi hanno appreso astrazioni di questo genere, lo mostrano principalmente attraverso la loro abilità nel condurre una ricerca con successo. La scienza normale può procedere senza regole solo fin tanto che la comunità scientifica relativa accetta senza discutere le particolari soluzioni di problemi che sono già state raggiunte. Le regole dovrebbero perciò acquistare importanza, e la caratteristica scarsa considerazione ad esse riservata dovrebbe scomparire, ogniqualvolta i paradigmi o i modelli sono sentiti come insicuri. Questo è esattamente ciò che accade ».

3) Da Galileo ai giorni nostri, pare che le linee generali del procedimento scientifico si possano considerare note; importante è però non attribuire loro pretese che non hanno. Il metodo scientifico indica unicamente i *passi* che si debbono seguire (e che si sono rivelati nel procedere concreto degli scienziati) al fine di ottenere descrizioni o spiegazioni attendibili in base a comuni criteri di verità fondati sulle posizioni epistemologiche oggi accettate nella scienza empirica.

Nulla può dire il metodo, circa il modo con cui essere colpiti da

« idee geniali » o « intuizioni originali », ossia circa il modo di elaborare nuove ipotesi di lavoro. Per dirla con il Popper, « dalla metodologia mai ci si devono aspettare verità profonde » [9], ma solo dunque indirizzi logico-pratici. In base alle conoscenze preesistenti (oltre che da osservazioni casuali) nasce nella mente del ricercatore il *problema* su cui poi, orientato da *ipotesi di lavoro*, cercherà di sviluppare la ricerca.

È in genere nella formulazione dell'*ipotesi* (che è in fondo una « soluzione anticipata » del problema, in attesa di verifica) che si nota quella creatività cui si riferisce Mills, ma essa rimarrebbe vuota intuizione se non sottoposta alla rigorosa prassi del metodo scientifico. Sempre per evitare l'impressione di cedere ad un vuoto nominalismo, chiariremo ora che per noi le scienze empiriche propriamente dette, ossia quelle che sono in grado di adottare il metodo logico-sperimentale, sono le *scienze naturali* (fisica, chimica, biologica ecc.) e le *scienze dell'uomo* (individuali = psicologia; sociali = economia, sociologia, antropologia, ecc.).

L'assunzione centrale del metodo logico-sperimentale è allora che si ha una scienza quando, avendo a che fare con oggetti o fatti *empirici* (ossia osservabili direttamente o indirettamente), si giunga a dare, del loro assetto e del loro mutamento, una *spiegazione* in termini di *leggi* e di *teorie verificabili* che concedano di *prevederne* gli assetti e i mutamenti futuri.

4. Oggetti e fatti empirici e spiegazione scientifica.

— Chiariamo ora ogni punto messo in risalto nella definizione appena costruita. Si è detto « oggetti o fatti empirici ». Con ciò si intende che è possibile fare opera di scienza solo se si è di fronte a oggetti in qualche modo evidenziabili agli occhi dei sensi anche, come è per gran parte della fisica contemporanea, con l'ausilio di strumenti che rivelino (come il Geiger) l'esistenza di oggetti o fatti empirici non direttamente osservabili, e così come in sociologia, attraverso gli indicatori, gli indici statistici e altri strumenti di rilevamento, è possibile rilevare strutture e processi sociali.

L'evidenza empirica degli oggetti è definita opportunamente da C. G. Hempel [10] « diretta » o « indiretta » a seconda appunto della possibilità, per l'uomo, di constatarne l'esistenza direttamente oppure attraverso rilevamenti (come l'eccitazione del Geiger) che presuppongono l'esistenza di oggetti o di fatti.

Si è parlato di « spiegazione ». Per essa è da intendersi la deduzione dei fatti da spiegare, da leggi o teorie. Si abbandona così tutta una serie di interpretazioni dello spiegare, reperibile in sede di filosofia essenzialistica o metafisica, oppure in quella fisica classica che pare concedere alle « cause » una realtà ontologica. È però inglobabile nella concezione nomologica della spiegazione (come deduzione da leggi) anche una forma di spiegazione che è oggi di larga adozione anche in sociologia, ossia la spiegazione funzionale.

Quando si asserisce che un sistema funzionale tende a ripristinare un certo stato allorquando viene perturbato, si asserisce infatti implicitamente che tale comportamento omeostatico è assunto quale legge [11]. Un funzionalismo depurato da tentazioni teleologiche in senso tradizionale (ossia finalistiche) è accettabile ogni qualvolta si sia in grado di reperire « sistemi » organici o sociali o di altra natura, dotati di qualche forma di autoregolazione [12].

La forma logica della spiegazione è allora la seguente, definita quale modello nomologico-inferenziale:

$$\begin{array}{cl} \left.\begin{array}{c} L_1\ L_2\ \ldots L_n \\ C_1\ C_2\ \ldots C_n \end{array}\right\} & \textit{explanans} \\ \hline E & \textit{explanandum} \end{array}$$

Nella terminologia del Popper le $L_1\ L_2\ \ldots L_n$ corrispondono alle « asserzioni universali: cioè ipotesi che hanno il carattere di leggi di natura »; le $C_1\ C_2\ \ldots C_n$ sono altresì « asserzioni singolari che valgono per l'evento specifico in questione... Dalle asserzioni universali... deduciamo l'asserzione singolare » [13].

5. Le leggi e i fatti sociali: sociologia e storia. — Si è parlato, nella definizione, di *leggi*. Le leggi sono enunciazioni che stabiliscono un'uniformità di comportamento degli oggetti di una scienza. Esempi di leggi sono, la $Ec = \frac{1}{2}\ mv^2$ della fisica che esprime il legame fra l'energia cinetica, la massa e la velocità di un corpo che cade; il predicato « ogni qual volta aumenta la domanda di un bene, il suo prezzo tende ad aumentare » per la economia di breve periodo; l'enunciazione « attorno ai 20 anni di vita il timo si atrofizza » per la medicina ecc.

La caratteristica fondamentale di una legge scientifica è la sua *generalità*: la legge si riferisce infatti a tutti gli oggetti o i fatti compresi dal predicato con cui viene enunciata. Non sono quindi possibili leggi scientifiche su un evento singolo e irripetibile. La storia, ad esempio, ha un campo di studio costituito da eventi individuali e irripetibili, per cui non ha senso scientifico parlare di « leggi storiche », mentre può essere possibile, in sede di sociologia, applicare una legge reperita oggi a fatti certi accaduti ieri e aventi le caratteristiche specificate nella legge attuale.

L'analisi storica, preoccupata di dare una visione dinamica (diacronica) degli eventi, non si sofferma, né potrebbe farlo, sperimentalmente su un evento E per farne oggetto di prove e riprove tese a reperirne le leggi generali di comportamento. La storia, quale susseguirsi di fatti, è invero paragonabile ad un laboratorio in cui le variabili mutano continuamente, per cui l'attribuire a singoli fatti o a classi di fatti (mai omogenee) le caratteristiche di « cause » e di « effetti » ha un senso solo se ricondotto alla necessità, per gli storici imprescindibile, di dare allo sviluppo degli eventi un filo conduttore logico e non meramente cronologico. Tant'è che le interpretazioni storiche sono tanto numerose quanto, spesso, incomunicabili fra di loro.

Generalmente lo storico ha in serbo alcune variabili chiave (l'Economico per Marx, il Religioso per Weber) che, ad ogni nuovo evento rilevante E, mostra di elevare a leggi generali (inespresse o esplicitate) da cui trarre chiarimento e spiegazione dell'evento singolo. Sua preoccupazione non è, in altri termini, l'andare alla ricerca di verifiche empiriche controllate per avallare quelle « leggi » e per fornire previsioni, ma il dar conto *ex post* di un divenire storico che in qualche « senso » bisogna pur motivare... La differenza fondamentale fra la direzione metodologica dello scienziato sociale e dello storico è dunque che, mentre il primo va alla ricerca di « *explanantia* » verificati sperimentalmente, il secondo li pone quali validi di per sé. Tali sono le « interpretazioni generali » che Popper ammette come « provvisori parametri teorici, di regola non verificabili, utili al lavoro storico » [14]. Provvisorietà e non verificabilità che in sede storica paiono rimanere sempre tali, e che fanno della «storia esplicativa» una forma di «Filosofia» ovvero di «Filosofia della storia».

6. LA PREVISIONE COME ESIGENZA INTELLETTUALE E PRATICA. —

Non diversamente da ogni altra scienza, la sociologia ambisce a produrre invece previsioni verificabili, circa gli stati futuri dei « fatti » sociali. La conoscenza scientifica, quale oggi la intendiamo, conferisce infatti all'atto del prevedere un valore probante il cui fascino costituisce uno degli stimoli principali della ricerca, gli altri consistendo nella facoltà, concessa dalla scienza, di dominare la natura e le cose, di rispondere a domande assillanti o magari urgenti. Il fascino, oltre che la necessità, di previsioni valide e accurate, vien rispondendo inoltre alle esigenze dei tempi in cui viviamo, caratterizzati da mutamenti tecnici, sociali, culturali e politici tanto repentini, quanto tendenzialmente ansiogeni, perché appunto, scarsamente dominabili e preavvertibili.

Non è casuale, a questo proposito, che le origini della sociologia siano state caratterizzate dalla produzione delle più vaste e pretenziose opere previsionali che mai il pensiero sociale ebbe a fornire: si pensi all'opera di K. Marx e di Comte con le loro spiegazioni e preconizzazioni degli sviluppi passati e futuri dell'intera umanità o dell'umana conoscenza. Certamente quelle opere « complete ed esaurienti » non furono atto di scienza ma, se mai, una sua esasperazione sotto la pressione degli avvenimenti del tempo; furono quelle che R. König definisce giustamente « Filosofia della storia e della società » [15], forme romantiche, se pur seducenti e in certi aspetti conoscitivamente valide, che testimoniarono l'esistenza di una problematica sociale cui necessitava rispondere scientificamente. E fu in quest'ultima direzione che, insieme ai contributi degli autori su citati [16], alcuni sociologi iniziarono quella vera e propria opera di *scienza* che non può non salire dal *semplice* al *complesso* della fenomenologia sociale, senza peraltro ambire ad esaurire gli argomenti e quindi ad esaurirsi in quanto disciplina, nel breve arco di una esistenza individuale, come i colossi omnicomprensivi di cui si discorreva più sopra, pretesero fare. La vera sociologia nacque con le ricerche di F. Le Play, con le teorie verificate di E. Durkheim, con le tesi metodologiche del Weber, con le pagine migliori di V. Pareto.

7. Una disputa sulla struttura logica della previsione. —

Dall'analisi del *semplice* (ossia di un adeguato numero di casi, definiti e circoscrivibili empiricamente) può scaturire una « legge » o una serie di « leggi » che possono dar luogo a « teorie » le quali, a loro volta, porranno in essere nuove ipotesi da verificare ad altri livelli. Qui analizzeremo quale significato scientifico e quali limiti operativi rechi in sé la previsione sociologica resa possibile dalle leggi e dalle teorie, così come essa si presenta oggi negli ambienti scientifici che l'accolgono quale strumento metodologico probante sul piano conoscitivo e pragmaticamente utile sul piano operativo.

Per lo scopo espositivo che qui intendiamo perseguire, converrà allora iniziare con un'analisi logica della *previsione*, per mostrare in seguito tutta la rilevanza strategica, anche in campo previsionale, della *teoria*.

L'atto previsionale può ancora essere descritto formalisticamente rifacendoci al già visto modello detto « nomologico-inferenziale » [17].

Il doppio tratto che separa l'*explanans* dall'*explanandum* nel secondo modello sta ad indicare il carattere probabilistico di alcune previsioni scientifiche. Sarà bene tenere presente che, generalmente, in sociologia si ha a che fare con modelli di questo secondo tipo.

$$\text{I}\quad \begin{array}{cl} \left.\begin{array}{c} L_1\ L_2\ \ldots L_n \\ C_1\ C_2\ \ldots C_n \end{array}\right\} & \text{explanans} \\ \hline E & \text{explanandum} \end{array}$$

$$\text{II}\quad \begin{array}{c} L_1\ L_2\ \ldots L_n \\ C_1\ C_2\ \ldots C_n \\ \hline\hline E \end{array}$$

In esso si afferma, in definitiva, che « se sono date le leggi L_1, L_2 ...L_n e le condizioni C_1, C_2 ...C_n, allora sarà da aspettarsi il fatto E ».

Se la legge è costituita dalla proposizione «l'uomo è mortale» e la condizione è «Socrate è uomo», si capirà bene come il fatto «Socrate è mortale» sia «spiegato» da quello che, in fondo, non è che una forma di sillogismo.

Nella spiegazione di fatti ripetibili è data anche la previsione: infatti, ogni qual volta noi constatassimo (o ponessimo noi stessi in essere, con un atto tecnico) le $C_1 C_2 ... C_n$, potremmo prevedere deduttivamente o probabilisticamente il fatto E.

Fra i metodologi della scienza tuttavia non esiste un completo accordo sull'identità logica, anche qui sostenuta, del modello nomologico-inferenziale della *spiegazione* e della *previsione*. Prenderemo in considerazione il pensiero di M. Scriven, S. F. Barker e N. Rescher [18].

Secondo Scriven esistono previsioni che non si basano su precedenti spiegazioni. Quando osserviamo la diminuzione di pressione nel barometro, prevediamo il temporale senza che quest'ultimo si possa far risalire all'abbassamento della colonnina nel nostro strumento. In realtà il processo logico non ci pare diverso nel caso si intenda spiegare o prevedere il fatto: si tratta solo di diversità di livello, la struttura del ragionamentorestando la stessa. Nel caso della spiegazione, semplificata, del temporale, vale il modello A, mentre per la previsione usiamo il modello C ma, come si nota dalla congiunzione, l'*explanandum* di B non è che la condizione C_1 del modello A.

Quindi l'affermare «è scoppiato un temporale *perché* si è abbassata la colonnina», significa *spiegare* in quanto l'abbassamento della colonnina stessa sta a richiamarci i modelli A e B. In altre parole nessuna spiegazione reperita con modelli nomologici-inferenziali è definitiva, ma sottintende modelli ad altri livelli.

MODELLO A	*MODELLO B*	*MODELLO C*
L_1 (Quando si abbassa la P si ha un temporale)	L_2 (Ogni volta che la colonnina scende, la P si abbassa)	L_3 (Ogni volta che la colonnina scende si ha un temporale)
C_1 (P si abbassa) ←	C_2 (la colonnina scende)	C_3 (la colonnina scende)
E_1 si ha un temporale	E_2 la P si abbassa	E_3 si ha un temporale

Il Barker dice che a volte è possibile *spiegare*, ma non *prevedere*.

Il caso di un paziente che si ammala di polmonite e muore può essere spiegato, ma non previsto, poiché è raro che, oggi, di polmonite si muoia. Qui in fondo, non si denuncia tanto la diversità logica dei due casi (spiegare e prevedere), ma se ne mette in luce solo la differente dotazione di certezza. Possiamo allora costruire tre modelli nomologici-inferenziali, D, E, e F, in un ordine di crescente gravità della malattia: si noterà che siamo costretti a fornire l'*explanandum* di probabilità crescenti. Non solo l'esatta previsione, ma anche l'esatta spiegazione necessita di una esatta stima delle condizioni C_1 C_2 o C_3. È solo sapendo quale gravità della malattia noi abbiamo di fronte che potremo e spiegare e prevedere.

	MODELLO D		*MODELLO E*		*MODELLO F*
L_1	(la polmonite leggera è causa di morte nel 1% dei casi)	L_2	(polmonite media: si muore nel 30% dei casi)	L_3	(polmonite grave: si muore nell'80% dei casi)
C_1	(Tizio ha la polmonite leggera)	C_2	(Tizio ha la polmonite media)	C_3	(Tizio ha la polmonite grave)
Em	(Tizio muore con 1% di probabilità	Em	(Tizio muore con 30% di probabilità	Em	(Tizio muore con 80% di probabilità

Il Rescher afferma che nella spiegazione la conclusione è certa e nella previsione no. Tale certezza è però della stessa natura e misura nei due casi. Se noi conosciamo le L, ossia le leggi che governano un certo insieme di fatti e le C_n, ossia sappiamo che ciò che è prescritto nella legge accade nella realtà, possiamo *inferire* il fatto F sia che si verifichi in un tempo $T + K$ o che si sia verificato in un tempo $T - K$ rispetto al nostro tempo T. I motivi per cui la spiegazione può essere falsa se il fatto è già avvenuto, sono gli stessi per cui non riusciamo a prevederlo se ancora è da venire.

Ciò che è indiscutibile è che *un nuovo modello, più preciso, può sorgere solo da una spiegazione di un fatto nuovo oppure inatteso* (ad esempio, un paziente con la polmonite leggera che muore).

Ma una volta instauratosi questo modello nuovo si deve riconoscere che la struttura del ragionamento esplicativo è identica a quello predittivo: ambedue sono infatti possibili con lo stesso modello.

Se si comunica ad un medico che alcuni malati ($> 1\%$) di polmonite *leggera* sono morti, egli dovrà cercare la spiegazione del fatto in un modello nuovo rispetto a quello unico di cui è fornito e che assegna una probabilità infima di morire a causa della polmonite e inserirà, ad esempio, la variabile *età* del malato. Se allo stesso medico si fosse comunicato che un paziente era colpito da polmonite leggera e gli si fosse chiesta una prognosi, simmetricamente avrebbe previsto tutto fuorché la morte del soggetto, la cui morte avrebbe stimolato *nello stesso modo* il medico a riesaminare il caso e a dotarsi del nuovo modello comprendente l'età o altri fattori.

Si può concludere che sia la spiegazione che la previsione sono governate dallo stesso modello di ragionamento, che ambedue possono essere errate per carenze del modello e che, infine, un aggiornamento del modello proviene dalla spiegazione resa necessaria da un fatto nuovo avvenuto in passato oppure dal non avveramento di una previsione. Anche per il Popper « il termine « predizione » ... comprende asserzioni intorno al passato (« retrodizioni ») o anche asserzioni « date » che desideriamo spiegare (« *explicanda* ») » [19].

8. LEGGE, TEORIA E LORO EFFETTI SULLA VERIFICA DELLA PREVISIONE. — R. K. Merton sostiene, d'altra parte, che la sola legge [20] empirica non permette previsioni efficaci in quanto la correlazione che essa pone in evidenza fra un « fatto » X e uno Y può non essere esauriente. In altre parole quando il rilevamento scientifico si limitasse a fissare come « in una popolazione mista i cattolici hanno una percentuale di suicidi più bassa dei protestanti » [21], essa darebbe luogo a previsioni quanto mai vaghe, poiché la correlazione non è inserita in una « teoria ».

Merton non nega la validità di previsioni extrapolate da tendenze o leggi statistiche; sostiene però che, mancando di una sovrastante «teoria sistematica», i margini di errore delle previsioni aumentano.

Merton mostra l'utilità, per la ricerca teorica e per la ricerca empirica, di far *derivare* o di *inserire* (a seconda che, rispettivamente, su un oggetto particolare sorga prima la teoria o prima la ricerca empirica) la legge in una « teoria » del tipo di quella recuperabile da una riesposizione delle tesi indicate da E. Durkheim ne « *Il suicidio* ».

Essa si presenta allora nel modo seguente [22]:

« 1 - La coesione sociale fornisce un sostegno psichico ai membri di un gruppo che sono soggetti a tensioni ed ansietà acute.
2 - Le percentuali dei suicidi sono in funzione delle tensioni e delle ansietà non alleviate cui le persone vanno soggette.
3 - I cattolici hanno una coesione sociale maggiore dei protestanti.
4 - Perciò si dovrebbero prevedere percentuali di suicidi più basse tra i cattolici che non tra i protestanti ».

Come appare evidente, la correlazione prima caratterizzata, nella legge empirica, dal legare insieme suicidio e appartenenza religiosa, viene ora ampliandosi e contemporaneamente *precisandosi* col mostrare come il suicidio sia da collegarsi con molti tipi di stati di ansietà, i quali sono maggiormente frequenti in gruppi individualistici, e quindi fra i protestanti.

In effetti la « *teoria* » non pare *logicamente* differente dalla legge, mentre ne è sicuramente una precisazione in quanto, collegando il «fatto» a leggi più articolate ne aumenta la capacità definitoria e, insieme, la possibilità di previsione.

Riformulando e riorganizzando i punti 1, 2, 3, 4, del paradigma teoretico del suicidio, possiamo infatti ottenere i seguenti « modelli ».

Se vi è *coesione sociale* allora si ha *sostegno psicologico* dei membri di un gruppo (L_1).

I cattolici hanno grande *coesione sociale* (maggiore dei protestanti) (C_1).
I cattolici hanno grande (maggiore dei protestanti) *sostegno psicologico* (E_1).

Dalle tre proposizioni scaturisce il modello:

$$\begin{array}{c} L_1 \\ C_1 \\ \hline \hline E_1 \end{array}$$

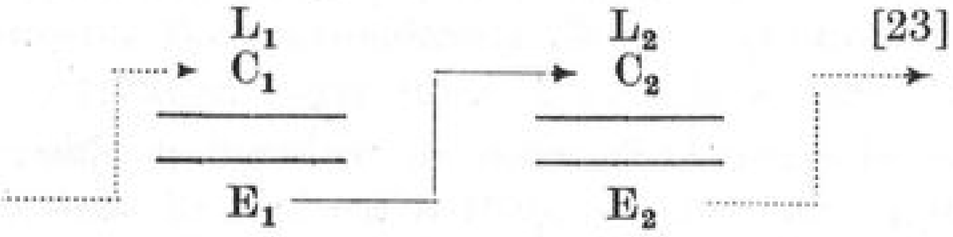
Ancora:

Se vi è *sostegno psicologico* allora si ha *bassa propensione* al suicidio (L_2).
I cattolici hanno grande *sostegno psicologico* (maggiore dei protestanti) (C_2)
I cattolici hanno bassa (minore dei protestanti) *propensione* al suicidio (E_2).

Il modello è ora:

$$\begin{array}{c} L_2 \\ C_2 \\ \hline \hline E_2 \end{array}$$

È immediato ora il collegamento fra i due modelli:



dove si vede come l'*explanandum* del primo diviene *condizione* del secondo. Il collegamento logico di due leggi vertenti su due oggetti relativamente distanti fra di loro, permette di *precisare* teoreticamente una legge empirica che comprendeva unicamente le categorie osservative di « cattolico (o protestante) » e « suicida » (o non suicida).

Così, uno schema causale se inizialmente era della forma:
essere cattolici → essere poco propensi al suicidio
ora diviene
essere cattolici → essere coesi → essere sostenuti psic. → essere poco propensi al suicidio

L'aumentata *precisione* (cosa questa indispensabile per ottenere una aumentata capacità previsionale) è palese.

Se infatti chiamiamo « p » la probabilità che un cattolico si suicidi, essa diverrà, poniamo, « p-k » per un qualsiasi individuo (anche non cattolico) che viva in un gruppo coeso; diverrà poi « p-k-z » per un individuo (anche non vivente in gruppo coeso), ma comunque sostenuto psicologicamente da qualcosa o qualcuno.

In definitiva, mentre chi è « sostenuto psicologicamente » sfugge maggiormente al suicidio, il cattolico *in quanto tale* può cadervi più spesso, perchè non è sempre detto che si dia il legame fra il suo stato di cattolico, la coesione e il sostegno psicologico. Una previsione operata sul dato « Tizio è cattolico » sarà sempre meno precisa che non nel caso si fosse certi, indipendentemente dalla religione di appartenenza, che Tizio è sostenuto psicologicamente da un ideale, da una persona ecc.

Compito della teoria è dunque il precisare, col collegarle, le leggi empiriche, dando nel contempo luogo eventualmente a nuove leggi o ipotesi di leggi. Infatti con ciò si stimola il procedere della scienza e si permettono previsioni e quindi interventi operativi sulla realtà, più accurati e definiti.

In ogni caso le previsioni circa il verificarsi di « fatti » che *siano* soggetti a ripetibilità, assumono, nella metodologia scientifica ordinaria, il significato di una estrapolazione da leggi, le teorie essendo esse stesse, come mostrato, insiemi di leggi.

Caso diverso è quello in cui non si abbia a che fare con « fatti » ripetibili, bensì unici e irripetibili.

Mentre nel campo dei « fatti » ripetibili anche una ipotesi estemporanea non legata a leggi e teorie non sorge che nella speranza, anzi nella supposizione, che leggi e teorie sull'argomento si possano reperire, nel campo dei « fatti » irripetibili non essendo possibile alcuna generalizzazione, la previsione non può contare che su quella sorta di metodologia straordinaria che si può definire intuizione geniale: destinata (peraltro) a rimanere tale ed a morire insieme col determinarsi (o il non determinarsi) del « fatto » previsto.

Se per « scienza » intendiamo quella procedura conoscitiva che garantisce la propria validità reperendo leggi e teorie generalizzanti e verificabili, dobbiamo ammettere che di fronte ad un mondo di fatti irripetibili, non è possibile alcuna opera scientifica, ma solo un accostamento qualitativo di « fatto » a « fatto » nel tentativo di riuscire convincenti. Un tale procedere sarebbe in certo modo regolato da modelli inferenziali del tipo di quello introdotto dalla scuola stoica (il sillogismo ipotetico) che deriva il particolare dal particolare.

9. LA RINUNCIA ALLE VERITÀ ULTIME O VISSUTE IN SOCIOLOGIA.

— Ora, per quanto riguarda la sociologia, i suoi fondamenti e le sue possibilità di essere scienza, problematico è stato ed è anche oggi per certi versi, stabilire se essa abbia a che fare con « fatti » ripetibili o non piuttosto irripetibili.

Così per la scuola storicista in genere e per il Dilthey, Windelband, Rickert e Freyer in particolare, con varie accentuazioni e terminologie, quasi sempre ontologizzanti, si intese porre una distinzione fra scienze *individualizzanti* (dell'« irripetibile ») e del *generalizzante* (del « ripetibile ») (Rickert). La sociologia farebbe parte, secondo questa corrente, della prima classe. Con la conseguenza, fra le altre, di non poter dar luogo a previsioni che si possano dire tratte da leggi generali, ma solo a interpretazioni intuitive, comprendenti o auto-comprendenti (Freyer).

La tesi che vuole la sociologia « scienza » dell'individuale, risente in ultima analisi di una impostazione metafisica che pretende assegnare al comportamento dell'uomo e dei gruppi una *essenzialità* e una *proprietà* che potrebbe essere ricollegata alle « sostanze » aristoteliche o ai « noumeni » kantiani, mentre la scienza sociologica, come ogni altra a carattere logico-sperimentale non tende ad altro che ad indagare e sistemare gli aspetti *fenomenici* del comportamento umano, convenzionalmente oggettivabili. La tesi della sociologia individualizzante, mostra di non essere disposta ad accettare il carattere pragmatico e convenzionale della scienza, perdurando in un vano perseguimento di forme di conoscenza che colgano *l'intima essenza* delle cose, rinunciando nel frattempo a riconoscere la sociologia empirica degna di essere definita scienza.

In realtà, la sociologia empirica (attraverso il pur scarso lavoro empirico e teorico fin qui svolto con successo) pare potersi decisamente allineare sullo stesso piano metodologico delle altre scienze riconosciute, mentre lo storicismo, l'intuizionismo, il partecipazionismo non sembrano differenziarsi ed emanciparsi da una pregnanza filosofica inevitabilmente soggettivistica e tendenzialmente scettica.

La tesi che viene qui accolta è quella che assegna alla sociologia la qualifica di scienza empirica generalizzante, e come tale capace di fornire *leggi* e *teorie* circa il modo di organizzarsi dei « fatti » sociali, senza che il significato esistenziale che questi ultimi assumono nei confronti del sociologo ne deformi l'oggettività dell'analisi e della interpretazione.

Non si accetta qui allora la tesi di chi intende con « conoscenza sociologica » esclusivamente una forma di partecipazione dall'interno di un gruppo o di una società. È chiaro che la conoscenza perseguita in questa prospettiva, oltre tutto sarebbe assolutamente priva della capacità di essere comunicata efficacemente ad altri, se non ad un livello relativamente superficiale. Se infatti il ricercatore ritiene di doversi staccare dal proprio mondo per immergersi in quello che intende studiare, quando vorrà riferire ad altri ciò che ha conosciuto troverà nel farlo la stessa difficoltà che avrebbe incontrato nel cercare di capire il mondo altrui restando attaccato al proprio. Gli stessi antropologi culturali, che pur accettano largamente la metodologia partecipante, si son provati ad affrontare la necessità di rendere comunicabili, concetti, categorie e definizioni analitico-partecipative. Così è dei « modelli culturali » della Benedict, dei « temi culturali » di Opler e della « personalità di base » di Linton, della « struttura » in Levi-Strauss, Radcliffe - Brown, ecc. Spesso però il loro tentativo è stato vanificato da implicazioni ontologiche e dalla aleatorietà degli indicatori. La partecipazione a nostro avviso può essere solo considerata una fonte di ipotesi.

10. EFFETTI SOCIALI DELLA PREVISIONE. — Ma se nella fase dello studio positivo dei « fatti » sociali, la problematica generale e i metodi concedono allo scienziato sociale di operare, pur fra ostacoli di varia origine, con una efficacia conoscitiva analoga a quella perseguibile in altre discipline scientifiche, problemi assolutamente peculiari, sorgono semmai allorché il sociologo fornisce delle previsioni. L'impatto di una previsione sociologica sul pubblico, può mettere infatti in moto processi di reazione di quest'ultimo nei confronti del « fatto » previsto, modificando l'andamento degli avvenimenti o le probabilità di verifica della previsione stessa. Se la previsione pubblicata recita « se si avrà A allora avverrà B », il pubblico, a seconda del *valore* che per esso assume il « fatto » B, accelererà o rallenterà il realizzarsi di A e, di conseguenza, l'insorgere di B; oppure, dato che A fosse inevitabile, potrebbe agire in modo di aiutare il processo causale che lo lega a B, o di ostacolarne il compimento facendo leva sugli stessi fattori che rendono ogni previsione probabilistica. Può valere il seguente esempio puramente ipotetico. Supponiamo che in America venisse pubblicata la seguente previsione: « poichè il reddito aumenterà per tutti gli americani, i negri aumenteranno la propria cultura e il proprio prestigio personale ».

L'impatto della previsione [24] sul pubblico bianco (che dipingeremo, per la bisogna, totalmente conservatore e razzista) sortirà le seguenti possibili reazioni:

1) I bianchi faranno in modo da rallentare il tasso di sviluppo del reddito generale americano, per non correre il rischio di vedersi sopraffatti dai negri. Agiranno cioè sulla causa A affinchè non si realizzi l'effetto B.

2) I bianchi lasceranno aumentare il tasso di sviluppo del reddito, cercando però in ogni modo di tagliare fuori dal suo godimento i negri. Essi agiranno ancora sulla causa A, deformandone il contenuto che, nella previsione, voleva il reddito aumentato « *erga omnes* ».

3) I bianchi lasceranno aumentare *erga omnes* il tasso di crescita del reddito, cercando però in ogni modo di distogliere i negri da un uso « culturale » del nuovo reddito, dirottandolo verso modelli consumistici, edonistici ecc. Essi agiranno, in questo caso, in modo da impedire il compimento causale che fa seguire B ad A, incrementando il numero di negri che (cosa già prevista in misura minore nella previsione, poichè tratta da una *legge* probabilistica del tipo « su cento negri che usufruiscono di un grosso incremento di reddito, novanta « domandano cultura ») di per sé avrebbero richiesto più automobili e televisori al posto di libri o di istruzione.

Ognuna delle tre possibili alternative rappresenta processi particolari del più generale processo definito da R. K. Merton « la profezia che distrugge se stessa » (« self stultifying prediction ») [25].

Sarà d'altra parte opportuno specificare che l'accadimento di predizioni, in senso generale « autoalimentantisi », sembra non essere esclusivamente legato alle conseguenze di « previsioni scientifiche pubblicate » bensì, a ben vedere, ad ogni forma di previsione capace di riscuotere fiducia e di sollecitare, in un senso o nell'altro, l'interesse emotivo del pubblico.

In tal modo il « teorema di Thomas » [26] è valido non solo per la classe delle previsioni scientifiche, ma anche per le *profezie* propriamente dette o per le *dicerie*: si pensi alle vicende della Last National Bank del 1932 o alla *caccia all'untore* di manzoniana memoria.

In effetti, ciò che distingue, da un punto di vista metodologico, i processi di autodistruzione (o di autoadempimento) scaturiti da *dicerie* o *profezie*, dai processi di autoadempimento (o autodistruzione) scaturiti da studi scientifici, consiste nel fatto che, mentre gli studi scientifici daranno luogo a previsioni solo se si *sia verificata in precedenza, ovvero in sede sperimentale, una legge oppure una tendenza dei fatti organizzati in un certo modo, verso certi obiettivi*, le *dicerie* e le *profezie* ne prescindono o, quanto meno, non ne dipendono necessariamente. Può essere di nuovo utile, a questo proposito, il già citato esempio portato da Durkheim, in polemica col pragmatismo. Se prescindiamo dalle *previsioni scientifiche ipotetiche* (ossia non tratte da teorie e leggi verificate), troveremo che qualsivoglia previsione scientifica non può che essere tratta dallo stesso modello nomologico-inferenziale dianzi ricordato. Ed è infatti in questa direzione che ci pare si debba interpretare ciò che Ernst Nagel [27] intende quando afferma che la legge che prevede il decesso di chi si sottoponga a un certo gas non cessa di essere valida, per il solo fatto di aver ingenerato tanto timore che ognuno si guarda bene dal sottoporsi alla sua azione.

L'azione del gas venefico era stata verificata in precedenza. Se riusciamo in definitiva a concedere che la sociologia approdi a generalizzazioni empiriche e a teorie così come altre scienze, nulla impedirà di attribuire alle previsioni che essa fornirà lo stesso carattere della previsione circa l'azione di un gas venefico.

Per converso, è certamente legittimo asserire che l'autoadempimento (o l'autodistruzione) di *dicerie* e *profezie* comporti il raggiungimento fattuale di una certa realtà in esse dichiarata, ma ciò che non è legittimo è trarne la conclusione che dovevasi trattare di previsione scientifica per il solo fatto che la *prassi* vi si è adeguata. Un tale modo di agire corrisponderebbe all'esempio citato da Merton il quale, richiamandosi all'operato di chi *pone in essere* (non « prevede ») una certa realtà giustificandola « scientificamente », afferma argutamente: « Avendo visto mettere il coniglio nel cappello... possiamo poi guardare soltanto con sufficienza l'aria trionfante di chi alla fine lo tira fuori » [28].

Basilare è dunque la necessità di distinguere fra *previsioni* scientifiche che, rilevato un *trend* nella realtà lo proiettano nel futuro, e *programmi d'azione* che, prendano o non prendano le mosse da *trends* verificati, perseguono uno scopo non predittivo, ma acquisitivo. È la consueta distinzione fra il *sein* e il *sollen*, fra l'essere e il dover essere, fra scienza e tecnica (o politica).

Alcuni propongono altresì delle *tecniche* indirizzate ad annullare l'effetto delle reazioni del pubblico, tutte fondate sul tentativo di inglobare nella previsione anche le reazioni in modo da fornire una previsione che, pubblicata, non ne provochi più alcuna. In sostanza si tratterebbe di allargare il laboratorio ampliandone la comprensività.

Sulla scia del Simon, [29] si può affermare che, dati due candidati A e B ad una elezione è possibile annullare gli effetti che potrebbero insorgere dalla pubblicazione di una previsione che desse A per vincente e B per perdente in quanto, sapendo che gli incerti voterebbero per il perdente e sapendo del sufficiente peso numerico degli incerti per determinare il risultato finale, si potrebbe in questo caso prevedere la vittoria di B. Pare ad ogni modo evidente che a questo punto si sia daccapo: infatti una nuova pubblicazione della previsione corretta sortirebbe una nuova reazione positiva nei confronti di A, dato ora per perdente. È quindi da concludersi che, se la previsione originaria era la vittoria di A, dopo questa ultima pubblicazione essa effettivamente si realizzerà. Ora il punto è questo: è chiaro che se il ricercatore avesse tenuto per sé la prima previsione, A avrebbe vinto, ma è anche chiaro che pubblicandola come tale avrebbe contribuito a determinare la vittoria di B. Ci si chiede a questo punto se una pubblicazione corretta all'uopo, non sia un tentativo tecnico « per far volgere » la vittoria ad A come voleva il *trend* originario. La questione può sembrare oziosa, ma in realtà essa può nascondere una delicata questione logica e deontologica. Possiamo distinguere due casi: il primo in cui il ricercatore è solo a conoscenza dei *trends*, ossia di tendenze sociali T verso il compimento di un certo fatto F, e il secondo in cui il ricercatore conosce anche le modalità di reazione che il pubblico attuerebbe quando venisse messo a conoscenza del fatto che le sue azioni lo stanno portando a F. Supponiamo che queste reazioni siano negative. Nel primo caso la previsione sortirà una controreazione e F non si realizzerà.

Ma nel secondo, se il ricercatore conclusivamente enunciasse solo che « il fatto F non si realizzerà », esso potrebbe invece realizzarsi, poiché il pubblico non saprebbe che F è conseguenza di T. Pare dunque che, per non cadere nella *predeterminazione tecnica, ossia in una manipolazione* anziché in una previsione scientifica, in un modo o nell'altro la previsione originaria vada sempre dichiarata. Ora, pur rimanendo questo un problema aperto, noi siamo del parere che anche nell'ultimo caso descritto, l'aggiunta, nella previsione, dei modi conosciuti di reazione del pubblico, sia ancora manipolatoria, dato che potrebbe trasformarsi in un consiglio, in uno stimolo ad un comportamento anziché limitarsi a prevederlo. Neurath, che parla addirittura di «situazioni confuse» che si pongono in questi casi al sociologo, afferma infatti: « Ad esempio, uno scienziato può prevedere che in un certo posto cadrà una meteorite e che resteranno uccise alcune persone. Egli potrebbe pubblicare questa previsione; ma in questo caso la popolazione si allontanerebbe e quindi la previsione non risulterebbe in armonia con le future proposizioni-d'osservazione fatte per saggiarla. Supponendo che egli scrivesse la previsione soltanto nel suo taccuino, allora potrebbe forse avere successo in questa. Se poi preferisse dire che la popolazione se ne sarà andata quando cadrà la meteorite, allora la previsione sarebbe contradetta ancora una volta dalle proposizioni-d'osservazione perchè quelli, pieni di diffidenza, pensando che lo scienziato li voglia far muovere nel suo proprio interesse, potrebbero rimanere dove si trovano » [30].

Per concludere, a nostro avviso, quando un tecnico, riprendendo una legge ormai verificata in sede scientifica, la *applica*, non fa altro che porre premeditatamente in essere le condizioni prescritte, in modo tale che la previsione si avveri. Ma in sociologia fare questo è non solo fare della tecnica, ma anche della politica, il che ci porta lontano dalla conoscenza scientifica. Inoltre, aldilà del carattere manipolatorio dell'azione politica, la tecnica e la politica come abbiamo detto all'inizio del saggio, non aggiungono alcuna conoscenza scientifica (se non indirettamente) a quella consolidata *già prima* dalla legge da cui esse sono rese possibili.

Lo stesso Nagel, per recuperare la predicibilità dei fatti sociali, ripiega sulla validità della legge da cui si trae la previsione: «un enunciato il quale aspiri ad essere una legge è, quanto alla forma logica, un condizionale...»; e esso è valido nei termini in cui è espresso: «*se* certe condizioni vengono soddisfatte *allora* certe altre cose vengono pure realizzate... » [31].

Il valore, a questo punto eccessivo, attribuito all'avveramento della previsione sembra derivare da una cultura deweyana che accentua a dismisura l'importanza della verità pragmaticamente conseguita.

Noi siamo del parere che il lavoro scientifico-sociologico cessi nel momento in cui viene pubblicato nella sua versione originaria, e che del suo destino dopo questo momento non ci si debba preoccupare se non ponendovi nuova attenzione per nuovi lavori di ricerca e lasciando alla società e ai politici la decisione di porre o non porre in essere le condizioni necessarie per verificare ulteriormente la legge da cui è tratta la previsione.

11. ULTERIORI PROBLEMI CONNESSI CON LA FONDAZIONE DELLA SOCIOLOGIA. — Secondo A. Comte scopo della sociologia è « l'investigazione delle leggi di azione e reazione delle diverse parti del sistema sociale » [32].

Per H. Spencer la sociologia deve spiegare come « generazioni successive d'individui sono prodotte, allevate e rese atte alla cooperazione » [33]. E. Durkheim ritiene da parte sua che la sociologia debba perseguire i legami fra le cause e gli effetti della fenomenologia sociale, precisando inoltre che l'effetto « trae dalla causa la propria energia, ma all'occorrenza gliela restituisce e quindi non può scomparire senza che essa ne risenta la mancanza » [34]. La tesi di M. Weber è, infine, che

« la sociologia ... deve designare una scienza la quale si propone di intendere in virtù di un procedimento interpretativo l'agire sociale, e quindi di spiegarlo causalmente nel suo corso e nei suoi effetti » [35].

Le quattro citazioni non hanno un mero significato antologico, ma sono sintomatiche dei quattro fondamentali indirizzi che la sociologia ha seguito dalle sue fondazioni a tutt'oggi. In questi indirizzi si manifesta in altri termini una sorta di universo delle concezioni epistemologiche che furono e sono per certi versi ancora alla base delle scuole sociologiche destinate, più di altre, a lasciar traccia di sé e, anzi, a promuovere ben precisi e spesso contrastanti sviluppi teorici e di ricerca.

Nella posizione di Comte ritroviamo, esplicitamente, l'impronta inconfondibile del positivista che aspira a reperire in sociologia rigide leggi sociali, così come il suo secolo partoriva in ogni scienza, naturale. Nell'altro grande positivista, H. Spencer, il concetto di sistema, tipico delle scienze fisiche viene reinterpretato in chiave organicistica e le « parti » del sistema vengono ora concepite quali *organi del corpo sociale*.

Il positivismo di Durkheim pone inoltre le basi di una particolare metodologia sociologica che tien conto della necessità di reperire leggi, cause ed effetti e che nel contempo ponga in luce gli aspetti funzionali (che implicano di nuovo una visione in qualche modo organica della società) dei fatti sociali, non solo aprendo così la strada alla tradizione strutturale-funzionale in sociologia, ma ponendosi anche a fianco della visione antropologico-culturale.

Il pensiero di Max Weber testimonia dal canto suo una convinzione epistemologica che trova ancor oggi molto credito e che vuole le azioni dell'uomo spiegabili nelle loro motivazioni, attraverso uno sforzo di comprensione storico-situazionale reso possibile dal carattere intenzionale dell'umano agire. Tale metodo che Weber vuole comunque destinato a fornire verità generali che « il sapere empirico soltanto può darci » è limitato dal non essere applicabile alla sfera delle motivazioni non consapevoli che, per il Weber, costringerebbero le scienze sociali ad inquadrarsi « nello stato di scienze naturali dell'agire sociale; mentre in realtà, essendo rivolte all'agire intenzionale, non possono essere altro che scienze dello spirito nomologiche » [36].

In definitiva, sul piano del metodo assunto dalle varie impostazioni ora viste, appare evidente, come è già stato messo in rilievo in altra parte di questo lavoro, come, sia il positivismo, che l'evoluzionismo sociale, il positivismo funzionalistico e lo storicismo aspirino ad acquisire uniformità (leggi) che leghino fra loro i fatti e, in tal modo, descrivano e spieghino la fenomenologia sociale.

In effetti ognuna delle scuole metodologiche, passate ora in rapida rassegna, differisce dalle altre per le pretese teoretiche che si sovrappongono al fine centrale della sociologia di cui erano espressioni, pretese che, spesso senza la necessaria gradualità, tendevano a dare una portata e un senso esauriente, completo e quindi potenzialmente chiuso ad uniformità (leggi) reperite o date per assioma. I risultati furono generalmente (eccezion fatta, in certo modo, per il Weber) opere architettoniche autosufficienti da cui fu possibile estrarre le parti metodologicamente e teoreticamente migliori, solo a patto di ridefinirle secondo una dotazione concettuale nuova, convenzionale e sicuramente oggettiva.

Non è a caso che R. K. Merton, che particolarmente insiste, nella sua opera, sulla necessità di procedere in sociologia fondando i propri passi da un lato sul metodo scientifico (logico-sperimentale) e dall'altro su pretese teoretiche graduali (*middle range-theories*), ha posto in introduzione al suo libro fondamentale la notissima massima di Whitehead: «Una scienza che esita a dimenticare i suoi fondatori è perduta».

In definitiva abbiamo mostrato, fin qui, come ogni qualsivoglia approccio alla realtà (nel nostro caso sociale) pretende sempre di assurgere a conoscenza generale, ossia a descrizioni, spiegazioni, previsioni generali o oggettive. Inoltre, anche se in modo spesso inespresso, è pur sempre implicita in ogni forma di pensiero sociale la convinzione che, per dar credito ad una certa uniformità, o legge, sia necessario studiare un certo numero di casi, riconoscendo con ciò che la sola base adeguata per costruzioni nomologiche veritiere è un procedimento di natura sperimentale e induttiva. Da qui a riconoscere finalmente il valore fondamentale della statistica (ovvero della matematica applicata a fenomeni ripetibili) il passo è breve. Come nota il Lazarsfeld, lo stesso «Max Weber fu in certi periodi entusiasta della quantificazione, eseguendo numerosi computi lui stesso; Tönnies inventò un proprio coefficiente di correlazione; durante le vacanze pasquali, von Wiese conduceva regolarmente i suoi studenti nei villaggi, affinchè essi potessero vedere i suoi concetti sulle relazioni sociali, posti in atto nelle famiglie contadine» [37]. E l'elenco potrebbe ovviamente continuare, comprendendo fra gli altri lo stesso Comte [38], il Durkheim de «*Le suicide*», Le Play ecc.

L'ostacolo che si oppone a questo punto all'accettazione di un rigoroso metodo scientifico, che non può non essere, in sociologia fondamentalmente identico alle altre scienze empiriche se si attendono da essa le stesse *garanzie di validità*, sembra risiedere unicamente da un lato nella difficoltà, ritenuta da molti ancor oggi invalicabile, di *definire soddisfacentemente e oggettivamente*, i concetti sociologici e dall'altro, nel timore di dar luogo a fredde, se pur valide, interpretazioni della realtà così come è data, accostata senza spirito critico, ma piuttosto con intenti, anche inconsapevoli, di conservazione politica.

12. L'USO DELLE DEFINIZIONI IN SOCIOLOGIA.

— Il primo punto coinvolge problemi per la soluzione dei quali è necessaria una disposizione del sociologo a *rendere* oggettive con procedura convenzionale le definizioni degli oggetti sociologici, rinunciando alla pretesa metafisica di *scoprire* un'oggettività essenziale ritenuta propria degli oggetti stessi.

In tal modo si vede come le definizioni scientifiche giuochino un ruolo insostituibile nel procedere di una qualsiasi scienza: i termini linguistici che designano oggetti scientifici devono avere un significato omogeneo per tutto il corpo degli scienziati, e il modo più semplice per ottenere ciò è di far ricorso a *definizioni operative,* ossia a enunciati che indichino fenomenicamente gli oggetti e i fatti in guisa tale che sia possibile evidenziarne direttamente o indirettamente, ma comunque empiricamente, la consistenza o darne una misura. Le definizioni non operative, ossia sganciate dalla responsabilità immediata di una loro verifica, sono valide solo a patto che si presupponga di poter approntare una procedura di oggettivazione empirica, ossia di riduzione a definizione operativa; la loro introduzione, in qualità di « costrutti ipotetici », è resa a volte necessaria dal carattere incompleto di una definizione o di una legge che mostrino un alto grado di validità, eccetto che per un certo aspetto che rimane lacunoso, e che viene dunque colmato con questo tipo di definizione, provvisoria e accessoria. Quando ciò non avviene e si fonda tutta la definizione, l'ipotesi, la legge su una definizione non operativa, si ricade in un ambito lontano dal procedere scientifico, per finire in quello metafisico. Secondo il Blalock, « gli scienziati pensano mediante il linguaggio teorico e verificano le loro ipotesi nel linguaggio operativo. Non è strettamente necessario trovare le operazioni corrispondenti a ciascun concetto teorico, ma è importante controllare che concetti non definiti in modo operativo non si insinuino in affermazioni che si vuole siano verificabili.

Se questo avviene, i problemi contenuti nelle « ipotesi » saranno generalmente privi di significato e suscettibili di dare origine a dibattiti senza fine » [39].

Un esempio di definizione teorica può essere la seguente, tratta da Benoit-Smullyan [40], il quale parla di *status* quale « posizione relativa all'interno di una gerarchia ». In quanto tale, la definizione appena riportata non costituisce uno strumento utile per la ricerca scientifica, mentre « l'indice delle caratteristiche di *status* (ISC) di Warner e la « scala dello spazio di soggiorno » di Chapin sono esempi di procedimenti usati per ricavare definizioni operative dello *status* sociale » [41]. È facile notare anche in questa posizione come il concetto di *status*, mentre di per sé è un concetto metodologico e non ontologico, sia reso oggettivo riconducendolo a operazioni empiriche in cui dunque ritorna in giuoco il concetto di « dato » e non di « esperienze semplici », o di « proposizioni indecomponibili », che implicherebbero una realtà ultima. Un concetto sociologico, in altri termini, è validamente definito in via ordinaria se è evidenziabile attraverso indicatori empirici.

13. Il problema dei valori. — Il secondo ostacolo cui si accennava è l'accusa di « acriticità », rivolta alla sociologia logico-sperimentale,

che finirebbe con lo sconfinare nella conservazione politica dello *status quo*. Secondo Mills questa aspirazione della sociologia contemporanea sfocia in un *empirismo astratto* [42] destinato ad essere strumentalizzato politicamente. La stessa posizione è comune ad altri autori di estrazione più o meno marxista, quali Lucien Goldmann [43], Noam Chomsky, Herbert Marcuse. Gunnar Myrdal, da parte sua, sentenzia: « Abbiamo bisogno di punti di vista ed essi presumono valutazioni. Una scienza sociale disinteressata è da questo punto di vista un puro non senso. Essa non è mai esistita né mai esisterà » [44].

In Italia, Antonio Santucci, nella sua introduzione ad Habermas, [45] riprendendo uno stile ricorrente, condanna la sociologia, della forma che noi sosteniamo, a rendersi « omogenea a una prassi che gestisce tecnicamente l'esistente », mentre il Bagolini [46] sancisce come « sarebbe facile fare esempi di situazioni nelle quali le valutazioni e le scelte dei fini predeterminano le direttive e le conclusioni delle ricerche sociologiche empiriche, e le predeterminano o in senso conservatore per legittimare il potere connesso a gruppi di interessi già costituiti e prevalenti o viceversa in senso progressivo e rivoluzionario ».

In ognuna delle posizioni è presente una convinzione di base circa l'ineluttabilità del condizionamento da parte dei valori oppure, nel caso l'evitarli fosse possibile, circa l'inutilità o anzi il servilismo conservatore che ne deriverebbe, oppure, ancora circa l'esistenza di ricerche sociologiche ammaestrate « *ad finem* » che dimostrerebbero l'impossibilità di ricerche obbiettive.

Il nostro parere è che l'uso del metodo logico-sperimentale consente una forma di conoscenza sociologica scientificamente garantita.

L'affermazione secondo cui i giudizi di valore non sono evitabili in quanto, come voleva Freyer, con la sociologia la società studia se stessa, oppure in quanto si ha necessità comunque di «punti di vista», ci pare priva di fondamento reale, se intesa oltre certi limiti. Ciò che conta è di intendersi sul significato da attribuire al termine neutralità. Secondo noi neutralità non significa cogliere la *natura* delle cose, ossia quella oggettività essenziale intima e unica degli oggetti del mondo reale che sarebbe da alcuni percepibile come vera e da altri, meno fortunati, deformata. Parlare della natura di un oggetto secondo noi non ha senso scientifico, ma metafisico: ciò che noi intendiamo scientificamente per « oggetto » non è altro o di più che la sua definizione concettuale empiricamente fondata. In sostanza si tratta di convincersi che l'oggettività (l'« attenzione indifferente » di Freud e la « *wertfreiheit* » di Weber) si raggiunge non sul piano del reale, ma sul piano del convenzionale, ossia sul piano della formalizzazione dell'empirico.

L'accordo fra gli uomini, ossia l'oggettività, la si raggiunge allora accogliendo quale oggettivo il rapporto convenzionale semantico fra oggetti e termini che li designano. Gli oggetti scientifici hanno una definizione che ne limita, in varia misura, l'estensione e i connotati d'origine osservativa, in modo tale che intanto sono verificabili in quanto si osservi nella realtà l'esistenza dei caratteri previsti nella definizione. L'osservazione, a sua volta, sembrerebbe ricadere sotto il pericolo del condizionamento descritto da parte dei valori, ma ciò sarà tanto meno probabile quanto più precisa sarà la definizione stessa. Per ultimo, l'inserimento dell'azione dello scienziato nelle procedure del metodo logico-sperimentale pare non lasciare dubbi sulla garanzia di oggettività che quest'ultimo gli impone: il criterio della verifica è infatti canonizzato in un processo di induzione-deduzione formalizzate che si compiono in una serie di atti chiaramente neutrali quali i passaggi logico-matematici e statistici oggi disponibili.

In conclusione, sostenere l'impossibilità di mantenersi neutrali nel concettualizzare e nel condurre una ricerca sociologica con metodo logico-sperimentale, ci pare assurdo quanto l'affermare che la fisica o la chimica (non il loro uso da parte dell'uomo) siano insiemi di conoscenze viziate. La realtà è che esse, come la sociologia quando si sforza di seguirle per quanto riguarda il metodo, sono le forme di conoscenza più valide che mai in nessun altro modo sia dato raggiungere sugli stessi argomenti. Infatti, oltre alle brevi considerazioni fin qui svolte, v'è da notare che qualora ci si dovesse convincere che i giudizi di valore sono ineliminabili, non vi sarebbe comunque per la sociologia alcuna alternativa scientifica: accettare i valori nella ricerca, compiendo anzi ogni sforzo per immedesimarsi il più possibile nel proprio oggetto di studio (gruppo, società, comunità ecc.), significa abbandonarsi ad una vana autoriflessione da cui il sociologo non trae che una verità esistenziale, ma non scientifica. Un tipo di approccio di carattere più antropologico che sociologico mostra di non apprezzare, ad esempio, l'uso esclusivo di questionari, ma li vuole sostituiti (nel caso di una ricerca sul lavoro) da una « vita aziendale » vissuta interamente. Noi riteniamo al contrario che una simile metodologia esclusivamente « partecipante » sia, pur nell'interesse che può suscitare, la meno auspicabile se pretende di essere qualcosa di più che un modo di costruire ipotesi.

Infatti:

1) chi può dire *quanto a lungo* si debba vivere in un'impresa per poter alla fine dire di conoscerne la vita? e 2) quanto si deve essere « *interni* »? Ancora 3) quando si stende il rapporto, *quale pensiero si esprime*? quello proprio, quello degli operai, dei tecnici, dei dirigenti?

14. NEUTRALITÀ DELLA SCIENZA, NON DEGLI SCIENZIATI. — Arrivando ora al punto centrale dell'accusa di « conservatorismo », possiamo dire che, a nostro avviso, un sociologo di scuola logico-sperimentale è, nella sua opera, tanto conservatore quanto lo può essere un fisico che indaghi i processi subatomici o uno psicologo che studi i processi di apprendimento nel bambino.

Indubbiamente, in quanto forme di conoscenza e non di intervento sulla realtà, tutte le scienze empiriche generalmente non modificano e non devono modificare alcunché del loro oggetto, ma non si capisce perché ad un analista della società che intenda scoprirne le leggi o le tendenze attuali per consegnarle poi ai politici o per farne oggetto di una sua personale forma di intervento extrascientifico teso a modificarle, debba essere mossa l'accusa di conservatorismo.

L'esistente ha un suo carattere problematico che non può essere saltato a piè pari per il timore che, studiandolo scientificamente, gli si possa attribuire una dignità e una validità che secondo un giudizio morale gli si rifiuterebbe. Molto spesso, sono proprio gli atteggiamenti di critica metodologica, i pregiudizi, i dogmi « di scuola » a determinare una visione del reale molto più viziata sul piano conoscitivo di quanto, chi se ne fa portatore, potrebbe ottenere dopo un più approfondito esame scientifico.

Un'accusa più pertinente è quella secondo cui spesso gli studi sono promossi da interessi particolari. C'è solo da osservare che questa accusa tende a colpire, a ben vedere, più i sociologi che la sociologia. Non si può infatti imputare al metodo di una scienza (che rimane comunque valido) di essere implicitamente conservatore o asservito: il fatto è che gli interessi di parte, ma, certamente, anche tutti i bisogni esistenziali, possono essere soddisfatti solo con l'ausilio di conoscenze genuine e non con dottrine dogmatiche o politicizzate. Se durante il periodo fascista e nazista hanno ricevuto grande impulso le scienze genetiche a svantaggio delle teorie psico-sociali e ambientalistiche, o se nel periodo attuale, e non solo nei paesi collettivisti, sembra accadere esattamente il contrario, ciò non significa che le conoscenze (verificate) raggiunte in due climi di condizionamento culturale di tal fatta siano pseudoconoscenze: la psicologia ha fatto passi in avanti sia con lo studio dei cromosomi che con le scoperte derivate dalle teorie ambientalistiche e psico-sociali. Non è quindi un problema di « verità »: ma di auspicare un equilibrio degli indirizzi di ricerca.

Come sarà visto più avanti, i valori culturali agiscono unicamente a due livelli fondamentali della ricerca sociologica, sia empirica che teorica: la scelta degli oggetti di studio (problemi) e l'elaborazione delle ipotesi (o dei collegamenti fra leggi o teorie). In definitiva, come i rimorsi di coscienza di Oppenheimer e degli scienziati atomici in genere non riguardano la fisica, ma i suoi rapporti, mediati dai fisici con la società, così il fiorire di ricerche sui piccoli gruppi o sulla mobilità sociale nelle società industriali possono ben essere espressione di una politica generale della scienza che favorisce maggiormente studi di questo tipo, che non studi centrati su problemi forse più gravi, quali le società sottosviluppate, ma ciò non toglie che le conoscenze che così vengono raggiunte sono conoscenze perfettamente valide sotto il profilo conoscitivo. In definitiva, se è certamente vero che un metodo sociologico di tipo « critico » o « marxista » si oppone all'esistente, consistendo essenzialmente in ciò il suo fascino e la sua portata, non è assolutamente scontato che il metodo logico-sperimentale *in quanto tale* ne prenda le difese.

15. LA PRESSIONE DEI VALORI NELLA COSTRUZIONE DELLE IPOTESI E NELLA SCELTA DEI PROBLEMI. — Come abbiamo detto, è sulla formulazione dell'ipotesi che può altresì aversi una deformazione di una eventuale « pura » intuizione scientifica, da parte di pregiudizi, ideologie o bisogni particolari: un ricercatore razzista ipotizzerà un successo inferiore dei bambini negri a scuola credendo in una loro intrinseca inferiorità, collegabile al colore della pelle.

Purché l'ipotesi sia operativa (ossia nel caso consenta comunque modalità di verifica empirica), la pressione dei valori non procura in sé alcuna mistificazione del reale, in quanto l'ipotesi (i bambini neri di pelle rendono meno nei corsi scolastici) potrà essere verificata effettivamente. Ma sarà solo tentando di falsificarla che sarà possibile rivelare come il colore della pelle sia una pseudo-causa (ossia una causa provvisoria ed euristica) e che una variabile indipendente sottostante è l'istruzione dei genitori.

Anche questa a sua volta sarà ricollegabile al reddito delle famiglie negre. È presumibile allora che queste tre ricerche *possano* essere originate da punti di vista o ideologie diverse dei ricercatori: ciò non toglie che nei limiti in cui sono partite da definizioni operative e hanno dato luogo a ipotesi verificabili, non hanno procurato alcun danno alla scienza. La conoscenza scientifico-sociologica, come ogni altra, viene allora configurandosi come un insieme di leggi e teorie (verificate o verificabili) a vari livelli di profondità, ognuno dei quali valido *nei termini in cui è espresso*, mentre è solo dal libero giuoco della falsificabilità che è possibile scendere a livelli (leggi e teorie) più precise ed estensive.

Un altro punto (precedente cronologicamente al primo), sul quale indubbiamente agiscono generalmente, ma non necessariamente, i valori della più varia natura, è la scelta del problema. Questioni e bisogni di origine sociale e politica, sanitaria o economica spingono gli scienziati e gli uomini in genere a riconoscere come degni di studio settori della realtà che, per il loro contenuto, vengono dunque selezionati dalla serie enorme e multiforme delle osservazioni casuali. È quanto mai evidente che questa relazione con i valori non pregiudica in alcun modo l'oggettività della ricerca, ma, semmai, pone problemi validi e degni di studio, ancora una volta, nell'ambito della *politica della scienza*, non già della metodologia. Secondo Talcott Parsons, « l'indagine scientifica è in se stessa un processo d'azione; è il perseguimento non della conoscenza in astratto, ma di una particolare conoscenza di cose particolari. Con riferimento ai dati, essa è un processo selettivo, la selezione essendo determinata sia, come abbiamo visto, dalla struttura dei sistemi teorici che da considerazioni extrascientifiche » [47].

Aldilà dei valori e dei bisogni, la caratteristica fonte di problemi dello scienziato rimane comunque a nostro parere quella insondabile curiosità intellettuale che sollecita lo scienziato a fare della conoscenza una professione [48] e, come afferma H. Greniewski [49], una sorta di gioco sempre rinnovantesi «...je crois que chaque recherche scientifique est un jeu; le chercheur, le savant en est le partenaire; l'objet de la recherche est l'adversaire ». È, in definitiva, una delle molteplici forme di porsi di fronte alla realtà: così come l'artista, il politico, il missionario o il tecnico, anche lo scienziato ha una sua ottica selettiva, una sua sensibilità che lo spinge a conoscere sempre qualcosa in più.

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[23] Le linee tratteggiate stanno ad indicare la possibilità sempre aperta di collegare i modelli di ogni analisi nomologico-inferenziale ad altri modelli dello stesso carattere.
[24] Tratta, supponiamo, da leggi ampiamente verificate in precedenza su campioni di negri il cui reddito sia aumentato.
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TRANSLATION: J. E. HOLMSTROM - CHRISTCHURCH, HAMPSHIRE

SCIENTIFIC METHOD AND LEGITIMATION OF SOCIOLOGY

Sociology is often described as a young science. But the question is also asked whether it is a science at all. Can it indeed be a science? This has so long been debated that to anyone nowadays who has accepted the position either that sociology is a science on a par with others, or that sociology means the philosophy of society, of social institutions, etc. the debate has become somewhat barren. In point of fact, even among those who maintain the second of these theses there is sometimes an ambition to describe their own particular methodology as being a « scientific » process even while recognising the need for ad hoc *adjustments and concessions to the intrinsic nature of this particular science. The present author's opinion is that no science can afford to make any compromise that impairs its guarantees of objectivity and communicability: for one of the characteristics of a science must be its use of a method that is of general application, particular branches of science differing only in the diversity of their research techniques. If sociology follows such a method, then it is a science. The difficulties that arise from so doing are often merely pseudo-difficulties which are mistaken for real ones only because it is not understood that similar ones have arisen, and will continue to arise, in all other sciences also; while moreover the methodology now available makes it possible to re-interpret and thereby to resolve them. There is in fact no doubt that the scientific method can be used in sociology; the only real difficulties are those connected with the complexity of its aims and with its relationship to the society it studies.*

1. EXISTENTIAL KNOWLEDGE AND SCIENTIFIC KNOWLEDGE. — First of all there is a need to make clear what is meant in this essay by scientific knowledge. We shall take the view that the latter is to be distinguished from any kind of encounter with reality pursuing technical or political aims, and that even in cases where such encounters are capable of being interpreted as a form of knowledge they nevertheless do not constitute scientific knowledge, whose characteristic feature is that it is not concerned with the attainment of desired states but with attaining, by methodical means, an understanding of reality.

In other words there is more than one way of « knowing ». Knowledge of a new social reality such as the product of a revolution can be gained even without having knowledge of the old reality; change, disintegration, construction are certainly matters of knowledge. But knowledge of them is not necessarily scientific knowledge; it becomes so only if arrived at by using the *method* of science.

In line with a definition recently proposed by an anthropologist it would be fair to generalize that the « *recognition* » process whereby one becomes possessed within oneself of an already « *given* » *reality* is an essentially passive concomitant of being social and of tending towards a settled conformism, by contrast with *cognition* which is a stretching forward to new realities and new values [1].

This clearly implies that in cognition a leading role is played by practical and innovative activity.

Furthermore the scientific method consists essentially in logical-experimental procedures for the « verification » of hypotheses or theories - or, as Popper prefers to put it, in procedures directed towards « falsifying » them, so that if the confutation is unsuccessful they take their place in what Kuhn defines as « standard science »: the science that a particular scientific community, over a certain period of time, accepts as being able to serve as a foundation for its subsequent practice [2] in so far as it supplies that community with *paradigms* whereby a certain class of questions can be answered and whereby it is often possible to respond to certain needs in application.

It should be evident that here there is no intention of attributing to scientific knowledge a character more genuine or less genuine than other kinds of knowledge; all that is intended is to supply a very necessary clarification by restricting the scope of the expression « scientifically verified knowledge » to knowledge which is governed by the logico-experimental scientific method. The possibility of constructing, of innovating and of definitely « knowing » without the agency of science is not denied, nor is it denied that intuition, sensitivity, the synthesising ability of the individual, whether he is a scientist or not, supply a large part of our everyday knowledge and often serve as a foundation for the starting of highly scientific investigations and studies. All it is intended to assert is that science provides a way of arriving at knowledge of reality which is founded upon the logico-experimental method, while any other kinds of knowledge which cannot be traced back to this may be better or worse as determined by personal or social circumstances, tastes and ideologies. It is in fact not uncommon to find cases where people claim a « scientific » character for their own work solely because of the prestige attaching to that adjective (according to Sorel, Karl Marx was such a case) although in their work they do not pay the proper tribute entitling them to use this distinction as they do not follow the *method* of science as well as pursuing its aim.

2. NEO-POSITIVISM, PRAGMATISM AND THE REVALUATION OF THE SCIENTIST. — Underlying this conception of science whose principal characteristic is that the method used consists in verification of hypotheses, it is easy to recognise a neo-positivist or logical-empirical position, in some respects recalling principles which date back to Galileo but are not followed out to their ultimate consequences. The particular aspect of the neo-positivist position we intend to profit from here is, in the first place, its firm opposition to the injection of metaphysical elements into the scientific system. But if this otherwise acceptable presupposition were allowed to deflect us from our course it might finally lead either to a revival of « physicalism » such as would reduce science to a collection of propositions regarding physical facts or, on the other hand, to the solipsism of Wittgenstein which contrasts with the intersubjective character of scientifically known facts. In our opinion, while it is certainly inadmissible and even impossible to insert metaphysical elements into scientific propositions, this does not exclude extra-empirical reflections from playing a part in the study of the scientific method. On the contrary, the whole doctrine of method is by definition extra-empirical, for if this were not so it could be nothing other than a physical reality thinking about itself. What the orthodox neo-positivist school regards as « meaningless propositions » do not appear meaningless to us unless it is impossible to give an empirical version of them without thereby implying an opposition between proposition and fact in terms of indecomposable propositions (Wittgenstein) or in terms of logical coherence between factual propositions and physical facts (Neurath). It is certainly possible to establish a correspondence between predictions based upon empirical concepts and empirical objects even of a complicated kind (contrary to Wittgenstein) and to define the process of verification as being not so much logical (contrary to Neurath) as methodological or supported by conventions which

might be called « procedural » since they are neither non-formal nor obviously empirical (which would be a contradiction in terms). Indeed the act of verification involves (not « presupposes ») a psychological agreement among the observers in regard to the « datum », which either confirms or contradicts what had been anticipated in an inter-subjective manner. At this level a connection is established between convention and empiricism - a connection not attributable qualitatively to either of the two components and not assertable *a priori*, but observed in practice.

The concept of « present disposition » (about which more will be said later) which we attribute to a reproductive cell is not ontological but methodological. This statement is another way of saying that the implication of such a term is neither true nor false at the moment it is used, nor can it be reduced to a state of truth or falsity by means of logical calculations, but is simply a methodological concept constructed for the purpose of *making it possible* for the laws of reproduction and classification to be verified by applying them to objects capable of reproduction without these laws *thereby being* verified. According to Neurath the sociologist is completely free in his quest for laws save for the one condition to which he must always conform: namely that of referring his predictions to structures that are already determinate in space and time [3].

We may, therefore, understand this « freedom » of the scientist as extending also to the assumption of concepts for which there is no empirical evidence, but which are capable of being postulated methodologically in order to make explanations and predictions possible. While it is clear that ontologising the « present disposition » would lead us to entelechy and therefore back into deductive metaphysics, it is also evident that the only way of continuing the quest for more accurate and empirical connections is by conferring a name, methodologically, upon whatever is provisionally and hypothetically taken to be the « cause » of a certain fact. All this goes to show how essential it is for us to conceive scientific knowledge as being an interactive process which sees, in the searcher and in the external world, the two realities upon which it is possible to base the assumption of methodological conventions for the construction of concepts and for the evaluation of proofs. It is, in fact, through passing from the symbolical to the empirical, and through denying the scientist the ability we ascribe to him of acting as an intermediary, that a position taken up by the anti-dualistic neo-positivists has collapsed. Weinberg himself, goes further, and in trying to formulate a new theory of language which takes account of the neo-positivists' fundamental views he acknowledges that the difficulty of formal language lies in the fact that within the scope of such a language it is impossible to express the relation between the rules of protocol that govern objective language and the empirical data to which those rules refer [4]. He then proceeds to suggest introducing the term « datum » of experience. But it is impossible to see how such a datum can be conceived otherwise than as the typical product of the scientist's conceptualising activity (abstraction) which, at least on this level, is not metaphysical but methodological.

Another fundamental point on which we agree with neo-positivism and to which we have implicitly assented is the need to accept prediction as a characteristic feature of science. Once the objection that denies the possibility of inter-subjective truth has been overcome and once the scientist is freed to constitute himself as an autonomous creator of conventions (insofar as empirical reality is autonomous in the production of objects), prediction can once again be acknowledged as a very valuable instrument for the confirmation or disproof of scientific laws. Here again, however, extreme physicalism would appear paradoxically to invest these laws with the ontological character that Galileo ascribed to astronomical laws [5], whereas in the present author's opinion it is better to stop at the point where a law is recognised as being a methodological instrument employed for the purpose of explaining and predicting. This question too will be taken up later, but enough has been said to rule out any rigidly pragmatical position as regards prediction. Here prediction is to be understood not solely as pursuit of something useful but as enunciation of empirical truth objectivised by way of conventional meanings. While it is indeed possible to reach as far as the verge of some desired reality by using procedures of the « trial and error » type, or by jumping straight into explanations of the facts, it is essential to remember that prediction has the additional aim of either confirming or refuting given explanations of facts by so reconciling them with one another as to constitute general laws.

This thesis has been well stated by the sociologist Emile Durkheim in terms of the following example: « ... suppose that a simple-minded man has been persuaded that a physical ill from which he suffers is due to evil spirits having entered his body (as indeed is often believed in primitive societies). Suppose he is made to swallow some very disagreeable substance, being assured that this will chase away the evil spirits tormenting him. Believing so he is cured. The result is what was anticipated, the treatment may even have been 'appropriate'. None the less the idea underlying it was a false one » [6].

It is evident that if a science were understood to mean nothing more than a system able to supply predictions that came true and were found useful, without being expected to concern itself with explaining the facts it chose for its field of investigation, it would be deemed to have yielded satisfactory results and to be justified merely by the circumstance that these results had great practical value. Further, the same criterion must be supposed applicable to extreme neo-positivism: indeed, provided a prediction is seen to stand up to confrontation with facts (and disregarding the difficulty already mentioned as attendant upon « confrontation ») the extreme positivist will be satisfied - holding that the phenomenon under investigation is thereby *explained* - as long as the explanation accords with simple empirical facts and is not contradictory but coherent on the logical plane. Actually, however, the scientific mind refuses assent to this merging of what is logical with what is atomic: as soon as a law has been promulgated by one scientist the others feel urged by various motives to attempt a disproof of it, and if further investigations are carried out with that object they in turn will engender further explanations notwithstanding that the latter may be mutually contradictory, may not be coherent with « established knowledge » and may not be reconcilable with some of the « atomic » facts.

In other words, a science regarded as a source of explanations able to be extended beyond the reach of any possible challenge would pursue lines of research pointing towards some object already explained from a particular aspect, and it would be able not only to furnish sounder explanations of that object but also to supply more accurate predictions in a larger number of cases.

This function of science might, with some hesitation, be welcomed even today as providing a fulcrum for neo-positivist epistemology, and it is associated with another: the unifying function of science. Such, indeed, is the aspect from which the present author considers the logic of science is likely to gain the truest acceptance: namely, as a study of possible interconnections between laws whose existence has been established in different fields of science, carried out for the purpose of rendering explanations and therefore predictions more accurate. But even this work, which obviously will have to be undertaken anyway as part of the future integration of the scientific and technological world, cannot envisage an identification of the knowable with the real taking place once and for all. On the contrary it needs to be borne in mind that science is not necessarily the only means whereby reality can be sensed; it is merely the form of knowledge that purchases its generality and its effectiveness not by actually *denying the existence* of other aspects and of others of man's cognitive methods but merely by *ignoring them methodologically*. In other words, they are not *absolutely* «meaningless» but are solely extraneous to scientific sense.

3. METHODOLOGY AS THE GENERAL INSTRUMENT OF INDIVIDUAL INTUITION. — Furthermore, even someone seeking an approach of his own to sociology has first to make a fundamental choice between sociology as a science and sociology relying mainly upon intuition, participation, psychoanalysis, historiographical methodology, etc.

The former alternative is based on the conviction that sociology too is an empirical science despite the fact that nearly always it has to rely on an indirect empiricism, an empiricism mediated by objects (either individuals or groups) which does not, however, resolve itself into these but is concerned with the structures and the processes (facts) to which they give rise as perceived by emprical indicators.

The methodological choice is derived from the epistemological choice. If sociology is in fact an empirical science it cannot do otherwise than employ the method that is typical of empirical sciences.

The indispensable orienting function which methodologists perform will, therefore, become more and more prominent, particularly so in view of the extreme proliferation that characterizes modern science and because scientists themselves do not always have an accurate knowledge of methodology even when, to the eyes of an observer, their actual work shows a very evident uniformity of procedure.

While C. Whright Mills [7] states that physicists such as Kusch and Bridgman consider there is no such thing as a scientific method, he recalls that according to William S. Beck the creative process « is so intimately connected with the emotional structure of the individual that it is ill-adapted to generalization ». Here it seems to us that Mills is making an incorrect appraisal of methodology; for in point of fact:

(1) It is not the purpose of methodology to tell the scientist how he should proceed, but to find out what method he is in fact using in his research, even unconsciously, with a view to studying the logical structure of that method and of refining its theoretical and technical aspects to the scientist's advantage.

(2) Mills accepts the statements of the physicists he cites uncritically, or in other words he trusts to the *subjective opinions* they hold as regards their emotional creative procedures. He forgets, for instance, the solid foundations

of mathematical methodology inculcated into the physicist in the course of his academic and personal studies. Often the scientist is unaware of any methodological « rules » because he belongs to a tradition already permeated with the criteria of theoretical-experimental science. On the other hand it should be repeated that the given explanations can be extremely useful because, as Kuhn [8] put it when referring to scientists' consciousness of the methodology they use, «if they have in fact learned any abstractions of this kind the effect of it is revealed mainly in their ability to conduct a research successfully. Science is normally able to proceed without rules only to the extent that the scientific community concerned is ready to accept without question the particular solutions to problems previously reached. The rules should therefore gain in importance, and the characteristic neglect of them should tend to disappear, in cases where the paradigms or models are felt to be unreliable. This in fact is exactly what is seen to happen».

(3) The general lines of scientific procedure seem to have been common knowledge from the time of Galileo down to our own day, but it is important not to read more into them than they imply.

All that the scientific method lays down is the successive *steps* that should be taken (and which are seen to be taken in the procedures actually followed by scientists) in order to arrive at descriptions or explanations that will satisfy common criteria of truth founded upon the epistemological positions modern empirical scientists occupy.

Nobody can say how or by what means « strokes of genius » or « original intuitions » are generated, or how new working hypotheses are started. As Popper expressed it, «no profound truth is to be expected from methodology» [9] but only logical-practical conclusions. It is on the basis of pre-existing knowledge (besides chance observations) that a *problem* takes shape in the mind of the researcher, towards which he will then try to direct his research guided by *working hypotheses.*

Generally speaking it is in the formulation of *hypotheses* (which are essentially « anticipated solutions » to problems, awaiting confirmation) that the creativity to which Mills refers may be seen, but this would remain barren intuition if it were not subjected to the rigorous application of the scientific method. In order to avoid giving an impression of surrender to an empty nominalism, let it again be emphasised that for us the empirical sciences properly so called, meaning those sciences that are amenable to the logico-experimental method, are the *natural sciences* (physics, chemistry, biology, etc) and the *human sciences* (psychology which relates to individuals; economics, sociology, anthropology, etc. which relate to societies).

Thus the fundamental assumption underlying logico-experimental method is that a science has been constituted when, having to handle *empirical* objects or facts (meaning objects or facts that can be observed either directly or indirectly) it is possible to deduce from their characteristics and their changes an *explanation* formulated in terms of *laws* and *verifiable theories*, which will in turn make it possible to *predict* future characteristics and changes.

4. EMPIRICAL OBJECTS AND FACTS: THEIR SCIENTIFIC EXPLANATION. —

Let us now clear up a point which was thrown into relief by the definition just given, where reference was made to « empirical objects or facts ». What was meant by this phrase is that scientific work can be carried out only in regard to objects that are directly manifest to the visual or other human senses or, as is true for a large part of modern physics, with the aid of instruments (such as a Geiger counter) capable of revealing the existence of such objects or facts as are not observable directly. Sociology is a similar case to the last mentioned, being enabled by indicators, statistical indices and other indicating instruments to study social structures and processes.

Empirical evidence of objects has aptly been defined by C. G. Hempel [10] as being either « direct » or « indirect » according to whether their existence is discernible by human agency directly or is dependent on indirect indications (exemplified in the excitation of a Geiger counter) which presuppose the existence of objets or of facts.

Another word used here is « explanation ». This is intended to mean the deduction of the facts to be explained from laws or theories. Using it in this sense we are disregarding a whole set of interpretations of « explaining » which are implicit in existential or metaphysical philosophy or in the part of classical physics and which seem to imply that « causes » possess ontological reality. It is possible, however, for the nomological conception of « explanation » (as being something deduced from laws) to embrace also a form of explanation which is now widely adopted in sociology, known as functional explanation. When it is asserted that a functional system tends to revert to a certain condition after having been perturbed it is in fact implicitly being asserted that the homeostatic behaviour takes place in obedience to laws [11]. Functionalism, if purified from teleological (or finalistic) temptations in the traditional sense, becomes acceptable where a possibility exists of detecting organic, social or other « systems » endowed with some form of auto-regulation [12]. In such a case the logical form of explanation can be defined by a nomological-inferential model as follows:

$$\begin{array}{cl} \left.\begin{array}{c} L_1\ L_2\ \dots\dots\ L_n \\ C_1\ C_2\ \dots\dots\ C_n \end{array}\right\} & \textit{explanans} \\ \hline E & \textit{explanandum} \end{array}$$

In Popper's terminology the L_1, L_2 ... L_n terms correspond to « universal assertions; i.e. hypotheses having the same character as natural laws », and the C_1, C_2 ... C_n terms are « singular assertions » which hold good for the specific events. From the universal assertions the singular assertion is deduced [13].

5. SOCIAL LAWS AND SOCIAL FACTS; SOCIOLOGY AND HISTORY. — In the definition, a reference was made to *laws*. These are statements of uniformities in the behaviour of the objects studied by a science. One example of a law is $E_c = -\frac{1}{2}mv^2$ in physics, which expresses the connection between the kinetic energy, the mass and the velocity of a falling body. Another is the statement that « whenever the demand for a certain commodity increases its price tends to rise », which relates to short term economics. Yet another is the statement in medicine that « around 20 years of age the thymus becomes atrophied ».

The fundamental characteristic of a scientific law is its *generality*: such a law applies to all the objects or all the facts included in the predicate for which it is stated. It is therefore impossible to state a scientific law in respect of a single and unrepeatable event. History, for instance, has its field of study made up of individual and unrepeatable events and for this reason it is scientifically meaningless to speak of «historical laws», whereas in sociology it may be possible for a law established today to be applied to certain facts which occurred yesterday, provided they have the characteristics specified in the present law. Historical analysis, being concerned to give a dynamic (diachronical) account of events, does not and cannot dwell experimentally upon any one event E in order to submit it to tests, and repetitions of tests, with a view to establishing general laws that govern behaviour. In so far as it is concerned with sequences of facts, history may better be compared with laboratory investigations in which the variables are continually changing so that the attribution of characteristic « causes » and « effects » to single facts or classes of facts (never homogeneous) has no meaning unless it related to the necessity, from which historians have no escape, of displaying the development of events on a logical thread, not merely a chronological one.

That is why historical interpretations are so manifold and are often inconsistent with one another. Generally the historian keeps in stock a few key variables (like economics for Marx, religion for Weber) which, for each relevant new event E, seems to offer a contribution to the building up of general laws (either unexpressed or rendered explicit) from which a clarification or explanation of the single event may be deduced. In other words the historian's preoccupation is not that of hunting for empirical confirmations such as would enable him to authenticate those « laws » and to make predictions, but is to give a post hoc account of an event which has taken place and to which some sort of motivation needs to be ascribed. Thus the fundamental difference between the methodological urge experienced by the social scientist and that experienced by the historian lies in the fact that the former is setting out to find experimentally verified « explanantia » whereas the latter puts forward his explanations as being self-validated.

Such are the « general interpretations » which Popper accepts as « provisional theoretical parameters, usually not verifiable, but useful for the historian's work » [14]. History always retains this provisional and non-verifiable character which is what makes « historical explanation » a form of « philosophy », best described as « philosophy of history ».

6. Prediction as an Intellectual and Practical Need. — Sociology, no less than any other science, aspires to be able to make verifiable predictions regarding the future states of social « facts ». Indeed scientific knowledge in the modern sense of the word is attended by something we feel as a challenge and it is the fascination of this that provides one of the main stimuli for research, the other stimulus being the ability which science confers on us to dominate nature and things in response to demands which are forceful and urgent. Moreover the necessity for true and accurate predictions as well as the fascination of being able to provide them are a response to the times in which we live, characterized as these are by so many technical, social, cultural and political changes which tend to generate anxieties for the very reason that they are difficult to control and to predict.

In this context it is no accident that the beginnings of sociology coincided with the production of the most conspicuous and far-reaching predictive works that social thinking has ever generated. For examples of this one need only recall the works of Karl Marx and Comte, with their explanations of the past and their predictions of future developments affecting all mankind and all human knowledge. The motive for these « complete and exhaustive » works was not indeed scientific but the outcome of exasperation due to the pressure of events at the time. They were of the kind aptly defined by R. König as philosophy of history and society [15], romantic in form, alluring and from certain points of view cognitively valid, providing evidence for the existence of social problems which called for scientific answers. And it was in this last-mentioned direction that in addition to the authors cited here [16] certain sociologists initiated *scientific* work truly and properly so called, necessarily of a kind which proceeded from the simpler to the more complex in social phenomenology - without however aspiring to exhaustive treatment of the whole global field within the short compass of an individual human life as the universal colossi mentioned above set out to do. The birthplace of sociology in the true sense lay amid the researches of F. Le Play, the proven theories of E. Durkheim, the methodological theses of Weber the best pages of V. Pareto.

7. A CONTROVERSY AS TO THE LOGICAL STRUCTURE OF PREDICTION. — From an analysis of a single occurrence (or of a sufficient number of cases able to be defined and empirically circumscribed) there may issue a « law » or a series of « laws » and these may give rise to « theories » which in turn may suggest new hypotheses ready for testing at a higher level. Let us now proceed to analyse the scientific significance, and the operational limits, of sociological prediction rendered possible by the sociological laws and theories, viewing the matter from the standpoint of those scientific circles that look upon predictive capability as an explanatory methodological instrument on the cognitive plane, and as a pragmatically useful one on the operational plane.

For the expository purpose intended here it will be best to begin with a logical analysis of *prediction* and to follow this by an analysis of the strategic relevance of *theory* in the field of prediction.

A formalistic description of the predictive act can also be given by reference to the « nomological-inferential » model already explained [17]. The double line which separates the *explanans* from the *explanandum* in the second of the models below is meant to indicate the probabilistic character of some scientific predictions and it will be well to remember that in sociology we are concerned, generally speaking, with models of that type.

$$\begin{array}{lcl} \text{I} & \left.\begin{array}{c} L_1\ L_2\ \ldots\ldots\ L_n \\ C_1\ C_2\ \ldots\ldots\ C_n \end{array}\right\} & \textit{explanans} \\ \cline{2-2} & E & \textit{explanandum} \end{array}$$

$$\begin{array}{lc} \text{II} & \begin{array}{c} L_1\ L_2\ \ldots\ldots\ L_n \\ C_1\ C_2\ \ldots\ldots\ C_n \\ \hline\hline E \end{array} \end{array}$$

In this scheme it is definitely affirmed that « given the laws $L_1, L_2 \ldots L_n$ and the conditions $C_1, C_2 \ldots C_n$ the fact E is to be expected ». If for instance a law consists in the proposition « man is mortal » and the condition is given as « Socrates is a man » it will be easily seen that the fact « Socrates is mortal » is « explained » by something which fundamentally is nothing else than a form of syllogism.

An explanation of facts that are repeatable is also a prediction of them. Indeed every time we ascertain that the C_1, C_2 ... C_n exist (or ourselves bring them into existence by some technical action) we are enabled to predict the fact E either deductively or probabilistically.

However, the methodologists of science do not all quite agree as to the logical identity of *explanation* and *prediction* in the nomological-inferential model as here maintained. Let us consider the thinking of M. Scriven, S. F. Barker and N. Rescher [18] on this point. According to Scriven predictions exist which are not based on precedent explanations. When, for instance, we observe a drop in the berometric pressure we forecast a storm although the latter is not able to make the mercury column in our instrument rise again. In reality the logical process does not appear to us any different whether we intend to explain or to predict the fact; it is merely a question of difference in level of the reasoning, whose structure remains the same. Model A is valid as a simplified explanation of the storm whereas model C is required in order to predict the storm but, as may be seen from their juxtaposition below, the *explanandum* in B is the same as the condition C_1 of model A. Hence the statement « a storm has occurred *because* the mercury column has dropped » means *explaining* the event in the sense that the drop of the column itself recalls us to models A and B. In other words no explanation arrived at from a nomological-inferential model is itself definitive but models at other levels are implied by it.

MODEL A	MODEL B	MODEL C
L_1 (When P falls there is a storm)	L_2 (Whenever the mercury column falls there is a pressure drop)	L_3 (Whenever the mercury column falls there is a storm)
C_1 (P falls) ←	C_2 (The mercury column falls)	C_3 (The mercury column falls)
E_1 There is a storm	E_2 There is a drop in pressure	E_3 There is a storm

According to Barker, sometimes it is possible to *explain* without being able to *predict*. The case of a patient who catches pneumonia and dies can be explained but not predicted, as it is a rare event nowadays to die of pneumonia. Fundamentally what is being pointed out here is not the logical difference between the two cases (explaining and predicting) but their differing degree of certainty. Since, then, we need to qualify the explanandum according to rising degrees of probability we may construct three nomological-inferential models D, E and F corresponding to an increasing degree of severity of the illness, for it is not only accurate prediction but also accurate explanation that demands exact estimating of the conditions C_1, C_2 or C_3 and it is only if we know the seriousness of the illness that we are in a position to explain and to predict.

MODEL D	MODEL E	MODEL F
L_1 (Slight pneumonia is fatal in 1% of cases	L_2 (Moderately severe pneumonia is fatal in 30% of cases)	L_3 (Severe pneumonia is fatal in 80% of cases)
C_1 (John has slight pneumonia)	C_2 (John has moderately severe pneumonia)	C_3 (John has severe pneumonia)
Em (There is 1% probability that John will die)	Em (There is 30% probability that John will die)	Em (There is 80% probability that John will die)

Rescher declares that the conclusion drawn from an explanation is certain whereas that drawn from a prediction is not. Actually, however, the certainty is of the same nature and dimension in both cases. If we know the L (or in other words the laws governing a generality of facts) as well as knowing the C_n, or if we know that whatever is decreed by laws really will occur, this enables us to infer either that a fact F will occur in a time $T + K$ or that it has already occurred in a time $T — K$ in relation to our time T. The reasons why an explanation of something that has already happened may be wrong are the same as the reasons why we may be unsuccessful in our prediction of something still to come.

What is indisputable is that a *new and more accurate model can be constructed only if some new or unexpected fact comes to light* (such as a patient dying of only slight pneumonia).

But it should be noted that explanatory reasoning and predictive reasoning will both have the same structure once this new model has been established, it being the same model that makes both of them possible.

If a doctor is informed that some patient ($> 1\%$) with only *slight* pneumonia has died he will have to seek an explanation of this fact according to a new model, differing from the one on which he has hitherto relied which assigned lowest probability of death from pneumonia; he might for instance bring in the patient's age as an additional variable. Symmetrically with this, if the same doctor had been asked for a prognosis regarding a certain patient who had a slight touch of pneumonia he would have excluded all likelihood of this being fatal and if death in fact ensued this would have compelled him, *in the same way*, to reconsider the case and equip himself with the new model.

It can, therefore, be concluded that explanation and prediction are performed according to the same model of reasoning, that both of these performances may be rendered erroneous by deficiencies of the model and, finally, that motivation for updating the model is supplied either by the explanation made necessary by a new fact experienced or by the non-fulfilment of a prediction. Popper recognises that the term « prediction » covers assertions regarding the past (« retrodictions ») as well as « given » assertions that call for explanation (« *explicanda* ») [19].

8. LAW, THEORY AND THEIR EFFECTS ON THE VERIFICATION OF PREDICTIONS. — R. K. Merton maintains that an empirical law [20] alone cannot serve as a basis for successful prediction because the correlation it asserts

between a « fact » X and a « fact » Y may not be exhaustive. To give an example, if a scientific appraisal were limited to the statement that « in a mixed population the Catholics have a lower percentage of suicides than the Protestants » [21] any predictions based on this statement would be very unreliable because the correlation is not embodied in a « theory ».

Merton does not deny the validity of predictions extrapolated from statistical trends or laws, but he maintains that in the absence of some overlying « systematic theory » their margin of error tends to increase. Merton explains the importance, both in theoretical and in empirical research, of *deriving* a theory from a law or of *inserting* a law into a theory (according to whether, in a particular case, the theorising precedes empirical research or follows it) - meaning a theory similar in type to what can be constituted by restatement of the theses adumbrated by D. Durkheim in his book on suicide, as follows [22]:

« 1. Social cohesion provides psychic support to group members subjected to acute stresses and anxieties.

2. Suicide rates are functions of *unrelieved* anxieties and stresses to which persons are subjected.

3. Catholics have greater social cohesion than Protestants.

4. Therefore, lower suicide rates should be anticipated among Catholics than among Protestants ».

Here it is evident that the correlation first characterized in the empirical law as a link between suicide and religious affiliation is amplified, and at the same time rendered more *precise*, by showing that suicide can be linked with many types of anxiety states that occur more frequently in individualistic groups, and therefore among Protestants.

Though in effect « *theory* » does not appear to be *logically* any different from law, there is certainly a gain in precision when « facts » are connected with better articulated laws; this improves their defining power and, along with that, the possibilities of prediction.

By formulating and reorganising points 1,2,3,4 in this theoretical paradigm of suicide we are enabled to realise the following series of « models »:

> Where there is ***social cohesion***, ***psychic support*** is afforded to the members of a group (L_1).
>
> Catholics have great ***social cohesion*** (greater than Protestants) (C_1).
>
> Catholics have strong ***psychic reinforcement*** (greater than Protestants).

From these three propositions the next model emerges:

$$\begin{array}{c} L_1 \\ \underline{C_1} \\ \hline E_1 \end{array}$$

Furthermore:

> Where there is ***psychic support*** there is ***low propensity*** to suicide (L_2).
>
> Catholics have strong ***psychic support*** (stronger than Protestants) (C_2).
>
> Catholics have low ***propensity*** to suicide (less than Protestants) (E_2).

The model now becomes:

$$\begin{array}{c} L_2 \\ C_2 \\ \hline\hline E_2 \end{array}$$

and the connection between the two aforementioned is immediately apparent:

$$\begin{tikzcd}[row sep=small]
 & L_1 & & L_2 & \\
 {} & C_2 & & C_2 \arrow[r, dotted] & {} \\
 & = & & = & \\
 {} \arrow[ruu, dotted] & E_1 \arrow[rruu] & & E_2 \arrow[ruu, dotted] &
\end{tikzcd} \qquad [23]$$

indicating that the *explanandum* of the first becomes the *condition* for the second. Thus a logical linking of two laws which relate to two objects relatively far apart makes it possible to impart theoretical *precision* to an empirical law which contained only the observed categories « catholic (or protestant) » and « suicide (or non-suicide) ». In this way a chain of causation initially formulated as:

to be catholic ⟶ to have low propensity to suicide

is made to become:

to be catholic ⟶ to have a sense of cohesion ⟶ to feel psychically reinforced ⟶ to have low propensity to suicide

The greater *precision* (an essential factor in achieving greater predictive capability) is manifest. If in fact the probability of a catholic committing suicide is denoted by « p » the probability of any individual (even a non-catholic) who lives in a coherent group doing so will be « p - k » and the corresponding probability for any individual who receives psychic reinforcement from something or someone (even though not living in a coherent group) will be « p - k - z ».

Here the point is that while 'psychically reinforced' people resist suicide more than others yet some Catholics *as such* may be more liable to suicide, for it is not affirmed that psychical reinforcement and being a Catholic are always linked. Hence a prediction based on the statement « so-and-so is a Catholic » must always be less exact than one based on the statement that so-and-so is « psychically reinforced » by some ideal or by another person, etc.

Therefore the purpose of theory is to determine empirical laws more accurately by correlating them with another, while at the same time perhaps giving birth to new laws or hypotheses of laws. In fact by so doing it offers a stimulus to scientific procedures making it possible to predict events and to influence reality with greater accuracy and certainty.

In any case, predictions regarding the occurrence of « facts » that are *susceptible* to repetition take the form in ordinary scientific methodology, of extrapolations from laws, the theories being themselves collections of laws as has already been shown. When we are dealing not with repeatable « facts » but with unique non-repeatable « facts » the case is different.

Even in the field of repeatable « facts » an extraneous hypothesis, unconnected with any laws or theories, must be vacuous unless there is some hope, or rather some expectation, that it can be related to laws and theories which are repeatable; but in the field of non-repeatable « facts » no generalization whatever is possible and prediction cannot be based on anything more solid than an abnormal sort of methodology describable as a flash of genius, predestined to remain such and to expire when the predicted « facts » are observed to occur (or not to occur). If by « science » we understand that species of cognitive procedure which is able to guarantee its own validity by referring back to generalizing laws and theories which are capable of verification, we are bound to assume that when we are confronted with a world of non-repeatable « facts » it is impossible for us to perform any scientific work and the most that can be done is to juxtapose one « fact » with another qualitatively in the hope that something that carries conviction will emerge. Such a procedure would in a sense be governed by inferential models (hypothetical syllogisms) of the type introduced by the Stoic schools of philosophy, deriving one particular from another.

9. Renunciation of the Quest for Final or Experiential Truths in Sociology. — The foundations of sociology and the problem of whether it can be regarded as a science have been discussed, and sometimes still continue to be discussed, by reference to the question whether this subjects is concerned with repeatable « facts » or more truly with non-repeatable ones.

Thus the historicist school in general as represented by Dilthey, Windelband, Rickert and Freyer in particular, though with a varying distribution of emphasis and using a terminology that is nearly always ontological, draws a distinction between the *individualizing* sciences which deal with the « non-repeatable » and the *generalizing* sciences which deal with what is « repeatable » (Rickert). Following this line of thought sociology belongs to the former class of sciences, with the consequence among others that it cannot generate predictions able to be referred to general laws, but only intuitive interpretations which may or may not be self-intelligible (Freyer).

The thesis holds that sociology is an individualizing « science ». This belief derives ultimately from the metaphysical position which ascribes an « essentiality » and a « property » to man and to human groups, corresponding more or less to the Aristotelian « substances » or the Kantian « noumenon »; whereas sociological science like any other has a logico-experimental character and purports to do no more than investigate and systematize those *phenomenal* aspects of human behaviour that can conventionally be objectivised. The thesis that sociology is an individualizing science seems reluctant to accept the pragmatic and conventional character of science and continues in a vain quest for forms of knowledge able to incorporate the *inner essence* of objects, meanwhile denying that empirical sociology can be a definite science.

In point of fact empirical sociology (judging from the admittedly modest amount of empirical and theoretical effort hitherto devoted to it with success) appears decisively to be capable of cultivation on the same methodological plane as the other recognised sciences, whereas the historicists and those who rely on intuition and anticipation seem unable to differentiate or emancipate themselves from a philosophical trend which is inevitably subjectivist and sceptical.

The thesis accepted here is the one that allows sociology to be described as a generalizing empirical science capable, as such, of providing *laws* and *theories* for the organisation of social « facts » without the existential meaning which these assume in the sociologist's mind impairing his power to analyse and interpret them objectively.

We do not, therefore, here accept the thesis according to which « sociological knowledge » can be shaped only by internal participation within a group or a society. It is clear that knowledge so constituted would be completely devoid of any capacity for being communicated to other persons save at a relatively superficial level. If in fact an investigator feels obliged to detach himself from his own world in order to become immersed in the world he intends to study he will find himself in the same difficulty, when wishing to convey what he has found out to others, as he would encounter if he tried to understand the world of others whilst remaining attached to his own. Even those cultural anthropologists who broadly accept the methodology of participation have been led to recognise the necessity for making analytical and participative concepts, categories and definitions communicable to others. This statement applies equally to the « cultural models » of Benedict, the « cultural themes » of Opler, the « basic personalities » of Linton, the « structure » of Levi-Strauss, Radcliffe-Brown, etc. Often, however, their endeavours have been rendered vain by ontological implications and by the randomness of the indicators. In our opinion participation can be considered only a source of hypotheses.

10. The Social Effects of Prediction. — However, even if the positive stage of studying social « facts » can be gone through by a social scientist, using these methods in the light of the general problematics and overcoming obstacles of varied origin, with an efficiency of cognition comparable with that of other scientific disciplines, he will encounter problems that are quite specific as soon as the tries to make predictions. The impact of a sociological prediction on the public can indeed give rise to reactions capable of modifying the course of events or the probability of the prediction itself being confirmed. If a prediction is broadcast in the form « if A happens then B will happen » the public may seek to hasten or delay the occurence of A (and therefore of B) in accordance with the *value* it attaches to the « fact » B. Alternatively, if A were inevitable the public might act in such a way as either to assist or to hinder the causal process linking A with B by using as a fulcrum those factors that give every prediction a probabilistic character. This may be illustrated by the following purely hypothetical example: Let us suppose that in America it were predicted that « if there is an increase in the income of all Americans, the negroes will raise their level of culture and personal status ». The impact of this prediction [24] on the white public (whose sentiment we will assume for our purpose to be entirely conservative and racist) may then give rise to any of the following reactions:

(1) The whites may act in such a way as to slow down the increase of the American total income in order not to run the risk of their being overtaken by the negroes, i.e. they may act upon the cause A so as not to realise the effect B.

(2) The whites may allow the total income to increase but try in every way to exclude the negroes from benefitting, i.e. they may so act upon the cause A as to distort its predicted outcome of increasing income *erga omnes*.

(3) The whites may allow the income to increase *erga omnes* while seeking in every way to discourage the negroes from making cultural use of it, as by

diverting it to consumer expenditure, hedonistic objects, etc.; in other words they may act in such a way as to impair the causal accomplishment of B following A, due to increasing the number of negroes - an occurrence which in itself would have led to an increased demand for motor cars and television sets instead of for books and education. (In a minor sense this result is already implicit in the prediction itself which had the character of a probabilistic *law* such as that « among every hundred negroes enjoying a gross increase in income, ninety will spend part of it on education »).

Each of these three alternative possibilities represents a particular form of the more general process described by R. K. Merton as « self-stultifying prediction » [25].

Incidentally it should be noticed that predictions which are « self-stultifying » in this general sense do not seem to be limited to « published scientific predictions »; it is clear that the description may apply equally well to any form of prediction capable of arousing confidence and an emotive interest on the part of the public.

In this way the Thomas theorem [26] holds good not only for predictions of a scientific nature but also for *prophecies* properly so called and for *rumours*: here consider the vicissitudes of the Last National Bank in 1932 or the hunting of the poisoner in the Manzoni case.

From the methodological standpoint, what differentiates self-stultifying (or self-fulfilling) processes started by rumours or prophecies from those originated by scientific investigations is the fact that such investigations are able to be used as a basis for predictions *only if it has already been ascertained by experiment or by observation that the facts, when organized in a certain way, point in a certain direction*, whereas rumours and prophecies leave the earlier facts out of account and do not necessarily depend on these.

On this point it may be useful to recall Durkheim's example already mentioned in connection with his rebuttal of pragmatism. Apart from *hypothetical* scientific predictions - meaning scientific predictions not deduced from already verified theories and laws - we find that the only means by which such predictions can be inferred is in accordance with the nomological-inferential model described above. Indeed this is what seems to be meant by Ernst Nagel [27] in his argument that a law predicting that anyone who exposed himself to a certain gas would die is not invalidated by the fact that knowledge of this law has engendered such fear of the gas that nobody does expose himself to it. In this instance the action of the poisonous gas had already been proved. If we are able definitely to accept that sociology arrives at empirical generalizations and theories just like other sciences there is nothing to stop us accepting that the predictions it supplies have the same character as the predictions regarding the action of a poisonous gas. Conversely, while it is certainly permissible to argue that the self-fulfilment (or self-stultification) of rumours and prophecies involves actual achievement (or non-achievement) of whatever reality they assert, it is not permissible to conclude that the predictions are scientific merely because they work out well enough *in practice*. To think in that way would correspond to the example given by Merton in reference to the line of thought followed by someone *postulating* (not predicting) a certain reality in a manner claimed to be « scientific »: this, he wittily remarks, « is like having seen the rabbit being put into the hat and then being contented by the air of triumph with which the rabbit is produced » [28].

There is, therefore, a fundamental need to distinguish between two different things; on the one hand scientific *predictions* made by projecting a known existing trend into the future and on the other hand *programmes of action* whose purpose - whether or not they are prompted by already proven trends - is not prediction but achievement. This corresponds to the customary distinction drawn in German between « *sein* » (being) and « *sollen* » (becoming), or to that drawn between science and technology (or politics).

In political life there also are people who propose *techniques* designed to nip the public's reaction in the bud, based on the principle of publicising the anticipated reactions of the public at the same time as the predictions themselves so that no further reactions will be produced. Essentially this is comparable to expanding the « laboratory » by making its operations more comprehensive. Simon [29] in particular holds that if two candidates A and B are standing for election, of whom it has been predicted that A will win and B will lose, the opposite can be made to happen by the device of publishing a later prediction that B will win - provided it is known that those who are undecided will vote for whichever of the two is expected to lose and it is estimated there are enough of them to swing the balance of the final result. It seems obvious, however, that the prospects could thereupon be restored to what they had been at the beginning by giving fresh publicity to the original prediction which would cause a fresh positive reaction in favour of A who had now become expected to lose, and here the following point should be noticed: If the investigator had kept the first prediction to himself A would have won, but if he gave it publicity he would thereby have contributed to making B the winner. The question therefore arises whether such publicity is not to be regarded as a technical device if contrived with the particular end in view of « swinging back » the victory to A in accordance with the original trend. This question may seem an idle one but in fact it may conceal a nice question in logic and ethics. Two cases can be distinguished: the first where the investigator knows only that social trends T operate in the direction of realising a certain fact F, the second where he knows also how the public would react if it were made aware that its actions were carrying it towards F. Let us suppose that these reactions would be negative. Then in the first case there would be a counter reaction and F would not be realised; but if in the second case the investigator were to announce conclusively that « the fact F will not be realised » it might on the contrary be realised nevertheless, because the public would not know that F was a consequence of T.

It seems then that if *technical predetermination* or *manipulation of opinion* is to be eschewed and only scientific prediction is to be allowed the original prediction ought always to be declared in one way or another. While leaving this an open question, the present writer is of opinion that in the second of the two cases described, such indications might perhaps amount to manipulation, seeing that they could lend themselves to being turned into advice or stimuli instead of being limited to the provision of a forecast.

Neurath, referring specifically to the « confused situations » that confront the sociologist in such cases, mentions the following example as a parallel: «a scientist might predict that a meteorite will fall at a certain spot and kill some people. He might publish his prediction, in which case the population would move away and the prediction would not be in harmony with future observations proposed for testing it; but if he merely recorded his prediction in his notebook these tests might have been successful. If, however, he had preferred to say that the population would already have moved away before the meteorite had arrived his prediction would once more have been contradicted by the outcome of the proposed observations, since the people concerned might have been suspicious and, thinking that the scientist had reasons of his own for wanting them to go away, might have remained where they were » [30].

In conclusion, the author considers that when a technologist picks up a law which has already been scientifically confirmed and *applies* it all he is doing is to procure deliberately the conditions under which the law will operate in the predicted direction. In sociology, however, the parallel to this involves not only ensuring the due performance of a technical operation but also participation in politics, which is something that goes far beyond concern with scientific knowledge. Furthermore, quite apart from the manipulatory character of political action there is the point already made at the beginning of this paper that neither technology nor politics add anything (except indirectly) to the scientific knowledge, *already* derived from laws, which makes such action possible in the first place. Nagel himself, in order to contend that social acts are predictable, falls back on the validity of the laws from which the predictions are deduced. He says that a statement proposed for acceptance as a law must, as regards its logical form, be a conditional statement, for its validity depends on the terms in which it is expressed: « if certain conditions are satisfied *then* certain other things will become true » [31].

In this respect too much stress is laid on the need for predictions to be confirmed, seemingly as a result of Dewey's philosophy which overemphasizes the importance of truth being verified pragmatically. Our own opinion is that sociological work is scientific only up to the point where its original interpretation is published, after which the sociologist should not be concerned except in directing attention to new research work, handing over to society and the politicians the decision whether or not to create the conditions necessary to confirm later the laws from which the prediction was deduced.

11. FURTHER PROBLEMS CONNECTED WITH THE FOUNDATIONS OF SOCIOLOGY. — According to A. Comte the object of sociology is to investigate the actions and reactions that occur in the various parts of the social system [32]. According to H. Spencer its objects is to explain how successive generations of social units are produced, educated and adapted for cooperation [33]. E. Durkheim considers that sociology should study the connections between the causes and effects of social phenomena and he further maintains that the effects acquire their momentum from their causes and have to recuperate it from them when necessary; hence the causes cannot disappear without this affecting the phenomena [34]. Finally, M. Weber argues that sociology should denote a science which seeks to arrive at an understanding of social actions by means of an interpretive procedure and which then goes on to explain its causation, the course it follows and the effects that ensue [35].

It is not merely for the sake of listing them that these four authors are cited here but because they represent the four main directions in which sociology has developed from its early beginnings to the present day. That is to say, along these lines a kind of universe of epistemological conceptions has grown up all of which have been, and some of which still remain, basic to the sociological schools of thought most likely to leave an abiding mark and to impel serious, well defined and often contrasting developments of theory and research.

In the position taken up by Comte one can recognise, explicitly and unmistakeably, the stamp of the positivist setting out to discern rigid laws in sociology like those to which every branch of natural science was giving birth in his century. In the work of the other great positivist, H. Spencer, the concept of system which is typical of the physical sciences is reinterpreted with an organic keynote, the component parts of that system being thought of as *organs of the social body*.

The positivism of Durkheim was the basis also of a particular sociological methodology, responding to the need for recognition of laws, causes and effects while at the same time highlighting the functional aspects of social facts (here again implying a view of society which is in some sense organic). This not only cleared the way for a structural-functional tradition in sociology but also established its position alongside anthropological-cultural theorising.

The thinking of Max Weber provides evidence of his own epistemological conviction, which many still share, according to which human actions can be explained by their motivation if an effort is made to understand their historical background, such understanding being rendered possible by the fact that human actions are intentional. However, the method recommended by Weber for furnishing general truths is subject to a limitation since according to him the only available source of such truths is empirical knowledge, so this method cannot be applied to unconscious motives. Weber holds that recognition of such motives would impose on the social sciences the status of natural sciences concerned with human action, whereas in reality the social sciences being oriented towards the study of intentional actions cannot be described otherwise than as nomological sciences of the mind [36].

So far as concerns the working methods that have been evolved from the various standpoints here reviewed it seems clear in conclusion - as has already been suggested earlier in this paper - that positivism, social evolutionism, functional positivism and historicism all represent endeavours to perceive uniformities (laws) which interconnect social facts in such a way that social phenomena can be described and explained.

Actually each of the methodological trends to which passing reference has been made here differs from the others as regards the theoretical claims superimposed upon the central purpose of sociology it expresses. As these claims are often without the necessary gradation they have tended to imply that the uniformities (laws) discovered or taken as axiomatic apply over so wide a range as to be all-inclusive. Except for some of Weber's work the results obtained have generally been like self-sufficient pieces of architecture, and the possibility of extracting from them whatever were methodically and theoretically the best elements has depended on the ability to redefine them in terms of new conventional and securely objective concepts.

It is no accident that R. K. Merton, author of a book laying particular stress on the need for sociology to proceed by steps regulated on the one hand by the scientific (logical-experimental) method and on the other hand by « middle-range theories », has quoted as an introduction to this fundamental work Whitehead's notable saying that a science which hesitates to forget its founders is lost.

To sum up what has been demonstrated up to this point, whichever way we select as an approach road to reality - in this instance social reality - the approach must be directed towards the attainment of generalized knowledge or of descriptions, explanations or predictions that have a general and objective character. However, social thinking is seen to be always ruled by the conviction - implicit if not expressed - that belief in some kind of uniformity or laws necessitates that a certain number of cases should have been examined. To recognise this necessity is to acknowledge that the only adequate basis for truthful nomological constructions is a procedure which relies upon experiment and induction. It is a small step from acknowledging this to recognising the fundamental value of statistical techniques (meaning mathematics applied to repeatable phenomena). As Lazarsfeld has observed, Max Weber himself was sometimes an enthusiast for quantification and he made many calculations himself; Tönnies invented a correlation coefficient of his own; Wiese regularly led his students through villages, during the Easter vacations, in order to see concepts relative to social relationships in action among peasant families [37]. Obviously these instances might be multiplied so as to include among others those of Comte [38], of Durkheim in his book *Le Suicide*, of Le Play and so on.

If sociology is to offer the same guarantees of validity as the other empirical sciences a rigorous scientific method must be adopted, which can be no different from the method used in those. The obstacles to this are twofold, on the one hand there is the difficulty of defining sociological concepts satisfactorily and objectively while on the other hand, even if these were overcome, there would still remain the fear of producing interpretations of reality which, even if valid, would be lifeless and devoid of critical inspiration, charged rather with political conservatism of which their propounders might not be aware.

12. The Use of Definitions in Sociology. — The first point that arises under this head concerns problems whose solution demands that the sociologist should be able, by conventional means, to *objectivise* his own definitions of sociological objects, relinquishing any metaphysical pretension to *discovering* an inherent objectivity in the objects themselves.

When the point is stated in this way it becomes clear that scientific definitions have an indispensable part to play in all sciences. Linguistic terms designating scientific objects must convey the same meanings throughout the whole body of scientists. The simplest way of achieving this is to employ « *operative definitions* », meaning statements which indicate objects in such a way as to make it possible not only to confirm them by reference to phenomena but at the same time to test their consistency and to measure them.

The use of « non-operative definitions », meaning definitions that do not imply the acceptance of an immediate obligation to verify them, is permissible only in cases where an empirical process of objectivisation, or a procedure for relating them to operative definitions, may be presupposed. Sometimes they become necessary, as « hypothetical constructions », in cases where a definition or a law exhibits a high degree of validity except for some particular aspect which is incompletely covered: a definition of this provisional and accessory type may then be used to fill the gap.

In all other cases the founding of a whole definition, hypothesis or law on a non-operative definition would mean dropping back into an ambit far removed from scientific procedure, and ultimately into metaphysics. As Blalock has expressed it, « scientists think in theoretical language but verify their hypotheses in operational language; and although it is not necessary to discover operations corresponding to each and every theoretical concept it is important to prevent concepts that are not operationally defined from insinuating themselves into statements whose verification is invited. Should that happen the problems contained in the « hypotheses » will generally be meaningless and give rise to interminable arguments » [39].

As an example of theoretical definition Benoit-Smullyan's use of the word *status* to denote position within a hierarchy may be cited [40]. In its primitive form his definitions does not, as such, amount to a useful instrument for scientific research whereas Warner's « index of status characteristics » (ISC) and Chapin's « resident space scale » are examples of procedures used for the purpose of deriving operative definitions of social status [41]. Here is one more illustration which makes it easy to see how a concept such as *status*, although in itself a methodological and not an ontological one, can be made objective by reconnecting it with observable empirical processes whereby the concept of a « datum » comes back into play instead of « isolated experiments » or « undecomposable propositions » which would imply an ultimate reality. In other words a sociological concept is effectively defined in ordinary life provided that evidence of it can be obtained by the use of empirical indicators.

13. THE PROBLEMS OF VALUES. — The second of the obstacles mentioned is the imputation to logical-experimental sociology that it is « non-critical », tending therefore to lose itself in mere political conservation of the *status quo*. According to Mills the aspirations of present day sociology are such that it issues in an abstract empiricism [42] intended to be implemented by political actions. This position is shared by other authors with more or less Marxist leanings such as Lucien Goldmann [43], Noam Chomsky and Herbert Marcuse. The attitude of Gunnar Myrdal « is that we need to have points of view and that these presuppose valuations which reduce the notion of a disinterested social science to utter nonsense, something that has never existed and never will exist » [44]. In Italy, Antonio Santucci, picking up in his introduction to Habermas [45] a dagger frequently used for attacking sociology in the form endorsed here, seems to identify it with a technique applicable to the management of the existing set-up (« *omogena a una prassi che gestisce tecnicamente l'esistente* ») and Bagolini [46] affirms that it « would be easy to give examples of situations where the evaluation and choice of ends predetermines both the directions and the conclusions of empirical sociological investigations, imparting either a conservative slant to justify the powers already vested in existing or leading groups or, on the contrary, a progressive and revolutionary slant».

Each of these positions is founded upon an inbuilt conviction that conditioning (of statements) by values is unavoidable and that even if this influence could be avoided such investigations directed « *ad finem* » could have no useful outcome but lead only to a servile conservatism demonstrating the impossibility of objective research.

The present author's opinion is that by using the logical-experimental method it is indeed possible to arrive at scientifically warranted sociological knowledge. Except within certain limits, he sees no real basis for the assertion that value judgements are unavoidable because, as Freyer put it, sociology is « society's study of society ». What matters is to agree on the meaning of the term neutrality. In our opinion it does not mean apprehending the nature of things in the sense of attaining that intimate and unique objectivity which penetrates to the essence of objects in the real world and which may be perceptible by some people as the truth whereas others, less fortunate, see only its distortion. In our opinion it makes no sense scientifically, but only metaphysically, to speak of the nature of an object; what we understand scientifically by an « object » is nothing other than the empirically based conceptual definition of that object.

Essentially it is a matter of convincing ourselves that objectivity - the « indifference and attentiveness » of Freud and the *Wertfreiheit* of Weber - is not attainable on the plane of reality but only on the plane of convention, the plane whereon empirical experiences are formulated. On that plane, objectivity - human agreement - is achieved by accepting as objective the conventional semantic « relationship » between the objects and the terms that designate them. Scientific objects are so defined as to limit their extension, and also to limit those of their connotations that originate in observation, in such manner (though in varying degree) as to make them verifiable in the sense that the real existence of the characteristics predicated in the definition is able to be observed.

Observation in its turn is exposed to the same danger of being conditioned by value judgements, but the risk of this is lessened in proportion to the preciseness of the definition. Ultimately the introduction of the scientist's working methods into the logical-experimental procedure would appear to leave no room to doubt the objectivity thereby imposed; for in fact the criterion of verification is canonised in the formal processes of induction and deduction to be gone through in a series of actions, such as application of the logical-mathematical and statistical techniques now available, which are manifestly neutral.

In conclusion, it seems just as absurd to assert the impossibility of maintaining neutrality in the conceptualisation and conduct of sociological researches when using the logical-experimental method as it would be to assert that the sciences of physics and chemistry (although not their uses by man) are vitiated in the same way. The fact is that these sciences, and likewise sociology when the effort is made to follow their lead as regards method, are examples of the most perfect form of knowledge in their respective fields that ever has been attained by whatever means. Apart from the considerations briefly adumbrated up to here, it should indeed be noted that if the impossibility of eliminating value judgements were accepted then sociology would have no alternative open to it that could be called scientific. To acquiesce in its researches being conditioned by values, while making every effort to identify itself as much as possible with whatever units (groups, society as a whole, communities, etc) it was studying, would mean abandoning itself to a vacuous introspection from which the sociologist would be able to draw out only existential and not scientific truth. For instance, there is an anthropological rather than sociological kind of approach which seems not to appreciate the importance of relying exclusively on the use of questionnaires and which, in investigations of labour, prefers to substitute these by considering a « business life » as viewed from within. The present writer maintains on the contrary that an exclusively « participating » methodology such as that is unpromising if it purports to offer anything more than a way of constructing hypotheses - for indeed: (1) who can say *how long* it is necessary to be immersed in a business before one can claim to know the life of that business? (2) to what extent must one *belong in* it? (3) when the contact from within has been established *whose thought* is one expressing? one's own? that of the workmen? that of the technical staff? that of the management?

14. Neutrality of Science, not of Scientists. — Coming now to the crux of the accusation of « conservatism » (*conservatorismo*) levied against science it can be stated as the author's opinion that a sociologist subscribing to the logical-experimental school is, in his work, just as « conservationist » as it is possible for a physicist to be when enquiring into sub-atomic processes or a psychologist when investigating the learning process in children.

It is undoubtedly true in general of the empirical sciences, considered as forms of knowledge and not as interventions in the stream of reality, that they do not modify and ought not to modify in any way the objects they are investigating; but it is difficult to see why the charge of « conservationism » should be brought against an analyst of society who is setting out to discover its laws and its current trends but who afterwards hands them over for the politicians to use, or who uses them himself in a personal intervention outside of his science, with a view to modifying them.

There is a problematic side to everything that exists, which cannot simply be jumped over from fear that the study of it in a scientific fashion might tempt one to ascribe to it a dignity and validity which moral judgement would repudiate. Quite often an attachment to critical methodology along with prejudices and « scholastic » doctrine may be the very factors that can create in those who believe in them a more distorted view of the real world, at the cognitive level, than would be imparted to them by more profound scientific examination.

There is more substance in the allegation that often such studies are promoted by private interests; but here it should be remarked that it is sociologists rather than sociology who should be accused. It cannot in fact be held that the method followed in science (which is a sound one) is inherently conservative and servile, the fact being that it is only by genuine knowledge and not by dogmatic political beliefs that it is possible to satisfy the interests of all concerned, as well as certainly all existential needs. The circumstance that during the fascist and nazi period the genetic sciences received powerful stimuli at the expense of the psycho-social and environmental sciences, and that at the present time the exact opposite seems to be happening - not only in collectivist countries - does not mean that the knowledge won (and confirmed) under these two different climates of cultural conditioning has to be characterized as pseudo-knowledge. Psychology, for instance, has been advanced by the studies of chromosomes as well as by discoveries derived from environmental and psychosocial theories. The problem, therefore, is not one of « truth » (versus falsity) but one of a balance between the directions in which research is made to proceed.

As will be shown below, cultural values exert an impact which may be either empirical or theoretical at two fundamental levels of social research: the level at which objects (problems) are chosen for study and the level at which hypotheses (or correlations between laws and theories) are worked out. To give a concrete example, the qualms of conscience felt by Oppenheimer and the other atomic scientists were not essentially concerned with physics but with the relations between physics and society brought about through the intervention of physicists. Likewise the efflorescence of researches on small social groups, or those on social mobility in industrial societies, may well be expressions of a general policy in science tending to look with greater favour on studies of this kind than on studies concentrated upon what may be more serious problems, such as those of overdeveloped societies. The upshot from all this is that while it is certainly true to say that a sociological method of the « critical » or « Marxist » type operates in opposition to the existing state of affairs, this being essentially the source of its fascination and appeal, it cannot be absolutely maintained that the logical-experimental method *as such* might be used in defence of that state.

15. THE FORCE EXERTED BY VALUES IN THE CONSTRUCTION OF HYPOTHESES AND IN THE CHOICE OF PROBLEMS. — As already stated it is in the formulation of hypotheses that an impairment of « pure » scientific intuition might conceivably occur as a result of prejudices, ideologies and particular needs: for instance, an investigator with a racist inclination will conjecture an inferior scholastic performance of black children, believing them to have an intrinsic inferiority correlatable with the colour of their skin.

Provided that such a hypothesis is limited to being an « operational » one (or assuming the case can be tested empirically) its influencing by values does not, as such, result in any distortion of reality because the hypothesis (that children with a black skin are less successful at school) can be properly verified or negated. But it is only by trying to fault the hypothesis that it can be found out whether the skin colour is a pseudo-cause (a « provisional » and « heuristic » cause) and be discovered that the education of the parents plays a part as an independent variable. The latter in turn may be found to be correlated with the income level of the black families. It must also be presumed that these three investigations *may* start from differing points of view or differing ideologies held by different investigators concerned: but if so, provided they have started from operative definitions and the hypotheses developed from them are such as can be checked, they will have done no harm to science.

Thus scientific knowledge of sociology, like that of any other field, takes shape as an integrated pattern of laws and (verified or verifiable) theories formulated at various depths, each of which is valid *in the terms used for expressing it* and it is only by allowing free play to the possibilities of disproof that the laws and theories can be raised to levels of greater accuracy and extension.

Another point (precedent to the former in time) at which there can be no doubt that an investigation may (generally though not necessarily) be influenced by values is the choice of problems for study. Values are of many different kinds. Questions and requirements of social, political, hygienic or economic relevance impel scientists and people in general to rate some of them as factors of reality that ought to be studied, and a selection is made accordingly from the enormous and multifarious range of case observations on record. Here again it is obvious that the relationship to value judgements does not affect the objectivity of the research in any way, but this does raise real problems worthy of consideration in the field of science policy. As Talcott Parsons maintains, scientific inquiry is itself an « active process which consists in the pursuit not of knowledge in the abstract but of a particular kind of knowledge of particular objects. As regards its data it is a process of selection governed either by the structure of theoretical systems of by extra-scientific considerations » [47].

Over and beyong values and necessities, the scientist's characteristic motive in his choice of problems to investigate still remains, in this author's opinion, that same inexhaustible curiosity which led him to adopt the pursuit of knowledge both as a profession [48] and, as H. Greniewski [49] declares, a sort of ever repeating game « played between the research scientists on the one side and the purpose of the research on the other ». To conclude: science is one of the many ways that man can confront reality. Just as the artist, the politician, the missionary and the technologist, so also the scientist has his own selective vision, his own sensitivity, which urge him always in pursuit of knowing something more.

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[15] Cf. R. König. - « Sociologia », Milano, 1964, p. 149.

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N. Rescher. - *On prediction and explanation* in « The British journal for the philosophy of science », 7 (1957); for a discussion of this reasoning see G. Di Bernardo. - « *Previsione e profezia nelle scienze sociali* », in « Sociologia », Roma, nuova serie, anno IV, n.2, on which the summary here is based.

[19] op. cit., p. 44.

[20] In the sense of establishing statistical *trends* or empirical tendencies. Cf. R. K. Merton - « *Teoria e struttura sociale* », Bologna, 1959, pp. 153-154.

[21] R. K. Merton, op. cit., p. 137 et seq.

[22] R. K. Merton, op. cit., pp. 155-156.

[23] The cross lines are to indicate that models of any nomological-inferential analysis can always be related to other models of the same kind.

[24] Supposedly based upon laws which have been widely confirmed beforehand in reference to sample cases of negroes whose incomes have risen.

[25] Cf. R. K. Merton, op. cit., pp. 598 et seq. In the contrary case Merton speaks of « self-fulfilling prophecies ».

[26] Stating that «if people define certain situations as real the consequences of those situations will be real ». Back reference (in translation) to Merton, op. cit., p. 598.

[27] Cf. E. NAGEL. - « *La struttura della scienza* », Milano, 1968, p. 483.

[28] op. cit., p. 606.

[29] H. A. SIMON. - *The effect of predictions*, in M. Brodbeck « *Readings in the philosophy of the social sciences* », New York, 1968, pp. 447-455.

[30] O. NEURATH, op. cit., pp. 99-100.

[31] E. NAGEL, op. cit., p. 482.

[32] A. COMTE. - « *Cours de philosophie positive* », Paris, 1842, vol. IV, p. 327.

[33] H. SPENCER. - *Principi di Sociologia*, Torino, 1967, Vol. I, p. 494.

[34] E. DURKHEIM. - « *Le regole del metodo sociologico* », Milano, 1963, pp. 95-96.

[35] M. WEBER. - « *Economia e società* », Milano, 1961, p. 4.

[36] J. HABERMAS. - op. cit., p. 20.

[37] P. F. LAZARSFELD. - « *Metodologia e ricerca sociologica* », Bologna, 1967, p. 953.

[38] Who, according to Geymonat, not only hated mathematics but was himself detested by mathematicians. Cf. L. GEYMONAT. - *Comte e l'unificazione del sapere* in « Il Pensiero », n. 1-2, Jan-Aug., Milano, 1965.

[39] H. M. BLALOCK JR. - « *Statistica per la ricerca sociale* », Bologna, 1969, p. 25.

[40] BENOIT-SMULLYAN. - « *Status, status types, and status interrelations* » in « American Sociological Review », vol. 9, Apr. 1944.

[41] H. M. BLALOCK JR., op. cit., p. 23.

[42] C. W. MILLS. - « *L'immaginazione sociologica* », Milano, 1962, pp. 60-83.

[43] L. GOLDMANN. - « *Scienze umane e filosofia* », Milano, 1961.

[44] G. MYRDAL. - « *The relations between social theory and social policy* », in « British Journal of Sociology », XXIII (1953), p. 242.

[45] J. HABERMAS, op. cit., p.XI.

[46] L. BAGOLINI. - « *Mito potere e dialogo* », Bologna, 1967.

[47] T. PARSONS. - « *Lè struttura dell'azione sociale* », Bologna, 1962, p. 459.

[48] CF. M. WEBER. - « *Il lavoro intellettuale come professione* », Torino, 1967.

[49] H. GRENIEWSKI. - « *Le concept d'information dans la science contemporaine* », Paris, 1965.

HISTOIRE DE LA LOGIQUE

LES ÉTAPES DE SON ÉVOLUTION

Deuxième Partie

Cette étude est la deuxième partie de l'article publié dans le numéro précédent de "Scientia". L'auteur continue l'analyse des étapes historiques par lesquelles la logique est arrivée à son état présent. On examine la logique des Stoïciens, la logique à Rome et la formation de la terminologie, la scolastique des commentateurs et enfin la scolastique médiévale. Des problèmes presque oubliés, ou à peine mentionnés jusqu'à présent, sont présentés au lecteur: les parva logicalia, *avec le problème de la* suppositio, *des* syncategoremata, *des* insolubilia, *etc. La caractéristique de la logique scolastique est la dualité bien marquée de la* sermo *(parole) et* ratio *(raison). La scolastique unit la logique des Péripatéticiens à la logique des Stoïciens. La logique de la Renaissance nous offre aussi des aspects multiples mais encore moins connus que ceux de la logique du Moyen Age. On passe ensuite à la méthodologie, due à l'essor des sciences. On examine ensuite les courants "philosophiques": la logique de Kant, la logique de Hegel, de Marx - Engels, de Husserl, le courant psychologiste, théories des valeurs, etc. Enfin un grand chapitre est consacré à la logique mathématique, dont on montre la valeur et la portée. Un dernier chapitre conclut cette étude, dans lequel l'auteur montre que la logique n'est pas une science quelconque, mais, ainsi que l'affirmaient les scolastiques,* l'ars artium, scientia scientiarum, doctrina doctrinarum. *C'est une science purement contemplative, ayant un caractère spécial, parce que l'objet de la pensée est la pensée elle-même.*

VIII. - LA LOGIQUE APRÈS ARISTOTE. — L'école péripatéticienne tend, après Aristote, de plus en plus à être préoccupée du rapport des jugements entre eux et moins du rapport des concepts. Prantl remarque très bien (*Geschichte der Logik im Abendlande*, I, p.351), que « la fonction logique de la proposition commence à céder le pas à la fonction grammaticale ».

Cet objectif est poursuivi par les successeurs immédiats d'Aristote, Théophraste et Eudème.

Ils s'occupent surtout du syllogisme hypothétique, du *modus ponens* et du *modus tollens*.

Les préoccupations formalistes étaient dans l'atmosphère du temps et elles atteindront leur point culminant avec les Stoïciens.

IX. - LES STOÏCIENS. — Les historiens de la philosophie sont tous d'accord que la période post-aristotélique de la philosophie grecque est caractérisée par un pragmatisme croissant.

De ce point de vue on peut dire que ce qui est spécifique à la philosophie - et donc à la logique post-aristotélique - est un intérêt de plus en plus grand pour le côté pratique de la philosophie et un intérêt moindre pour les spéculations théoriques. Les principales écoles post-aristotéliques, l'école stoïcienne, d'Epicure et des Sceptiques, sont toutes caractérisées par cet aspect pratique de leur philosophie et, par conséquent, de leurs théories logiques.

L'école stoïcienne fut fondée par Zénon de Cytium (fin du IV^e siècle av.J.Ch.). La personnalité la plus puissante de cette école est Chrysippos (281 - 208), surnommée "la colonne du Portique". Celui-ci consolida l'école, ainsi que nous le dit Cicéron (*Les Académiques*, II, 24): *Chrysippus qui fulcire putatur porticum Stoicorum* ("Chrysippos considéré qu'il a consolidé le Portique des Stoïciens"). Diogène Laërce (*Les vies et les opinions des philosophes illustres, VII*, 180) nous dit qu'il a été un dialecticien si rénommé qu'on pensait "que si les dieux faisaient de la dialectique, ils ne feraient que celle de Chrysippos". On attribue a ce grand dialecticien de l'antiquité plus de 311 livres, beaucoup d'entre eux ayant un caractère logico-grammatical.

La logique prend dans la conception stoïcienne une importance tout à fait particulière, étant considérée comme une "vertu". Dans ce sens, Diogène Laërce écrit (op.cit., VII, 47): "La dialectique, disent-ils (les Stoïciens), est indispensable et est en soi une vertu (*areté*) qui comprend d'autres vertus particulières".

On trouve chez les Stoïciens, pour la première fois, l'appelation "logique" (les autres l'appelaient "dialectique"). La logique était divisée en rhétorique et dialectique. La dialectique, à son tour, comprenait deux grandes sections: 1) la signification - σημαινόμενον (*significatio*); 2) l'expression - σημαῖνον (*verbum*). Chacune de ces sections était divisée en plusieurs autres, dont on peut donner la liste suivante:

Logique

Rhétorique

Dialectique

Verbum	Significatio
1. Physiologie du langage	1. Théorie des critères
2. Grammaire	2. Théorie des concepts
3. Poétique	3. Théorie des catégories
4. Théorie de la musique	4. Théorie des propositions
	5. Théorie des raisonnements

Nous ne pouvons entrer ici dans l'étude de toutes ces divisions, mais nous dirons deux mots sur leur conception du rapport entre la logique et le langage et le raisonnement hypothétique.

1. *Logique et langage.* — Pourquoi les Stoïciens donnaient-ils une telle importance aux recherches logico-grammaticales? Cela était dû à une conception spéciale du rapport entre la pensée et le langage. Pour les Stoïciens la pensée ne pouvait être conçue que dans son expression verbale. Le principe dirigeant de l'existence

humaine (*hegemonikón*) était placé par eux dans le coeur d'où "radiait" un souffle jusqu'au larynx et le mettait en vibration en produisant le parler. Penser et parler étaient si étroitement liés entre eux que l'une de ces facultés ne pouvait fonctionner qu'en se basant sur l'autre. Le parler s'identifie ainsi avec le penser et n'est que la pensée dans son explicitation.

Malheureusement nous ne possédons pas les textes stoïciens dont on pourrait se faire une idée plus claire des développements de cette conception. Cependant nous pouvons dire que c'est trop simplifier pour affirmer que les Stoïciens étaient nominalistes et rien de plus.

Nous ne pouvons pas affirmer que ces philosophes étaient les logiciens du signe, comme on l'a prétendu de notre temps (surtout Lukasiewicz). Nous avons, d'ailleurs un texte de Boèce (*De Syllogismo Categorico*), qui nous explique suffisamment le rôle et l'emploi des signes: *Quotiescunque ita dicimus ut litteras pro terminis disponamus, pro brevitate hoc et compendio facimus* ("Chaque fois quand nous nous exprimons ainsi, que nous mettons des lettres à la place des termes, nous faisons cela comme abréviation et économie de temps".)

2. *Raisonnement hypothétique.* — Les Stoïciens partageaient les propositions en deux catégories: (a) les propositions simples (ἁπλᾶ ἀξιώματα) et (b) les propositions non-simples (οὐχάπλᾶ ἀξιώματα). Ce sont les propositions *atomiques* et *moléculaires* de Russell.

Les propositions non-simples étaient au nombre de sept:

(1) Proposition hypothétique construite avec la conjonction: "si", exemple: "S'il fait jour il y a de la lumière".

(2) Proposition adjointe construite avec la conjonction "parce que", exemple: "Il fait jour parce qu'il y a de la lumière".

(3) Proposition copulative, construite avec la conjonction "et", exemple: "Il fait jour et il y a lumière".

(4) Proposition disjonctive, construite avec la conjonction "ou", exemple: "Ou il fait jour ou il fait nuit".

(5) Proposition causale, construite avec la conjonction "puisque" (à cause de), exemple: "Il fait jour puisque (à cause) il y a de la lumière".

(6) Proposition comparative, construite avec la conjonction "que", exemple: "Il fait plus de jour que de nuit".

(7) Enfin, Diogène Laërce cite la proposition non-simple formée avec la négation, exemple: "Il n'est pas jour".

Les Stoïciens se sont occupés de toutes les possibilités de construire des propositions composées (non-simples). Bien que nous ne connaissions pas toutes ces propositions composées en détail, nous pouvons déduire néanmoins que le nombre des propositions composées, étudiées par eux, devait être énorme, si nous en jugeons d'après une information transmise par Plutarch dans *Questions de Symposium*

(VII,9). Il nous dit que Chrysippos, dans *Les définitions dialectiques* aurait soutenu qu'avec seulement les propositions simples on peut faire plus d'un million de propositions non-simples.

Ce qui est important dans toute cette discussion est que toutes ces propositions non-simples sont vraies ou fausses en fonction seulement des valeurs de vérité des propositions simples composantes.

Ils connaissaient donc très bien les fonctions de vérité de la logique mathématique actuelle.

L'étude des propositions était l'antichambre de l'étude du raisonnement. Les Stoïciens considéraient que les syllogismes catégoriques, dont s'est occupé largement Aristote, étaient incorrects du point de vue formel, mais corrects en ce qui concerne leur matière (Alexandre d'Aphrodise dans son Commentaire *Ad Analytica Priora*). Ils partageaient les raisonnements en deux catégories:

(1) Arguments démontrables, qui ont besoin d'une démonstration pour être valables, et ce sont les divers syllogismes démontrés par Aristote dans *Les Premiers Analytiques*.

(2) Arguments sans démonstration, qui sont les modes de démonstration stoïcienne.

Il y a cinq types de raisonnement stoïque ainsi que nous transmet Diogène Laërce et Sextus Empiricus. Ces cinq types de raisonnement sont fondamentaux "auxquels on peut réduire tous les autres".

Les Stoïciens étudiaient ces raisonnements d'après leurs formes et ils appelaient forme du raisonnement "la figure", c'est-à-dire, leur forme purement schématique, la forme de leur expression verbale.

Voici ces types de raisonnement:

I. Si le premier est, alors le deuxième.
Mais le premier est
Donc le deuxième est

II. Si le premier est, alors le deuxième
Mais le premier n'est pas
Donc le deuxième n'est pas

III. Le premier n'est pas en même temps avec le deuxième.
Mais le premier est
Donc le deuxième n'est pas

IV. Ou le premier est ou le deuxième
Mais le premier est
Donc le deuxième n'est pas

V. Ou le premier est ou le deuxième
Mais le deuxième n'est pas
Donc le premier est.

Ces "formes", qui - comme nous l'avons vu - ne sont employées que pour "économie de temps et abréviations", ont été prises en elles-mêmes par les logiciens mathématiciens, et Lukasiewicz soutient qu'il n'y a pas de forme déductive en dehors des formes littérales... Ces logiciens sont allés plus loin encore et ont traduit les cinq raisonnements fondamentaux des Stoïciens sans la copule "est": "Si le premier, alors le deuxième; mais le premier; donc le deuxième". Benson Mates reproche à C. Prantl la traduction de ces raisonnements avec "est" comme "la plus grande confusion dans la compréhension de la logique stoïcienne" (B. Mates: *Stoic Logic*, p. 88, Berkeley and Los Angeles, 1961).

Nous croyons que les anciens étaient beaucoup plus près de la compréhension du raisonnement stoïcien, et dans ce sens nous pouvons citer la traduction de Boèce:

Si A est, est B
Atqui est A
Est igitur B

La copule y est toujours.....

Il est vrai que la logique des Stoïciens diffère de la logique d'Aristote. Où est la différence? Elle consiste dans le fait que tandisque la logique d'Aristote est une logique du concept, la logique de Crysippos est la logique des propositions.

Cette différence apparaît surtout dans le domaine du raisonnement: le raisonnement stoïcien est propositionnel tandisque le raisonnement aristotélicien est conceptuel.

Mais tous les deux sont légitimes.

X. - Rhéteurs et Commentateurs. La Scolastique antique. — Après les Stoïciens, deuxième grande école de l'antiquité, il n'y a pas grande chose à citer.

On travaille sur le capital acquis, d'une manière ou d'une autre.

La logique se transforme dans l'école d'Epicure dans une théorie de la connaissance, la *Canonique*; elle prend un caractère négatif dans l'école sceptique et devient une conception probabiliste dans la Nouvelle Académie.

Enfin, la logique passe à Rome, où les besoins rhétoriques ont déterminé l'étude de cette science. Il y a une période de formation du langage logique dans la langue de Virgile, la terminologie étant hésitante et incertaine. La prédominance des conceptions stoïciennes est visible et donne une certaine empreinte à la logique latine, qui influencera plus tard les vrais commentateurs. Cicéron s'efforce de composer quelques traités de logique avec une terminologie approximative.

C'est ainsi que se forme une véritable scolastique antique, en travaillant sur l'héritage d'Aristote et des Stoïciens, due aux commentateurs dits "anciens": Boethius de Sidon, Alexandre Aegeus, Aspasius, Adrastus, Herminus et autres.

Les commentateurs suivants, soit grecs soit latins, sont des "éclectiques" (péripatétisme et stoïcisme) et quelques-uns - qui prétendent être des péripatéticiens purs - ne sont pas même conscients de leur éclectisme. Il y a ici à rémarquer - dans cette période, qui dure jusqu'à Boèce (480-524 ap.J.Ch.) - que dans cette période de "préscolastique" apparaissent comme constitués les sept arts libéraux, arts qui joueront un rôle si important au Moyen Age. On doit citer, dans la formation de la préscolastique, les noms de commentateurs: Appulée, Galien, Pseudo-Galien, Alexandre d'Aphrodise, Marius Victorinus, Augustin, Marcianus Capella, Boèce, Cassiodore.

La scolastique antique a quelques caractéristiques que nous pouvons synthétiser de la façon suivante.

1. Pas de découvertes logiques ou de conception nouvelle - en dehors peut-être de celles attribuées à Galien, la quatrième figure du syllogisme et la théorie de l'équipollence.

2. La logique aristotélicienne et la logique stoïcienne deviennent complémentaires, mais elles ne forment pas une synthèse et sont acceptées simultanément, comme représentant chacune une modalité de la pensée.

3. Sous l'influence de la conception stoïque la logique perd son caractère éminemment ontologique.

4. Sous la même influence stoïque, les logiciens deviennent intéressés des classifications interminables et de l'études des formes verbales classifiées de toutes les façons possibles.

5. C'est cette tendance, d'étudier les formes verbales et de les classifier, qui est à l'origine de la terminologie scolastique.

6. La terminologie logique est complètement établie.

7. Enfin, nous relevons le fait très important, que la logique est un art libéral et fait partie des connaissances de l'homme cultivé de cette époque.

8. La logique scolastique a ainsi une base naturelle et n'est que l'apogée de la scolastique antique.

XI. - La logique scolastique. — La logique de cette époque a été longtemps négligée et même dédaignée, de sorte qu'on n'a pas exactement su qu'est-ce qu'elle représente.

La réévaluation de cette logique a commencé par les travaux du logiciens polonais J. Lukasiewicz, qui a montré qu'une bonne partie de ce qu'on appelle aujourd'hui "le calcul propositionnel" était bien connue des logiciens scolastiques.

On a cru que l'époque scolastique était une époque morte, de domination de l'église, et où on ne pourrait rien trouver de valable. Mais il y a un univers scolastique, en pleine effervescence intellectuelle, où l'on travaille les problèmes les plus importants de la philosophie et de la logique. L'erreur fondamentale commise dans le jugement de valeur sur cette époque a été de confondre toute la production intellectuelle de ce temps avec la théologie. Ce qui est faux.

Le Moyen Age est surtout rationaliste. Le domaine entier de la philosophie accessible à l'intelligence humaine est rationnel et il n'y a pas de philosophie en dehors de la raison. C'est ainsi que nous dit Albert le Grand: "Le philosophe doit dire ce qu'il dit, avec raison" - *id quod dicit, dicere cum ratione.* Cela demandait une parfaite connaissance de l'instrument logique, d'où l'ampleur des recherches logiques.

Cette caractéristique du Moyen Age, le rationalisme à outrance avec son application logiciste, constitue une note paradoxale de la mentalité scolastique. D'une part le Moyen Age est mystique et théologique, d'autre part il est rationaliste et logiciste.

Nous allons maintenant examiner, d'une façon évidemment sommaire, la formation de la logique scolastique et ses problèmes importants.

1. *Les sources et la formation de la logique.* — En premier lieu les sources de la logique scolastique se trouvent dans l'héritage grecoromain. Le Moyen Age a à sa disposition l'oeuvre de Marcianus Capella, surtout l'enciclopédie du *Satiricon*, exposition complète des sept arts libéraux (parmi lesquels, la logique).

On étudie aussi les travaux du philosophe néoplatonicien Simplicius (IV-ème siècle), commentateur des oeuvres d'Aristote. Les livres de Marius Victorinus, parmi lesquels la traduction de l'*Isagoge* de Porphyre, sont aussi, une source des problèmes logiques du Moyen Age. Les traductions et les commentaires de Boèce sont en grande estime à cette époque, ainsi que ceux de Cassiodore. Les écrits d'Appulée, surtout *De Dogmate Platonis* (dont les chapitres II et III formaient un compendium de logique), sont aussi à l'origine de la logique scolastique.

Enfin, les écrits des auteurs préscolastiques occidentaux, doivent être cités, comme ceux d'Isidor de Sevilla († 636), dont le livre *Originum sive Etymologiarum libri XX* est une sorte d'encyclopédie qui commence avec les sept arts libéraux.

Le même rôle a eu en Angleterre Béda le Vénérable (674-735), dont le livre *De Natura Rerum* s'inspire des travaux d'Isidor de Sevilla.

Une autre influence, dans le développement de la pensée scolastique (et surtout de la logique), ont eu les logiciens chrétiens orientaux et surtout Jean Damascène (676-754), dont la "*Source de la Connaissance*" commence par une introduction philosophique à la logique d'Aristote.

Les philosophes syriens ont aussi cultivé la logique et ont écrit des commentaires (VII-ème siècle): Sever Sebokt nous a laissé un commentaire sur les *Analytiques* et sur le *De Interpretatione*; Athanase (645) a traduit *l'Isagoge* de Porphir; Georgios a traduit entièrement *l'Organon* et Jacob d'Edesse les *Catégories*.

En partant de cet héritage la logique scolastique s'est constituée en plusieurs étapes, qui peuvent être enumérées comme il suit.

a) La renaissance carolingienne, qui réorganise l'enseignement, et est caractérisée par l'apparition des centres de culture nouveaux: Les abbayes de Saint-Gall, Fulda, Reichenau, Corbie, etc.

C'est l'oeuvre d'Alcuin (735-804) en France, et de Rhaban Maure (780-856) en Allemagne.

b) Nous jugeons que l'apparition de cette grande figure du Moyen Age, Jean Scottus Eriugena (†877), est un moment important dans le devenir de toute la culture médiévale, à laquelle il a donné une empreinte néoplatonique. Ses disciples Heiricus d'Auxerre et Remigius d'Auxerre ont laissé une série de commentaires de logique.

On ne peut passer sous silence l'humaniste Gerbert d'Aurillac († 1003), qui a donné des cours à Reims sur *l'Isagoge* de Porphire, les *Catégories* et *De Interpretatione* d'Aristote, la *Topique* de Cicéron, et a commenté tous les livres de Boèce.

Le développement de la logique continue ensuite avec Abélard (1079-1142), dont le gros ouvrage est *La Dialectique*; *De Intellectibus* appartenant à un disciple d'Abélard; les travaux de Gilbert de la Porrée († 1154) dont nous citons *De sex principiis*; *Metalogicus*, traité très important (surtout du point de vue de l'histoire de la logique) écrit par Jean de Salisbury († 1180), élève d'Abélard; le livre des *Sentences*, dû à Pierre Lombard († 1164), qui tout en n'étant pas un livre de logique contenait une foule d'observations qui ont influencé la logique.

Au XII-ème siècle, Aristote est bien connu. On traduit dans ce temps tous ses oeuvres de logique, les tirant "de la mort et du sommeil" (*a morte vel a somno*), ainsi que le dit un des grands maîtres de Chartres, Thierry de Chartres.

Les six traités de *l'Organon* d'Aristote formeront maintenant la *logica antiqua*, qui sera divisée en deux parties: *logica vetus* et *logica nova*. La *logica vetus* contenait les traités des *Catégories* et *De Interpretatione* et la *logica nova* contenait les traités de l'Organon traduits plus tards, par l'intermédiaire des Arabes, à savoir *Analytica Priora*, *Analytica Posteriora*, *Topica* et *De Sophisticis Elenchis*.

Une autre série de problèmes, dont l'Organon aristotélique ne s'était pas explicitement occupé, formait l'objet de quelques traités plus petits de logique qui portaient le nom commun de *Parva Logicalia* - "Les petits traités de logique".

Par conséquent, la division de la logique au Moyen Age était la suivante:

LA LOGICA ANTIQUA
(Les six traités d'Aristote)

Logica vetus	*Logica nova*
1. *Praedicamenta* (*Categoriae*)	3. *Priora Analytica*
2. *Perihermenias*	4. *Posteriora Analytica*
	5. *Topica*
	6. *De Sophisticis Elenchis*

↓

PARVA LOGICALIA

1. *Proprietates terminorum* - un problème qui concerne la modification de la prédication d'un terme par sa position dans la proposition.
2. *Syncategoremata* - particules logico-grammaticales, qui n'ont pas un sens autonome et dont le sens dépend donc du contexte.
3. *Insolubilia* - paradoxes logiques au centre desquels se trouvait l'antinomie du menteur.
4. *Obligatoria* - un problème qui détermine l'interval de temps dans lequel on est obligé d'affirmer une proposition.
5. *Consequentiae* - le problème des conséquences hypothétiques, qui est ce qu'aujourd'hui on appelle le calcul des propositions.

Mais avant tous ces problèmes, on traitait le problème de la nature des universaux, c'est-à-dire des conceptes généraux.

2. *Les influences.* — Le destin de la logique grecque a eu des méandres très curieux: elle n'arrive pas directement en Europe, mais par les Arabes. Mais par cela elle garde un cachet alexandrin et néoplatonicien, dont elle ne pourra échapper ni à son apogée qu'elle atteint avec Thomas d'Aquin. Les grands philosophes arabes, Alkíndi († 873), Alfarabi († 950), Avicenna (980-1037), El Gazali (1059-1111) et enfin Averroës (1126-1198), ont influence le développement de la philosophie occidentale et surtout le sens de ce développement.

Nous pouvons aussi citer les influences judaïques, dans le même sens néoplatonicien; Avicebron (il est actif autour de 1050 en Espagne), Moses ben Maïmun ou Maïmonides (1134-1204) dont on a un *Vocabularium logicae* et Levy ben Gerson (en activité en 1350) avec commentaires sur *l'Isagoge* et les traités de logique d'Aristote, ont grandement déterminé le cours de l'histoire de la logique en Occident.

Il y a aussi eu une influence byzantine. Sous l'empereur Constantin Monomachos (1042-1055), on connaît une certaine recrudescence de la philosophie; on fonde une Académie et Michel Psellos (1018-1078?) commente les écrits d'Aristote, entre autres, l'*Isagoge* de Porphyre, les *Catégories* et *De Interpretatione*.

Psellos a passé quelque temps pour l'auteur d'un "*Résumé de la* sortes de sciences: les sciences réelles qui s'occupent de ce qui existe (*de ente*) et les sciences qui ne traitent pas de l'*ente* et ne sont donc pas de sciences réelles mais *sermocinales.* Les sciences sermocinales sont:

1. *Grammatica, quae docet recte loqui*;
2. *Rhetorica, quae docet ornate loqui*;
3. *Logica, quae docet vere loqui.*

La logique nous apprend donc à parler "vrai" (*vere*).

Mais dans ce sens elle n'est pas une science, mais *le mode* de toutes les sciences, *modus scientiarum.* Si l'on tient compte que *modus* signifie, mode, manière, procédé, principe, il résulte que la logique était conçue comme étant les principes de toutes les sciences (*modi scientiarum omnium*). L'argument principal, d'ordre logique, était le suivant (Albert le Grand, *De Praedicabilibus, De natura logicae*): "On ajoute également (c'est-à-dire à d'autres arguments) que le mode d'une chose ne peut se trouver avec la chose dont il est le mode dans la division de son genre" - *Addunt etiam quod nullius rei modus cum re, cuius modus est, venit in generis sui divisionem.* En d'autres termes, puisque la logique est la science des sciences - *scientia scientiarum*, elle ne peut être une des sciences, car alors elle serait le genre et en même temps une des espèces du genre.

La logique, ainsi conçue, est une science formelle, mais elle n'étudie plus directement les "*morphés*" d'Aristote; elle étudie les "*formalitates*" qui sont les images intellectuelles des choses et ne dépassent pas le plan des concepts abstraits. L'objet de la logique sont les "intentions secondes" - *intentiones secundae* - les concepts abstraits des choses; pour les dépasser et arriver aux "*morphés*" d'Aristote, il faut la foi.

B. *Le problème des universaux.* La nature de ces concepts généraux était le premier problème - *prima quaestio.* Pourquoi? Parce que - ainsi que pour Aristote - la science des scolastiques était la science de l'universel: *scientia est universalium* et la base de la science dépend de la nature de l'universel. Le problème était posé par Porphyre et dans l'*Isagogé* elle apparait en latin dans la traduction de cet opuscule surtout dans la traduction de Boèce. L'ampleur de cette question était bien justifiée: il fallait assurer les bases de la science qui consistaient dans l'universel.

Les solutions sont multiples: dans le traité de Jean Salesbury (XIII-ème siècle), portant le titre *Metalogicus,* nous trouvons 13 solutions. Mais on peut les réduire à quatre.

(1) *Nominalisme,* qui nie toute existence des idées générales, tant dans les choses qu'en dehors d'elles; les concepts généraux ne sont que de simples mots - *voces* - par lesquels on désigne des individue concrets.
Le nominalisme aurait eu comme premier représentant Roscelin (né en 1050), et arrive à son apogée et à sa victoire avec Guillaume d'Occam († 1347).

(2) *Le conceptualisme* d'Abélard, qui part de l'idée d'Aristote, selon laquelle l'universel est ce qui est prédicable de plusieurs - *qoud*

science de la logique d'Aristote". Cette erreur a été commise par C. Prantl, qui a bâti sur cette erreur toute une théorie. On a constaté plus tard que ce traité n'est qu'une traduction faite par Georgios Scholarios (1400-1464) du célèbre traité *Summulae Logicales* écrit par Pierre d'Espagne (1226-1277). Ce traité contenait toute la logique du Moyen Age et a eu aproximativement 176 éditions à cette époque, constituant le fondement de la logique scolastique.

Quoiqu'il en soit, il est certain que les Byzantins ont eu une influence sur l'Occident, et cette influence commence du premier contact avec l'Orient. Outre Psellos, on peut citer: Jean Italos, contemporain avec celui-là; Eustratios (1050-1120), Mitropolite de Nicée, auteur de divers écrits aristotéliciens, parmi lesquels *Analytica Priora*; Nikephoros Blemmides († 1272) auteur d'un "*Compendium de Logique*"; Georgios Pachymenes (1242-1310) qui a composé une exposition de la philosophie entière d'Aristote, qui nous est parvenue seulement dans la traduction latine; un autre "*Epitomé*" de la logique d'Aristote a été écrit par Georgios Aneponymos (imprimé à Augsburg en 1600); on a aussi d'autres écrivains de commentaires de logique comme Sophronias, Theodoros Metochites († 1330), Maximos Planudes (1260-1310), connaisseur de la langue latine, etc.

Cette série de philosophes byzantins continue plus tard jusqu'au XIV-ème, XV-ème, XVI-ème et XVII-ème siècle, influençant la pensée en Italie et déterminant la fondation de l'école platonicienne de Florence et le mouvement néoperipatéticien de Padoue et Ferrara.

J'insiste sur cette influence byzantine parce que, en général, on ne lui a pas donné l'importance qu'elle mérite. La liaison de l'Occident avec Byzance apparaît des premiers contacts avec cet autre foyer de la civilisation méditerranéenne. Nous ne pouvons donner ici qu'une indication mais décisive. Voici ce qu'écrit Bulaeus (*Historia Universitatis Parisiensis*, III,p.10): *Post expugnatam Constantinopolim a Francis et Venetiis sacro foedere iunctis Philippo Augusto rege Lutetiae conditum est collegium Constantinopolitanum ad ripam Sequanae prope forum Malbertinum*(...) *ut Graecorum liberi Lutetiam venientes una cum lingua latina paullatim vetus illud et patrium in Latinos odium deponeret* ("Après la conquête de Constantinople par les Français et les Vénétiens, unis par une fédération sainte, le roi de Lutèce étant Philippe Auguste [1165-1223], on a créé le *Collège Constantinopolitain* sur la rive de la Seine, près du forum malbertin,(...) de sort que en même temps avec les livres des Grecs qui viennent en langue latine, l'ancienne haine [qui était] aussi des ancêtres envers les latins soit graduellement abandonnée").

3. *Les idées spécifiques de la logiques scolastique.* — Nous allons souligner les principales particularités de la logique du Moyen Age, sans pouvoir nous étendre sur les détails des ces idées.

A. *La Nature de la logique.* Les scolastiques distinguaient deux *natum est de pluribus praedicari* - et cette propriété est spécifique et il l'a de son origine - *a nativitate sua.* Abélard remarque que ni les mots, ni les discours n'ont pas une "nativité", parce qu'ils sont d'institution humaine tandisque l'origine des choses est la création de leur nature. Alors, où se trouve le général? Il n'a lieu que dans l'acte de prédication qui se fait par un jugement - *sermo.*

(3) *Le réalisme trascendental* qui prend une forme bien définie avec Anselme de Canterbury (1083-1108), mais est édifié comme doctrine platonicienne des universaux par Guillaume de Champeaux (1070-1120).

Les universaux ont dans cette conception une existence en soi transcendente. Les difficultées sont nombreuses et ainsi apparaissent beaucoup d'explications, dont nous mentionnons spécialement celles données par les frères Bernard et Thierry de Chartres.

(4) *Le réalisme trimodal*, d'inspiration arabe, est élaboré par Albert le Grand (1193-1280) et par son disciple Thomas d'Aquin (1225-1274).

Albert le Grand accorde aux universaux une triple existence:

— *ante rem*, les universaux sont la cause préexistente des choses - *causa praehabens*;

— *in re*, les universaux accompagnent les choses et conditionnent la pensée et les noms des choses, ayant une fontion potentielle;

— *post rem*, les universaux sont la forme abstraite élaborée par l'intellect et qui est universelle en lui et dont la caractéristique est la communicabilité.

C. *La terminologie scolastique.* L'étude de la logique d'Aristote se faisait d'une manière spécifique, due aux besoins didactiques et méthodologiques. Mais il faut y voir plus: tous les vers mnémotechniques, inventés par les moines du Moyen Age, ne sont pas seulement forgés pour faciliter l'enseignement; ils sont en même temps une manière de penser les problèmes logiques d'une façon algébrique. Nous ne pouvons entrer ici dans les détails de cette terminologie (nous l'avons amplement exposée, avec les vers latins, dans notre *Istoria Logicii*, en roumain, Bucarest, 1969), mais nous donnerons un seul exemple: les vers qui indiquaient les modes et les figure du syllogisme. Voici ces vers bien connus:

Barbara, Celarent, Darii, Ferioque prioris;
Cesare, Camebtres, Festino, Baroco, secundae;
Tertia, Darapti, Disamis, Datisi, Felapton,
Bocardo, Ferison habet; quarta insuper addit,
Bramantip, Camenes, Dimaris, Fesapo, Ferison.

Se référant à cette terminologie, ridiculisée dans le passé, le mathématicien Gergonne écrivait dans les "*Annales des Mathématiques*" (VII,1816-1817): "Le grand nombre des conditions auxquelles on avait cherché à satisfaire dans la composition de ces vers artificiels aurait dû en faire excuser un peu la dureté".

C'est de cette façon qu'ils étudiaient les *Catégories*, le *De Interpretatione, les Analytiques*, la *Topique* et le *De Sophisticis Elenchis*.

D. *Parva Logicalia.* Nous avons déjà dit deux mots sur les "Petits Problèmes de Logique". Nous allons maintenant les passer en revue.

1) *Suppositic.* La *suppositio* était la disposition d'un terme significatif à la phace d'un autre. Les Scolastiques énuméraient toutes les espèces possibles des *suppositiones* et les règles qu'il faut respecter pourque la *suppositio* soit correcte.

La première règle et la plus importante était que la substitution d'un terme à un autre ne peut se faire que conformément à l'exigence de la copule - *juxta exigentiam copulae.*

Ce problème a disparu des traités modernes de logique et cela constitue vraiment une perte.

2) *Syncategeremata.* Il y a des termes qui ont un sens autonome, comme le substantif et le verbe et ce sont les *categoremata.* Il y a aussi d'autres termes qui n'ont pas par eux - même un sens autonome et ceux-ci sont les *syncategoremata* ou les particules "consignificatives". Toutes ces particules étaient minutieusement classifiées, étudiées, et on établissait des règles pour leur emploi correct. De notre temps ce problème est considéré plutôt comme un problème grammatical et c'est aussi une perte pour la logique.

3) *Consequentiae.* Les "conséquences" étaient la théorie de ce qu'on appelle aujourd'hui le calcul propositionnel. On connaissait très bien les principaux théorèmes de ce calcul (ou tautologies). On connaissait la différence entre implication formelle et matérielle, les lois de De Morgan, les conséquences modales, etc. etc.

4) *Insolubilia.* Dans un *tractatus* entier on étudiait les soit-disant "problèmes insolubles" dans le centre desquels se trouvait l'antinomie du menteur et ses variantes. On formulait en tout 19 variantes du "menteur" et 15 solutions. La théorie des niveaux logiques apparaît chez le logicien Pierre d'Ailly (1350-1425). La logique déontique des logiciens contemporains apparait alors comme une théorie des obligations - *Obligatoria.* Elle est utilisée comme solution des *insolubilia* par Jean Buridan († 1358). Enfin la règle de *circulus viciosus* de Russell, d'après laquelle "on ne peut définir une classe par des membres qui présupposent la classe", est déjà énoncée par Albertus de Saxonia († 1390) qui dit: "jamais la part ne peut signifier le tout dont elle est la partie" - *nunquam part potest significari totum cuius est pars.*

4. *Considérations générales.* La logique scolastique a été trop négligée et le trésor du Moyen Age doit être maintenant exploré. Sans aucune doute, les Scolastiques étaient de grands logiciens qui conservaient un grand dépôt, le trésor aristotélicien. La logique n'était pas pour eux un jeu de signes. Le *légos* scolastique était en même temps *sermo* et *ratio* et l'étudier signifiait approfondir la *sermo*, la parole, mais

en même temps la *ratio*, la raison. Si les "*formalitates*", objet de la logique, sont les *intentionae secundae*, ils ne nient pas les *formae exemplares ante rem*, lesquelles peuvent être atteintes, dans leur dernier fond ontologique, par un acte de fois. Mais jusqu'à cette dernière extrémité, la raison fonctionne pleinement, le rationalisme scolastique est vraiment une affirmation de la *ratio* s'incorporant dans la *sermo*.

XII. - RENAISSANCE. — On connaît bien les causes qui ont déterminé la chute de la scolastique et l'essor des sciences expérimentales. La nouvelle mentalité, dirigée vers l'exploration du concret immédiat, renonce aux essences des scolastiques, à l'universel d'Aristote. Cette nouvelle mentalité se reflète aussi dans les recherches de logique. Au commencement il y a un prolongement de la logique scolastique mais dans la suite plusieurs écoles se définissent.

1. *La logique rhétorique des cicéroniens.* Le premier humaniste qui attaque la logique scolastique est Laurentius Valla (1407-1457). Son oeuvre principale est *Dialecticae disputationes contra Aristotelicos*, dans laquelle il prend comme base de la nouvelle logique, plutôt rhétorique, les travaux de Cicéron et de Quintilien (*De institutione oratoria*). La réaction contre la logique scolastique trouve son expression définie, dans les oeuvres de Rudolphe Agricola (1442-1485), professeur à Heidelberg.

2. *L'école de Melanchton.* Dans l'Allemagne, une grande personnalité, Philippe Melanchton (1447-1560), prend comme source de la logique les livres d'Aristote, mais dans un esprit nouveau, fertilisé par Cicéron et Agricola.

3. *La dialectique ramiste.* Pierre de la Ramée (1515-1572), élève d'un élève d'Agricole, à savoir Jean Sturm, mène une lutte acerbe à Paris contre l'aristotélisme. Il soutient deux choses: que les écrits attribués à Aristote ne sont pas authentiques; que leurs contenus sont une accumulation d'erreurs. Ses principaux travaux de logique sont: *Institutiones dialectices* et *Scholae in liberales artes*.

Le ramisme s'est vite répandu en Europe: en Angleterre, en Allemagne, etc.

4. *Les aristotéliciens et averroistes.* À l'époque dont nous nous occupons, il y avait aussi un courant qui prétendait garder la doctrine d'Aristote dans toute sa pureté. Ce courant d'idées s'est surtout dévéloppé en Italie et provenant d'un mouvement péripatéticien plus ancien, qui s'est manifesté à Salerno, où un groupe de médicins cultivaient depuis le XII-ème siècle la doctrine de Galien. Autour de 1300 ce courant passe à Padoue sous forme averroïste et au centre de leurs préoccupa-

tions logiques se trouve la théorie de la démonstration. Une autre direction du péripatétisme nouveau est basé exclusivement sur les commentateurs grecs et spécialement sur Alexandre d'Aphrodise.

Parmi les péripatéticiens italiens on doit mentionner: Angelus Politianus (1454-1496), Petrus Pomponatius (1462-1524), Caesar Cremonius (1550-1631), Julius Caesar Vanini (1585-1619), Laurentius Maiolis, etc.

5. *La scolastique espagnole.* Le mouvement humaniste a ébranlé la scolastique, mais elle résiste encore en Espagne, où sont actifs, dans le domaine de la logique, une série de penseurs. Ces logiciens se sont formés autour de Jean Majoris Scotus (1478-1540), professeur à Paris, qui reprend la logique thomiste avec un accent particulier sur les *Parva Logicalia.* Ses élèves évoluent dans une direction éclectique. Une série de penseurs espagnols se sont formés dans cette atmosphère éclectique: Bartolomé de Castro qui publia *Temini Logicales* (Compluti 1512); Rodrigo del Cueto dont on a *Primus tractatus summularum* (Compluti, 1524); Ferdinand Enzinas auquel on doit un *Tractatus Syllogismorum* (Paris, 1526); Pedro Ciruelo qui publia *In Summulas Petri Hispani* (Salamanca, 1537) etc.

La scolastique espagnole atteint deux points culminants: avec Dominicius Soto, qui reprend l'aristotélisme thomiste et avec Petrus Fonseca qui veut se tourner vers l'aristotélisme pur.

6. *Les systématiques du XVII-ème siècle.* Au XVII-ème siècle une nouvelle direction se manifeste qui voit dans la logique deux caractères essentiels:

— Le caractère de système, de construction théorique, dans le sens d'Aristote.

— Le caractère de méthode, de procédé méthodique.

Les principaux logiciens de ce courant sont: Bartholomaeus Keckermannus dont l'oeuvre (posthume) est *Systema Systematum* (Hanovra, 1613); Jean Förster qui publie *Systema problematum theologicorum* (Wittebergae, 1610); Clemens Timpler auquel on doit *Physicae seu philosophiae naturalis systema methodicum* (Hanovra, 1605); Adam Scherzer qui écrit *Systema theologicae* (Leipzig, 1704); enfin le plus important des "systématiciens" est Jean Henricus Alstedius qui publie *Logicae systema harmonicum* (Herbonnae, 1614).

7. *L'école de Raymond Lulle.* Au Moyen Age apparaît une personnalité curieuse, qui s'occupe de la logique d'une manière originale.

Il s'agit de Raymond Lulle (1235-1315) qui est un précurseur de la logique mathématique actuelle.

Lulle proposait un système logique qui comprenait un "alphabète" primitif d'idées, et une méthode mécanique de combinaison de ces idées par laquelle on pouvait obtenir tous les jugements possibles. Cette "logique" était ce qu'il appelait *Ars Magna*.

Nombreux sont les adeptes de l'*Ars Magna* au temps de la Renaissance. On peut citer parmi ces partisans, qui ont cherché à perfectionner l'art de Lulle, Degui, avec *Lectura super partes artis generalis* (Padova, 1453); Jacobus d'Olesia qui écrivit *Commentaria super artem Raymondi Lulli* (Palma, 1516); Bernhardus de Lavinheta avec *Dialectica seu nova logica Lulli* (1516); Jacob Faber Stapulensis qui édita les oeuvres de Lulle, etc. Autres figures importantes de cette époque, qui se sont occupé de l'art de Lulle, sont Agrippa von Nettesheim, Pedro Ciruelo, Giordano Bruno, Antonius Belverius et beaucoup d'autres.

8. *L'école de Korydalée.* Moins connue est l'activité du savant commentateur grec Théophile Korydalée (1574-1646), qui s'est formé à Padoue sous la direction de Cesare Cremonini. Après avoir fini ses études en Italie, il revient à Athènes où il fonde une Université et une Académie.

Le mouvement de Padoue cherchait à interpréter Aristote dans un esprit moderne, plus conforme au mouvement scientifique du temps. Ensuite, Korydalée organise une autre Académie à Constantinople. Les disciples du savant commentateur grec rayonnent dans le Sud-Est de l'Europe et donnent des cours dans les Académies de Constantinople, de Jannina et surtout dans les Académies princières de Jassy et de Bucarest, qui deviennent des foyers de culture pour la péninsule balkanique entière. Les plus grands savants orientaux sont présents - à l'époque - dans les pays roumains et on possède un très grand nombre de manuscrits korydaléens (des centaines), témoignage irréfutable de la présence d'Aristote et de l'*Organon* dans cette région.

XIII. - LA MÉTHODOLOGIE. — Après la Renaissance, les résultats magnifiques obtenus dans les sciences ont déterminé, d'une part l'abandon de la logique scolastique, et d'autre part la recherche des canons méthodologiques pour mener d'une manière systématique les expérimentations. Les précurseurs de la logique de la méthode sont: Roger Bacon (1214-1292) visionnaire de la science expérimentale; Leonardo da Vinci (1452-1519), qui a déjà formulé les canons baconiens *de la concordance, des différences*, et des *variations concomitantes*; Galileo Galilei (1564-1642), qui n'est pas seulement un savant de génie, mais aussi un théoricien des méthodes de la science.

Francis Bacon (1561-1626) fait une critique de la logique scolastique, et par cela de l'*Organon* aristotélicien, et propose un "*Nouvel Organon*" - *Novum Organum*, dirigé spécialement vers la recherche expérimentale.

René Descartes (1596-1650) écrit deux travaux concernant la recherche scientifique et ses règles: *Regulae ad directionem ingenii* (non achevé) et *Discours de la méthode.* Descartes rêve d'une *Mathesis Universalis* qui n'est pas les mathématiques mais est contenue dans les mathématiques.

Le "Discours" peut être réduit à deux principes et à quatre règles.

Les deux principes sont: *le doute méthodique et les idées claires et distinctes.*

Les quatre règles sont: (1) n'accepter rien qui n'est pas évident; (2) partager le problème dans des problèmes partiels; (3) partir des questions les plus simples vers celles plus complexes; (4) énumérer d'une façon complète et n'omettre rien.

Le cartésianisme se répand dans toute l'Europe et gagne le monde scientifique de partout. La logique d'Aristote ne représente plus qu'un ensemble de règles mortes.

Deux traités méritent d'être mentionné à cette époque: *La logique de Port-Royal* (1662), dûe à A. Arnauld et P. Nicole, et composée sous l'influence des idées cartésiennes; *Logica Hamburgensis* (1638), due à Joachim Jungius, qui a eu un rôle analogue en Allemagne, bien qu'indépendamment des idées de Descartes.

Isaac Newton (1642-1727) énonce plusieurs règles de la recherche scientifique dans son oeuvre fondamentale, *Philosophiae Naturalis Principia Mathematica.*

On peut encore citer, dans la même direction de recherches, en Angleterre, John Herschel (1792-1871), William Whewell (1795-1866), Thomas Reid (1710-1796). Mais le plus important "systématisateur" de toutes ces règles est John Stuart Mill (1806-1873). Celui-ci fait une critique du syllogisme, en reprenant un argument sceptique, d'après lequel le syllogisme serait une *petitio principii.* Dans son livre *Logic deductive and inductive* (1843), Stuart Mill montre que le syllogisme va du particulier au particulier et donc il est basé sur l'induction. D'où le besoin d'organiser la recherche expérimentale, pour laquelle il donne cinq tables. F. Bacon a donné trois tables: table de présence, d'absence de dégrés. Stuart Mill montre que de telles tables sont des véritables modes de raisonnements et en ajoute encore deux: la méthode double de la concordance et de la différence; la méthode des résidus.

Dans le même ordre d'idées on peut citer Herbert Spencer (1820-1903), adversaire du syllogisme.

D'autre part, une série de penseurs français s'occupent de l'induction, avec beaucoup d'idées neuves et fécondes. Ce sont: Jules Lachelier (1832-1918), André Lalande (1867-1963), Claude Bernard (1813-1878).

En Allemagne on peut citer le psychologue Wilhelm Wundt (1832-1920) dont l'oeuvre monumentale en trois volumes, *Logik* (1883), portait un sous-titre significatif: "Une recherche des principes de la connaissance et des méthodes de la recherche scientifiques".

Autres recherches dans cette voie sont dues aux penseurs: E.

Darbon, qui a écrit *L'Explication mécanique et le nominalisme* (1911); M. Dorolle, auquel on doit *Les problèmes de l'induction* (1926); J. Nicod qui a publié *Le problème logique de l'induction* (1924), etc.

Nous arrivons maintenant à la méthodologie des sciences contemporaines.

L'apparition des géométries non-euclidiennes, la théorie de la rélativité, la physique quantique, etc., ont complètement changé la vision théorique de la science, qui tend de plus en plus à devenir une science non-cartésienne.

On cherche maintenant moins un canon - ou des canons - de la recherche scientifique, mais plutôt une logique de la science, qui puisse donner un schéma logique des résultats extraordinaires et quelquefois contradictoires obtenus expérimentalement.

Les plus importants théoriciens de la science moderne sont:

H. Poincaré, E. Meyerson, G. Bachelard, H. Weyl, F. Gonseth, R. B. Haldane, A. N. Whitehead, C. D. Broad, Stephane Lupasco, Paulette Février, Jean-Louis Destouches, etc.

Bien entendu on ne peut oublier l'école néopositiviste, avec Moriz Schlick, R. Carnap, F. Frank, H. Reichenbach, etc., ou les penseurs américains, comme W. Quine, N. Goodmann, etc.

Dans l'Union Soviétique, ce champs de recherches est très fréquenté, et on a obtenu des résultats dignes d'être mentionnés, dus à V. A. Smirnov, A. A. Zinoviev, G. I. Ruzavin, D. P. Gorski, etc.

En Italie ces sortes de recherches ont été cultivés dès le début de ce siècle, par F. Enriques (*Problemi della Scienza*, 1906), qui fonda avec E. Rignano la revue internationale de synthèse scientifique "*Scientia*".

Autres penseurs italiens dans ce secteur de recherches sont: L. Geymonat, P. Filiasi Carcano, G. Pretti, leur revue étant "*Analysis*" (Torino); S. Ceccato, F. Rossi-Landi, V. Somenzi et G. Vaccarino, qui ont développé leur activité à Roma et Milano et ont édité la revue "*Methodos*"; à Bologna on peut citer Aldo Testa (Revue "*Il Dialogo*") et R. Borsari et Fr. Spisani (revue "*Rassegna internazionale di Logica*"); enfin, une nouvelle génération est formée par A. Pasquinelli, E. Casari, E. Melandri, E. Agazzi, C. Mangione, C. Celluci (voir l'excellente étude sur ce mouvement due au prof. V. Somenzi: *Philosophy and Methodology of Science in Italy to-day*).

XIV. - La philosophie de la logique. — Les résultats extraordinaires de la science ont réfoulé la logique à l'arrière des recherches. Après Descartes, elle n'est plus qu'une collection de règles vénérables qu'on apprend en vertu de la tradition, mais auxquelles on n'accorde aucune importance. Le problème de la logique devient à cette époque un problème philosophique, concernant sa valeur et sa portée, surtout comme théorie de la connaissance.

La logique est conçue comme un problème fondamental de la connaissance c'est ainsi qu'elle apparaît dans la *Kritik der reinen Ver-*

nunft de Kant (1724-1804). La logique de Kant est une "logique formelle et transcendentale", transcendentale parce qu'elle regarde seulement les lois de l'intellect et de la raison, lois qui font l'expérience possible, préexistant à l'expérience (a priori).

Les adeptes de Kant offrent des variantes de sa conception. Parmi eux nous citons surtout le courant logico-méthodologique (L'école de Marbourg), avec H. Cohen, E. Cassirer, Jonas Cohn, A. Kronner, E. Lask.

Les philosophes allemands, après Kant, reprennent le problème philosophique de la logique. J. G. Fichte (1762-1814), a déjà l'idée de la triade logique-thèse, antithèse et synthèse. F. W. Schelling (1775-1854), tenant compte des progrès réalisées par les sciences expérimentales, n'accorde pas (comme l'ont fait Kant et Fichte) une indépendance de la pensée par rapport à son objet. La pensée est déterminée par son objet, mais l'objet est aussi determiné par la pensée.

Enfin G. W. F. Hegel (1770-1831) fait de la logique une métaphysique qu'il appelle "dialectique". Selon lui, la logique est un concept ouvert qui ne peut être défini. Le concept de logique se produit lui-même, dans son propre développement, et donc ne peut être préalablement donné. On peut néanmoins dire que la logique est la science de la pensée pure dans les éléments de la pensée pure. L'élément de la pensée pure est le concept, qui est la forme successive de l'idée, c'est-à-dire, en même temps de l'existence et de la pensée. En commençant par la connaissance initiale de l'absolu, mais aussi de son devenir (qui est la matière), la connaissance de l'idée sera, à son terme final, la connaissance de toutes les phases par lesquelles passe le concept. Le devenir du concept s'effectue par la triade dialectique: thèse, antithèse, synthèse.

Le devenir du concept est donc le devenir de la réalité. La logique est exposée par Hegel dans sa *Wissenschaft der Logik* (1812-1816).

Les adeptes de Hegel sont nombreux et son influence s'est ressentie partout en Europe. En Italie, le hegelianisme reçoit un cachet particulier avec Benedetto Croce et G. Gentile.

La gauche hegelienne fait une véritable révolution, en inversant la dialectique de Hegel et lui donnant une base matérialiste. C'est l'oeuvre de Marx (1818-1889) et d'Engels (1820-1895). Lenin (1870-1924) dans ses *Cahiers Philosophiques* nous dit que la logique n'est pas la science des formes extérieures de la pensée, mais "la science des lois du développement de toutes les choses matérielles (...), de tout le contenu concret du monde et de sa connaissance". La dialectique, ainsi conçue, commence d'un système de catégories, mais cette fois-ci, ce système est ouvert (et non fermé, comme chez Kant); le devenir se fait par la contradiction dialectique qui passe par les phases de la triade, thèse, antithèse, synthèse; enfin, le devenir est soumis aux lois dialectiques, dont les principales sont la loi de "l'unité et de la lutte des contraires", la loi du "passage de la quantité à la qualité" et la loi de la "négation de la négation".

On est d'accord que l'*Organon* marxiste reste à être constitué. Un débat intéressant, qui dure depuis longtemps dans l'Union Soviétique, concerne la nature et l'organisation des catégories. Une telle systématisation a été proposé par V. P. Tugarinov dans son livre "*La corrélation des catégories du matérialisme dialectique*". Autres travaux dans le même domaine sont: M. Rozental, "*Kategorii materialisteskij dialektik*" (Moscou, 1957); V. S. Bibler, "*O sisteme Kategorii dialekticeskoj dialektikii*" (Stalingrad, 1958); K. S. Brakradze, "Sistem i metod filosofii *Gegelja*" (Tbilisi, 1958), etc.

Dans le même temps et parallèlement se développe une conception psychologiste de la logique, qui conçoit cette science comme un chapitre de la psychologie. D'après cette conception, l'objet logique et les opérations logiques sont expliqués par des facteurs et lois psychologiques, et en définitive, par des facteurs subjectifs.

Parmi ces penseurs nous citons premièrement ceux de l'école anglaise: Thomas Hobbes (1588-1679), John Locke (1632-1704), George Berkley (1685-1753), David Hume (1711-1776), Thomas Reid (1710-1796), et l'école écossaise, Dugald Steward (1753-1828). Après, on peut citer William Hamilton (1788-1856), James Mill (1773-1836), John Stuart Mill (1806-1873), Herbert Spencer (1820-1903), Alex. Bain (1818-1903).

Le sensualisme français, qui commence par Condillac (1715-1780), se rattache au mouvement psychologiste anglais; dans le même courant d'idées peuvent être mentionnés: J. G. Cabanis (1757-1808), Destutt de Tracy (1754-1836) P. Laromiguière (1756-1837), etc.

Le pragmatisme apparaît aux États Unis avec William James (1842-1910) et comprend des logiciens importants comme C. S. Peirce (1839-1914), J. Dewey (1859-1952, l'anglais F. C. S. Schiller (1864-1937).

Enfin, on ne peut omettre les penseurs antiintellectualistes français, comme Jules Lachelier (1834-1918), Émile Boutroux (1845-1921), Henri Bergson (1859-1941), etc.

On ne peut oublier, dans cet ordre de recherches, la *Psychologie du Raisonnement*, due à Eugenio Rignano (Paris, 1920).

Une autres série de logiciens voient dans la logique une théorie des valeurs, ou une théorie normative, ou encore un art ou une *technique*.

Parmi eux nous pouvons citer les noms de quelques-uns, sous lesquels figurent des traités de logique devenus célèbres:

— Christof Sigwart (1830-1904), qui publia son traité *Logik* en deux volumes en 1873-1878 (Tübingen).

— Wilhelm Dilthey (1833-1911) dont nous citons le *Grundriss der Logik* (Berlin, 1865).

— Hermann Lotze (1817-1891), dont la *Logik* drei *Bücher vom Denken, von Untersuchungen, und vom Erkennen* parut en 1843 à Leipzig.

— Wilhelm Windelband (1848-1915) dont nous mentionnons *Die Prinzipien der Logik* (Heidelberg, 1913).

— Wilhelm Jerusalem (1851-1936) publia: *Der kritische Idealismus und die reine Logik* (Wien, 1905).

— Beno Erdmann (1851-1921) auquel on doit une *Logik* (Halle, 1892).

— Heinrich Maier (1867-1938) qui écrivit plusieurs ouvrages de logique, parmi lesquels le plus connu est *Die Syllogistik des Aristoteles* (1896-1900).

— Edmond Goblot (1858-1935) dont le *Traité de Logique* (Paris, 1918) est devenu célèbre.

— André Lalande (1867-1963) publia sa *Logique normative et vérités de fait* (Paris, 1929).

Le mouvement psychologiste a donné lieu a une réaction double: d'une part à la conception phénoménologique, d'autre part à la logique mathématique.

Bernard Bolzano (1781-1848) est celui qui fait une différence nette entre psychologie et logique, dans son ouvrage en quatre volumes *Die Wissenschaftslehre* (Sulzbach, 1837). Ses idées influencèrent Edmund Husserl (1859-1939), qui vit dans le psychologisme en logique "une des plus grandes erreurs de la philosophie". Il conçut la logique comme "une science pure et *a priori*". Ses écrits de logique sont: *Logische Untersuchungen* (Halle, 2 vol. 1900), *Formale und transzendentale Logik* (Halle 1929), etc.

XV. - La logique mathématique. — Nous arrivons ainsi à la logique moderne qui a pris une forme mathématique et qui apparaît non seulement comme une réaction à la conception psychologisante du siècle passé, mais aussi comme une conséquence du succès des mathématiques, dans le même siècle, succès qui a fait de ces sciences l'idéal de toute science, donc de la logique aussi.

Voici comment définissent la logique mathématique D. Hilbert et W. Ackermann: "La logique théorique appelée aussi logique mathématique ou logique symbolique est une extension des méthodes formelles des mathématiques dans le domaine de la logique".

Deux sont les caractéristiques principales de la logique mathématique:

— l'aspect du calcul algébrique qu'elle révétit;

— l'aspect purement formaliste.

Comme calcul algébrique, elle se présente comme un "système axiomatique", dont on choisit *ad placitum* les idées initiales et les axiomes et en vertu de quelques règles dites de "composition" et de "déduction", on construit de nouvelles formules.

Le caractère "formaliste" de la logique est déterminé par le fait qu'on n'emploie que des signes dépourvus de toute signification, de tout contenu: le signe n'est soumis qu'aux règles qui déterminent son jeu quand il entre en combinaison avec les autres signes.

La conception de la logique comme calcul est plus ancienne et on trouve de telles idées même dans l'antiquité. Mais le premier qui est vraiment un précurseur de la logique est Raymond Lulle, dont nous avons déjà parlé. Hobbes et Condillac ont eu l'idée d'un "calcul logique", mais ils n'ont pas abouti à laisser quelque chose qui vaille la peine d'être mentionné.

Mais le vrai précurseur de la logique mathématique est Leibniz (1646-1716), qui, bien qu'il n'ait pas réalisé un "système" a eu beaucoup d'idées qu'on retrouve actuellement dans la logique.

En même temps et après Leibniz apparaissent des traités où l'on expose des idées très proches.

> Gerolamo Saccheri (1666-1733) a publié la *Logica Demonstrativa* (1697), longtemps oubliée mais rappelée à la mémoire des logiciens par G. Vailati, dans son étude *Di un'opera dimenticata del P. Gerolamo Saccheri* (G. Vailati, Scritti, 1911).
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> L'influence de Leibniz est visible sur J. H. Lambert dans son livre *Neuer Organon* (1771) et sur J. A. Segner qui publia *Specimen Logicae universaliter demonstrata* (1740).

S. Ploucquet écrivit dans le même esprit *Fundamenta Philosophiae speculativae* (1759).

Dans le même temps, beaucoup de mathématiciens emploient des figures géométriques pour représenter les opérations logiques. Euler (1707-1783) figurait les diverse types de syllogismes par des cercles. Le mathématicien J. D. Gergonne (1771-1859) a repris l'étude géométrique des syllogismes, en partant des résultats obtenus par Euler. De la même manière ont étudié le syllogisme: John Venn (1834-1923) et C. L. Dodgson (1832-1898).

Salomon Maimon (1754-1800), philosophe influencé par Kant, publia un livre *Versuch einer neuen Logik, der Theorie des Denkens,* dans lequel il essayait de construire une algèbre logique.

En liaison avec le problème des jugements et de leur quantité il faut mentionner W. Hamilton (1788-1886), qui donna une théorie de "la quantification du prédicat".

Nous arrivons à la tentative de construire une algèbre de la logique, oeuvre de George Boole (1815-1864), dans ses travaux *The mathematical analysis of logic* (1847) et *An investigation of the laws of thought* (1854).

Autres "algébristes" de la logique sont:

— W. Stanley Jevons (1835-1882) qui publia *Pure Logic or the logic of quality apart from quantity* (1864) et *The principles of science* (1874).

— Augustus de Morgan (1806-1871) est l'auteur des ouvrages *Formal Logic of the calcul of inference, necessary and probable* (1847).

— Hugh MacColl (1837-1909) auquel on doit les travaux *The calculus of equivalent statements and integration limits* (1877), *Symbolic reasoning* (1905) et *Symbolic Logic and its application* (1906).

Un autre groupe d'algébristes de la logique sont:

— R. Grassmann (1815-1901) qui nous a laissé les écrits: *Der Begriffslehre oder Logik* (1872) *Die Logik und die anderen logischen Wissenschaften* (1890).

— C. S. Peirce (1839-1814) qui écrivit: *On an improvement in Boole's*

Calculus of Logic (1867); *Grounds and validity of the laws of logic* (1868); *On the algebra of logic* (1880); *The regenerated logic* (1896).

— Ernest Schröder (1841-1902), auteur des travaux d'algèbre de la logique très importants: *Der Operationskreis des Logikkalküls* (1887); *Ueber das Zeichen* (1890); *Vorlesungen über Algebra der Logik* (3 vol. 1890-1895).

Enfin, nous arrivons à Gottlob Frege (1848-1925) qui est le point culminant de la logique mathématique, bien qu'elle se soit développée indépendamment de lui.

Ses écrits principaux sont:

— *Begriffschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* (1879, Halle); *Die Grundlagen der Arithmetik* (1884, Breslau); *Funktion und Begriff* (1891, Jena); *Ueber Sinn und Bedeutung* (1892); *Grundgesetze der Arithmetik* (2 vol. 1893-1903, Jena).

L'idée fondamentale de Frege est que les mathématiques sont une branche de la logique. Pour cette conception, il avait besoin d'un instrument logico-mathématique de la même précision que les mathématiques elles-mêmes.

La logique mathématique lui doit le calcul propositionnel, qu'il a complètement axiomatisé, la découverte de la logique des prédicats et surtout la découverte de la fonction logique, concept central dans la logique des prédicats.

Le calcul propositionnel, chez Frege, part de deux idées primitives, la négation et l'implication, des variables propositionnelles susceptibles de prendre deux valeurs, le vrai et le faux, et de six axiomes (au commencemment il admettait neuf axiomes), de deux règles de déduction la substitution et la *modus ponens*. Avec ses idées symboliques il obtient des théorèmes, qui étaient connus des Stoïciens et Scolastiques.

Il introduit ensuite la notion de fonction propositionnelle, avec des explications un peu hésitantes et compliquées. Le jugement de prédication est remplacé de cette façon par la fonction logique: "x a le prédicat f" est symbolisé par f(x).

Le symbolisme de Frege est très compliqué, même inadmissible.

En même temps apparaît en Italie un mouvement logico-mathématique dû a Giuseppe Peano (1858-1932) et à son école.

À Peano on doit: *Arithmetica Principia novo methodo exposita* (Torino, 1889); *Notations de logique mathématique* (Torino, 1894); *Studi di logica matematica* (Torino, 1897); *Formulario matematico* (1897, puis 1905-1908).

Les recherches de Peano sont aujourd'hui moins mises en valeur, mais elles ont eu un rôle décisif dans le développement ultérieur de la logique mathématique et la fondation de l'arithmétique (d'après l'aveu de Russell lui-même). Peano veut, ainsi que Frege, donner un fondement à la théorie des nombres entiers. Dans ce but il a besoin d'un appareil logique plus adéquat, qu'il construit. Il cherche une juxtaposition de la logique et de la mathématique.

Dans sa conception, la logique mathématique étudie les lois logiques à l'aide des formules et des méthodes des sciences mathématiques.

La construction axiomatique de la logique est bien claire dans les écrits de Peano. Son symbolisme est très simple et il a laissé des traces dans tout le développement de cette science.

L'école de Peano comprend des logiciens et mathématiciens bien connus, dont nous citons:

- Cesare Burali-Forti (1853-1931) *Una questione sui numeri transfiniti* (Palermo, 1897) il a écrit *Logica Matematica* (Milano, 1893), *Sur quelques propriétés des ensembles d'ensembles* (1895), etc.
- Giovanni Vailati (1863-1909), qui publia *Le proprietà fondamentali delle operazioni della logica* (1891), *Dependenza fra le proprietà delle relazioni* (1891), *Il metodo deduttivo come Istrumento di ricerca* (1897), *La logique mathématique dans sa dernière phase de développement* (1899), etc.
- Alessandro Padoa (1888-1937) auquel on doit les écrits, *La logique déductive dans sa dernière phase de développement* (1922), *Il contributo di Peano all'ideografia logica* (1933), *Ce que la logique doit à Peano* (1936).
- Giovanni Vacca (1872-1953), dont nous citons les travaux, *Sui precursori della logica matematica* (1896), *Logica Matematica* (1929).

Autres logiciens italiens, qui sans pouvoir être appelés "peaniens", se sont occupés des problèmes de logique posés par Peano, sont:

- Annibale Pastore (n. 1868), le plus important logicien après Peano, qui écrit: *Sopra la teoria della Scienza* (1903), *La logica del potenziamento* (1936), *Logica sperimentale* (1939), *Logicalia* (publiée après sa mort, en 1957).
- Federigo Enriques, dont nous possédons les travaux, *Problemi della Scienza* (1906), *L'évolution de la logique* (1926), *La signification de l'histoire de la pensée scientifique* (1935) etc.
- Ludovico Geymonat, professeur à l'Université de Milano, adepte d'un néo-Rationalisme très proche de l'empirisme logique. On a de lui: *Le idee direttive del neo-empirismo* (1939); *Analisi critica del recente indirizzo di logica formale di Carnap* (1940-42, *Studi per un nuovo razionalismo* (1945), etc.

Nous citons encore:

Franco Spisani, avec la *Logica Produttiva*;

A. Pasquinelli, avec *Introduzione alla logica Simbolica*;

E. Casari, avec *Lineamenti di logica matematica*; M. Boldrini, avec *Teoria della statistica*; E. Persico, avec *Fondamenti logici della Scienza*; Ev. Agazzi, avec *La logica simbolica*.

Enfin, la philosophie de la cybernétique trouve bien des idées nouvelles dans la conception de V. Somenzi, dont nous citons le volume paru sous sa direction, *La filosofia degli automi* (Torino, 1965).

Nous remarquons spécialement, pour leur érudition, les travaux de Francesca Rivetti Barbó, professeur à Perugia: *L'antinomia del Mentitore nel pensiero contemporaneo* (1971), *Il senso e il significato* *di Frege* (1963), *Le antinomie concettuali ed il paradosso di Russell* (1957), etc.

Enfin, nous arrivons, après Frege et Peano, à l'oeuvre monumentale de B. Russell (1872-1970) et A. N. Whitehead, *Principia Mathematica* (3 vols. 1910-1913). La logique mathématique y est complètement édifiée. Le premier volume traite des problèmes de logique mathématique:

1. Théorie de la déduction qui est le calcul propositionnel.
2. Théorie des variables apparentes, qui est le calcul des fonctions propositionnelles, et qui comprend aussi les paradoxes logico-mathématiques avec la théorie des types (simple et ramifiée).
3. Classes et relations.

Le calcul logique est complètement axiomatisé.

Pour le calcul des propositions, Russell accepte deux idées primitives, la négation et la disjonction, et cinq axiomes (qui seront plus tard réduits à quatre). Comme règles de déduction, *Principia* acceptent la règle de la substitution et la règle du *modus ponens*. On obtient des "formules vraies", qui seront appelées *tautologies* par Wittgenstein.

L'idéal de Russell était celui du Frege: constituer la logique entièrement comme une science mathématique et dériver ensuite les mathématiques de la logique. Mais cette tentative se heurta, dès le commencement, à des grosses difficultés, et surtout à l'apparition des paradoxes logico-mathématiques. Cela a demandé l'introduction d'une théorie nouvelle dite la théorie des types, et le principe de réductibilité.

L'apparition des paradoxes a donné lieu à des recherches importantes et de nombreuses études.

La logique mathématique se ramifia alors dans trois directions, correspondant à trois conceptions philosophiques diffèrentes.

1. *Le logicisme*, théorie de Frege-Russell, selon laquelle les mathématiques sont une branche de la logique. Principaux adeptes: Chwistek (n. 1884); Ludwig Wittgenstein (1889-1951) auteur du célèbre *Tractatus logico-philosophicus*; J. Nicod, P. Ramsey, R. Carnap, etc.

2. *Le formalisme* qui veut établir les fondements des mathématiques à l'aide d'une logique symbolique, "parce que la pensée logique trouve son image dans un calcul algébrique". La logique et les mathématiques doivent être construites en même temps, l'une de ces sciences se fondant sur l'autre. Cette conception est due à David Hilbert (1862-1943). Oeuvres principales: *Axiomatisches Denken* (1928); avec W. Aeckermann, *Grundzüge der theoretischen Logik* (Berlin, 1928); avec P. Bernays, *Grundlagen der Mathematik* (Berlin, 2 vols. 1934-1938) etc.

Adeptes de cette école: W. Ackermann, Paul Bernays, J. von Neumann, etc.

3. *L'intuitionnisme* est dû aux hollandais L. Brouwer et A. Heyting. L'activité mathématique est une activité originaire de la pensée et tout ce qui est exact est mathématique. Donc il serait un cercle vicieux de fonder les mathématiques sur la logique ou sur toute autre science. Les partisans de cette conception admettent un affaiblissement du principe du tiers exclu, qui ne s'applique que dans les domaines finis. Heyting a formalisé la logique intuitionniste.

La logique mathématique évolue ensuite vers les logiques polyvalentes et modales. S'il est possible de choisir convenablement et conventionnellement les idées primitives et les axiomes de la logique, alors il serait possible d'imaginer qu'une proposition peut admettre plus de deux valeurs (le vrai et le faux).

On a construit de telles logiques où les propositions admettent plus de valeurs de vérités: ce sont les *logiques polyvalentes.* Telles sont les logiques de Lukasiewicz, de Botchvar, la logique modale de Moisil, etc.

Les systèmes formels de la logique ont été après étudiés en eux-même, en arrivant ainsi à une véritable technique formelle. Les systèmes formels sont des constructions symboliques conventionnelles, où les symboles sont dépourvus de toute interprétation. Dans leur construction on a distingué deux parties:

— L'étude du mode de construction du système formel, les conditions pour que les formules construits soient correctes et leurs possibilités déductives; c'est la *syntaxe logique* du système. Le terme a été employé pour la première fois par Wittgenstein, mais la théorie de la syntaxe d'un système a été faite par R. Carnap dans son livre *Logische Syntax der Sprache* (Wien, 1934).

— L'étude du système en liaison avec les notions de vérité et de fausseté est appelée *sémantique.* On voit bien que la sémantique est liée à l'interprétation d'un system formel. La théorie de la sémantique a été faite par A. Tarski, qui a introduit ce terme dans son étude *Grundlagen der Wissenschaftlichen Semantik* (1936), mais il a développé la *sémantique dans Der Wahrheitsbegriff in den formalisierten Sprachen* (Leopoli, 1936).

La plus grande déficience des systèmes formels est l'existence à leur intérieur des formules "indécidables", c'est-à-dire des formules dont l'appartenance à un système ne peut être démontrée ni infirmée. C'est ce troublant paradoxe de Kurt Gödel, qu'il démontra dans son étude *Ueber formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme* (1931).

Au moment actuel, trois sont les modalités de construction de la logique mathématique:

1. La logique comme système formel axiomatique (Russell, Hilbert, etc.).
2. Les logiques à schémas déductifs.
3. Les logiques combinatoires.

Les logiques à schémas n'emploient pas d'axiomes mais seulement des schémas de dérivation. Dans le cadre de ce formalisme, la logique n'est plus une théorie des propositions, mais une théorie de la démonstration. Les logiques à schémas ont été construites pour la première fois par G. Gentzen dans son travail *Untersuchungen über das logische Schliessen* (1934) et St. Jaskowski *On the rules of suppositions in formal logic* (1934).

Les logiques combinatoires conçoivent les opérations logiques intuitives comme opérations "pures" et cherchent à trouver un équivalent formel pour elles. Les formalismes purs de la logique combinatoire ont été construits par Haskel B. Curry dans *Grundlagen der Kombinatorischen Logik* (1930), *Combinatory Logic* (1958), etc.

Une autre direction dans laquelle s'est développée la logique combinatoire est celle appelée des calculs de λ- conversion, créés par Alonzo Church, S. C. Kleene, J. B. Rosser, etc.

En résumé, la logique mathématique, qui intentionnait assurer les fondements des mathématiques, est arrivée à être considérée comme un "langage bien fait", avec ses règles syntaxiques et sémantiques.

C'est une logique du langage scientifique le plus général, mais il se heurte à de telles difficultés qu'on ne peut dire si ce qu'on gagne pèse plus lourd que ce qu'on perd.

Il devient clair pour quiconque a été attentif à notre exposition, que la logique mathématique n'épuise pas l'acte de pensée. C'est pour cela que, dans les derniers temps, beaucoup de voix se sont faites entendre, pour revenir à une plus large compréhension de la logique sur tout son registre et ne pas la limiter à son aspect de "langage scientifique" Entre ces réactions nous mentionnons:

— I. Piaget, avec ses livres, *Classes, Relations, Nombres* (1942) et *Traité de Logique* (1949), etc.

— René Poirier, qui publia *Le Nombre* (1938), *Logique et Modalité* (1952) etc.

— Robert Blanché avec *Raison et Discours* (Paris, 1965) et *Structures Intellectuelles* (Paris, IIème éd. 1969), etc.

— Leo Gabriel, auquel on doit *l'Integrale Logik* (Wien, 1965), etc.

— Petre Botezatu, qui publie (en roumain) *Esquisse d'une logique naturelle* (1969, Bucarest).

— Anton Dumitriu, avec *Logique formelle et logique formalisée* (Bucarest, 1966) et *Theory and System* (Bologna, 1970), etc.

XVI. - CONCLUSIONS. — Nous avons poursuivi les étapes de l'évolution de la logique le long de son histoire.

Comme on l'a vu, chaque étape a développé un certain aspect de la logique. Dire qu'un seul aspect est le seul valable c'est se soumettre aveuglément à un préjugé. L'histoire de la logique nous a appris que chaque modalité - *Gestalt* comme l'appelle Bochenski - a sa valeur. De cette façon, la logique devient son histoire; l'histoire du *lógos* dans son devenir, dans son expansion dans le monde. Le lógos a un nombre indéfini de modalités; mais lui-même est la sommation intégrale de ses modalités. Même si ces modalités apparaissent quelquefois comme opposées, il faut les comprendre dans leur complémentarité.

D'un point de vue très général, on peut diviser la logique dans deux grands chapitres:

1. la logique comme analyse du mécanisme de la pensée;
2. la logique comme analyse du mécanisme de l'expression dans laquelle s'incorpore la pensée.

Notre exposition a montré cette continuelle oscillation des recherches logiques: entre ces constructions logico-grammaticales et la structure du mecanisme effectif de la pensée.

Mais ces deux directions de recherches se complètent réciproquement: le mécanisme de la pensée et la forme de ce mécanisme sont deux choses connexes, ou deux faces de la même chose. C'est pour cela que pour les Scolastiques le *lógos* était *sermo et ratio*, parole et raison.

La place de la logique parmi les autres sciences devient ainsi éminente. La logique, en tant que raison de toutes les autre sciences ne peut être de nature identique avec une science particulière. Comme nous l'avons vu, au cours de notre exposé, ni Aristote, ni les logiciens Scolastiques n'ont conçu la logique comme une science quelconque, mais comme la science des sciences, *scientia scientiarum*, *ars artium*, *doctrina doctrinarum*. Ce n'était pas seulement à cause de son importance qu'ils donnaient ce nom à la logique, mais surtout à cause de son caractère de principe par rapport aux autres sciences. Ainsi que toute science a besoin des principes pour se constituer, de même toutes les sciences ont besoin d'une science grâce à laquelle elles peuvent être théoriquement constituées. La place de la logique dans le contexte des sciences devient ainsi tout à fait spéciale.

Et cette particularité apparaît d'une manière évidente si nous pensons à ce qui est l'objet de la logique. Toute science présuppose un domaine de faits (concrets ou abstraits) qui fait l'objet de la pensée; mais l'objet de la pensée dans le domaine logique est la pensée elle-même. La pensée dans sa fonction de s'examiner elle même - fonction qui est la logique - apparaît ainsi comme la chose la plus extraordinaire qui arrive dans le monde; elle pense tout, mais pense, par un repliement sur soi-même, ce qui pense tout. C'est le fait le plus élevé, dans l'échelle des valeurs et Aristote avait raison de dire que la pensée dans sa fonction la plus pure est le "Bien le plus pur". C'est la plus grande dignité jamais accordée à la pensée, et pour Aristote, par cette dignité, la pensée reçoit un caractère "divin".

La logique, dans sa dignité le plus haute, est ainsi "la pensée qui se pense elle-même":

ἡ νόησισ νοήσεως νόησις.

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A. DUMITRIU

TRANSLATION: J. E. HOLMSTROM - CHRISTCHURCH, HAMPSHIRE.

A HISTORY OF LOGIC

THE STAGES OF ITS DEVELOPMENT

Second Part

The following is the second part of the article begun in the last issue of Scientia. *The author continues here his analysis of the historical stages through which logic has passed in reaching its present stage of development. Attention is given to the logic of the Stoics, that of Rome, the formation of the terminology, the scholastics of the commentators and, finally, the scholastics of mediaeval times. In the course of it the reader is introduced to problems which have since been almost forgotten or which until now have scarcely been mentioned: those of the* parva logicalia, *the* suppositio, *the* syncategoremata, *the* insolubilia, *etc. Scholastic logic is characterised by the stress it laid upon the duality of* sermo *(terms) and* ratio *(reasoning), scholastics forming a bridge between the logic of the Peripatetics and that of the Stoics. The logic of the Renaissance likewise shows multiple facets but these are even less known that the facets presented by the logic of the Middle Ages. We then go on to consider methodology, which is the outcome from the emergence of the various sciences and to examine the successive "philosophical" trends inherent in the logic of Kant, of Hegel, of Marx and Engels, of Husserl and in psychology, value theories, etc. After this a long chapter is devoted to mathematical logic, displaying both its validity and its scope. In his final section the author concludes that logic, so far from being just a science among others, is indeed what the schoolmen called it: the* ars artium, *the* scientia scientiarum, *the* doctrina doctrinarum; *a purely contemplative science with the peculiar characteristic that the object thought about is hought itself.*

VIII. - LOGIC AFTER ARISTOTLE. — After the time of Aristotle the peripatetic school became increasingly preoccupied with intrinsic relations between judgements and less so with relations between concepts. Prantl (*Geschichte der Logik im Abendlande*, I, p. 351) expressed this very well in his observation that "the logical function of propositions began to yield place to their grammatical function". This was the aim pursued by Aristotle's immediate followers Theophrastes and Eudemos, whose primary concern was with hypothetical syllogism, the *modus ponens* and the *modus tollens*. Formalism was in the air and reached its culminating point among the Stoics.

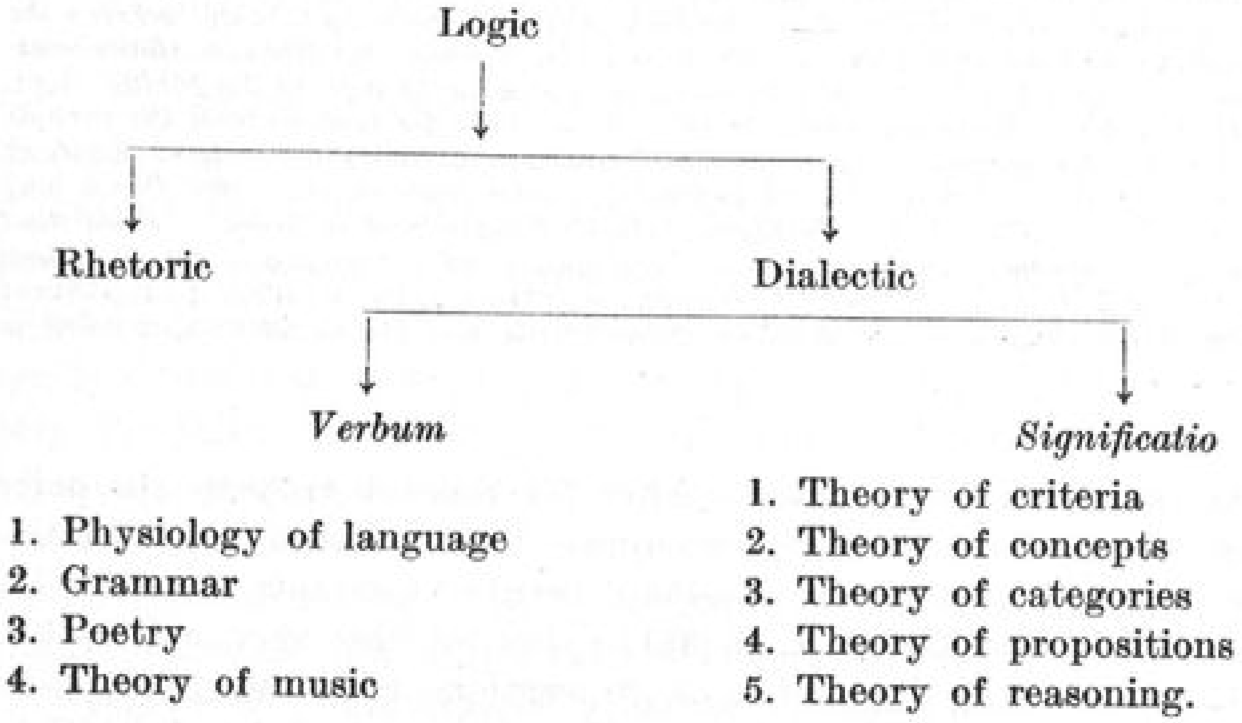
IX. - THE STOICS. — All historians of philosophy agree as to the post-Aristotelian period of Greek philosophy having been characterised by increasing pragmatism.

The specific feature of post-Aristotelian philosophy - and therefore of post-Aristotelian logic - can be identified, from this point of view, with an increasing interest in the practical aspects of philosophy and a lessened interest in theoretical speculations. All the main schools of philosophers after Aristotle, such as those of the Stoics, of Epicurus and of the Sceptics, are characterised by this practical slant of their philosophy and therefore of their logical theorising.

The Stoic school was founded by Zeno of Cytium at the end of the fourth century B.C. Its most powerful personality was Chrysippos (281-208 B.C.) surnamed "the gatepost" who gave the school its structure. As Cicero said of him in *The Academics*, II, 24: *Chrysippus qui fulcire putatur porticum Stoico-*

rum (Chrysippus was regarded as the buttress supporting the archway of the Stoics). Diogenes Laertius (in his *Lives and teachings of the great philosophers* VII, 180) tells us his renown as a dialectician was such as to generate the belief that "if the gods indulged in any dialectics it could not be other than that of Chrysippos". This great dialectician of ancient times was credited with more than 311 books, many being of a logico-grammatical character.

In the Stoic view logic occupied a place of quite special importance as being among the "virtues". It was in this sense that Diogenes Laertius wrote (op.cit. VII, 47) that the Stoics "look upon dialectics as filling an indispensable need and as being a virtue (*arete*) itself as well as embracing other more specific virtues". It is among the Stoics that "logic is so designated for the first time, being divided by them into the two branches called respectively rhetoric and dialectic" (whereas others called both of these "dialetics" and the latter in turn comprising two major sections, namely (1) meaning: σημαινόμενον (*significatio*) and (2) expression: σημαῖνον (*verbum*). Each of these sections was subdivided into several others which can be displayed as follows:



It is impossible to embark here on a study of all these divisions but a few words will be said on the Stoic conception of how logic is related to language and to hypothetical reasoning.

1. *Logic and Language.* — Why did the Stoics attach such importance to logico-grammatical investigations? They did so because of the special way they conceived the relationship between thought and language. Thought was inconceivable to the Stoics otherwise than through its verbal expression. They situated the governing principle of human existence (*hegemonikón*) in the heart, whence a breath "radiated" to the larynx setting it in vibration and producing speech. Thinking and speaking were regarded as so closely interlinked that neither of these faculties could function save on the basis of the other. Thus speech was identified with thought, being in fact nothing else than thought rendered explicit.

Unfortunately we no longer have the Stoic texts from which we might have been able to gain a clearer idea of how this conception was elaborated; but to describe the Stoics as nominalists and nothing more would certainly be an oversimplification. It is impossible to agree with some modern writers (notably Lukasiewicz) that these philosophers were logicians of signs. Furthermore we possess a text by Boethius (*De Syllogismo Categorico*) in which the purpose and proper use of signs is adequately explained: *Quotiescunque ita dicimus ut litteras pro terminis disponamus, pro brevitate hoc et compendio facimus* ("Whenever we express ourselves in this way, replacing words by letters, we do so for the sake of brevity and to save time").

2. *Hypothetical Reasoning.* — The Stoics divided propositions into two categories: (a) simple (ἁπλᾶἀξιώματα) and (b) not simple (οὐχάπλᾶ ἀξιώματα). These correspond to Russell's distinction between *atomic* and *molecular* propositions.

The non-simple propositions are of seven types:

(1) The hypothetical proposition formed by using the conjunction "if", e.g. "If it is day there is light".

(2) Adjunctive proposition formed by using the conjunction "because", e.g. "It is day because there is light".

(3) Copulative proposition formed by using the conjunction "and", e.g. "It is day and there is light".

(4) The disjunctive proposition formed by using the conjunction "or", e.g. "It is day or it is night".

(5) The causal proposition formed by using the conjunction "because", e.g. "It is day because there is light".

(6) The comparative proposition, formed by using the conjunction "Than", e.g. "It is day more (rather) than night".

(7) Finally, Diogenes Laertes mentions the non-simple type of proposition formed by negation, e.g. "It is not day".

The Stoics spent much time considering all the possible constructions of compound (non-simple) propositions. Though we do not know all of these in detail we can nevertheless infer from the information that has come down to us from Plutarch in his *Symposium Questions* (VII, 9) that the number they studied must have been very large, for he tells us that Chrysippos claimed, in his book *Dialectical definitions*, that by means only of ten simple propositions it would be possible to make over a million non-simple ones.

The important thing in all this discussion is that all these non-simple propositions are either true or false according to the veridical values of their component simple propositions alone.

Thus the Stoics were well acquainted with the "truth functions" of modern mathematical logic.

Their study of propositions served as an approach to that of reasoning. The Stoics held that the categorical syllogisms to which Aristotle paid so much attention were incorrect from a formal standpoint although correct as regards their material content (Alexander of Aphrodisias in his commentary *Ad Analytica Priora*). They divided the processes of reasoning into two categories:

(1) Demonstrable arguments which these being the various syllogisms proved by Aristotle in his *Prior Analytics*.

(2) There are five types of the latter, as conveyed to us by Diogenes Laertes and Sextus Empiricus. These five types of reasoning are fundamental ones "to which all others can be reduced". The Stoics studied them according to

their forms, which they called "the figures", meaning their purely schematic forms, those used for expressing them verbally.

The types of reasoning are as follows:

I. If the first is, then the second
Here the first is (true)
Therefore the second is (true)

II. If the first is (true), then the second is (true)
Here the first is not (true)
Therefore the second is not true

III. The first is not at the same time as the second
Here the first is
Therefore the second is not

IV. Either the first or the second is
Here the first is
Therefore the second is not

V. Either the first or the second is
Here the second is not
Therefore the first is

These "forms" - which, as already pointed out, are employed solely "for brevity and to save time" - have been taken over as they stand by the mathematical logicians. Lukasiewicz maintains that there can be no form of deduction other than these literal forms, but the mathematical logicians have gone further by translating the Stoics' five fundamental chains of reasoning without use of the copula "is", like "If the first then the second; but the first therefore the second". Benson Mates deprecates C. Prantl's having translated these reasonings with the inclusion of "is", arguing that this word has imported "the greatest confusion into the understanding of Stoic logic", (B. Mates: *Stoic Logic*, p. 88, Berkeley and Los Angeles, 1961); but the ancients seems to have come much nearer to an understanding of the Stoic reasoning, as is exemplified in the following translation [from Greek into Latin] by Boethius:

Si A est, est B
Atqui est A
Est igitur B

wherein the copula is in fact retained.

It is true that the logic of the Stoics differs from that of Aristotle. Where lies the difference? Il lies in the fact that the logic of Aristotle is the logic of the concept whereas the logic of Crysippos is the logic of propositions.

This difference is most clearly seen in the field of reasoning: for Stoic logic is propositional in contrast to Aristotelian logic which is conceptual. Yet both are legitimate.

X. - Rhetoricians and Commentators. The Ancient Scholastics. — The Stoic school was the second of the major ones in ancient times, after which there is nothing much that need be mentioned specifically as the work continued, along one line or another, on the basis of the capital already accrued.

In the Epicurean school, logic was transformed into a theory of knowledge known as *Canonical.* In the Sceptical school it took on a negative character and in the New Academy it became a probabilistic conception.

Finally, logic passed over to Rome where the cultivation of this science was governed by the needs of rhetoric. There was a period during which logical linguistics took shape in the language of Virgil, the terminology having been hesitant and uncertain. A predominance of Stoic conceptions can be seen, making some impression on Latin logic which later influenced the true commentators in their turn. Cicero in his logical writing endeavoured to use approximative terminology.

In these ways what may properly be called the scholarship of antiquity was shaped by the hands of the "ancient" commentators: Boethius of Sidon, Alexander Aegeus, Aspasius, Adrastus, Herminus and others working on the heritage of Aristotle and the Stoics. The commentators who followed them, either Greek or Latin, were "eclectics" who drew both upon the Peripatetics and upon the Stoics, though some professed to be purely peripatetic and were not even conscious of their own eclecticism. It should be noted that during this "pre-scholastic" period - which lasted until the time of Boethius (480-524 A.D.) - the seven liberal arts which were to play so important a role in the Middle Ages appear to have been constituted. The following commentators ought to be named as having been active in this pre-scholastic period: Appuleius, Galen, pseudo-Galen, Alexander of Aphrodisias, Marius Victorinus, Augustine, Marcianus Capella, Boethius, Cassiodorus.

Ancient scholasticism was characterised by certain features which can be summed up as follows:

1. Absence of any new logical or conceptual discoveries, save possibly those ascribed to Galien, the fourth figure of syllogism and the theory of equipollence.
2. Aristotelian and Stoic logic becoming complementary, not however forming a synthesis but accepted simultaneously as representing two different (but compatible) modes of thought.
3. The Stoic conception of logic losing its marked ontological character.
4. Logicians, under this same Stoic influence, becoming interested in endless classifications and in the study of verbal forms classified in every possible way.
5. Scholastic terminology originating in this inclination to study and classify verbal forms.
6. Logical terminology becoming completely established.
7. Finally, it should be noted as a very important fact that during this period logic became recognised as a liberal art with which every educated man should be expected to be familiar.
8. Thus scholastic logic has a natural foundation, being simply the apogee of ancient logic.

XI. - Scholastic Logic. — The logic cultivated in this period was for long afterwards so neglected and even despised that nobody really understood what it represented. Its re-evaluation began in the works of the Polish logician J. Lukasiewicz who showed that much of what is now called the "propositional calculus" was in fact well known to the scholastic logicians.

The scholastic era has been thought of as a dead one, dominated by the Church and incapable of yielding anything of value. But in point of fact there was at that time a scholastic world in full intellectual flower, wherein philosophical and logical problems of the greatest importance were being worked upon. The fundamental error committed in evaluating the era has been to confuse its intellectual production with theology which is quite wrong.

The temper of the Middle Ages was rationalist above all. At that time the whole range of philosophy accessible to the human intellect was held to be rational, and no philosophy was deemed possible outside of reason. Thus Albert the Great (Albertus Magnus) asserted that "whatever the philosopher has to say he must say with reason" - *id quod dicit, dicere cum ratione*. Fulfillment of this condition called for perfect familiarity with the logical instrument; hence the wide scope of the logical researches undertaken.

This characteristic feature of the Middle Ages, rationalism and its logical implications stretched to their utmost limit, strikes a paradoxical note in the mentality of the schoolmen. On the one hand the Middle Ages were mystical and theological, on the other hand they were rationalist and logicist.

Let us now trace the evolution of scholastic logic and of its major problems, although obviously we can do so only in outline.

1. *The Sources and Development of Logic.* — The primary origins of scholastic logic are to be found in the heritage from Greece and Rome. The Middle Ages had at their disposal the work of Marcianus Capella and in particular the encyclopaedic *Satiricon* which provided a full conspectus of the seven liberal arts (including logic as one of the seven).

The works of the fourth century neo-Platonic philosopher Simplicius, commenting those of Aristotle, were also being studied. The books by Marius Victorinus, including his translation of the *Isagoge* by Porphyry, are yet another source of mediaeval problems in logic. The translations and commentaries of Boethius were much esteemed at that time, and likewise those of Cassiodorus. Also the writings of Apuleius, particularly his *De Dogmate Platonis,* were among the origins of scholastic logic.

Finally, the works of certain Western pre-scholastic authors such as Isidor of Sevilla (AD 636) should be mentioned, his book *Originum sive Etymologiarum libri XX* being a sort of encyclopaedia beginning with the seven liberal arts.

In England the same role was played by the Venerable Bede (AD 674-735) whose book *De Natura Rerum* was inspired by the works of Isidor of Sevilla.

The development of scholastic thought (especially in the field of logic) was further influenced by the Eastern Christian logicians, especially by John Damascene (676-754) whose *Origin of Knowledge* opens with a philosophical introduction to the logic of Aristotle.

The Syrian philosophers, too, were cultivators of logic and wrote seventh-century commentaries: Sever Sebokt has left us one on the *Analytics* and another on *De Interpretatione*; Athanasius (in 645) translated the *Isagoge* of Porphyry, Georgios translated the whole of the *Organon* and Jacob of Edessa the *Categories.*

Starting from this inheritance, scholastic logic took shape by several stages which can be listed as follows:

(a) The Carolingian renaissance with its reorganisation of teaching, characterised by new centres of culture appearing in the Abbeys of St. Gall, Fulda, Reichenau, Corbie, etc. This included the work of Alcuin (735-804) in France and of Rhaban Maure (780-856) in Germany.

(b) The advent of that great figure in the Middle Ages, John Scottus Eriugena († 877), is to be accounted an important event for the evolution of mediaeval culture as a whole, to which he imparted a neo-Platonic stamp. A series of logical commentaries was left by his disciples Heiricus d'Auxerre and Remigius d'Auxerre.

Nor must we omit to mention the humanist Gerbert d'Aurillac (+1003) who lectured at Rheims on the *Isagoge* of Porphyry, on the *Categories* and *De Interpretatione* of Aristotle, and on the *Topica* of Aristotle, besides commenting all the books of Boethius.

After these authors the development of logic was continued by others, including first of all Abelard (1079-1142) whose great work was *La Dialectique* and one of whose disciples wrote *De Intellectibus*. Among the works of Gilbert de la Porrée († 1154) *De sex principiis* should be cited. *Metalogicus* is the title of a very important treatise (especially as regards the history of logic) written by John of Salisbury († 1180), another pupil of Abelard. The book entitled *Sentences*, by Pierre Lombard († 1164), is not a treatise of logic but it contains a mass of observations which have influenced that science.

In the twelfth century, Aristotle was well known. By that time all his works of logic had been translated, reviving them "from inanition and sleep" (*a morte vel a somno*) as was said by Thierry de Chartres, one of the great masters of that place.

The six treatises of Aristotle's *Organon* were now constituted as the *logica antiqua*, divided into two parts: *logica vetus* and *logica nova*. The first of these parts contained his treatments of the *Categories* and *De Interpretatione*; the second was made up of those treatises in the *Organon* that had been translated later by the Arabs, namely the *Analytica Priora*, the *Analytica Posteriora*, the *Topica* and De *Sophisticis Elenchis*.

Another set of problems, not explicitly dealt with in the Aristotelian *Organon*, formed the subject of some smaller logical treatises collectively called *Parva Logicalia*.

The layout of logical works in the Middle Ages was, therefore, as follows:

LOGICA ANTIQUA

(The six treatises of Aristotle)

Logica vetus

1. *Praedicamenta* (*Categoriae*)
2. *Perihermenias*

Logica nova

3. *Priora Analytica*
4. *Posteriora Analytica*
5. *Topica*
6. *De Sophisticis Elenchis*

PARVA LOGICALIA

1. *Proprietates terminorum* - a problem concerning how the predication of a term is modified by its position in the proposition.
2. *Syncategoremata* - logico-grammatical particles whose sense depends on the context as they have no autonomous sense.

3. *Insolubilia* - logical paradoxes, centered upon the antinomy of the liar.
4. *Obligatoria* - a problem which determines the time interval during which a proposition has to be affirmed.
5. *Consequentiae* - the problem of hypothetical consequences, known today as the propositional calculus.

But, over and above these problems, attention was concentrated on the nature of universals or, in other words, on general concepts.

2. *Influences.* — The destiny of Greek logic was to meander over a very strange course: it did not reach Europe directly but via the Arabs. While so deviating it acquired an Alexandrine and neo-Platonic flavour, which it never afterwards lost even at the appogee it reached in the hands of Thomas Aquinas. The great Arab philosophers Alkindi († 873), Alfarabi († 950), Avicenna (980-1037), El Gazali (1059-1111) and finally Averroës (1126-1198) thus influenced the development of Western philosophy, particularly as regards the direction it took.

Jewish influences in the same neo-Platonic sense can also be cited, under the names of Avicebron (who was active in Spain around 1050), Moses ben Maïmun or Maïmonides (1134-1204) who left us a *Vocabularium logicae* and Levy ben Gerson (active in 1350), with commentaries on the *Isagoge* and on the Aristotelian logic which largely contributed to setting the course followed by the history of logic in the West.

Further, there was Byzantine influence. Under the emperor Constantine Monomachos (1042-1055) a certain recrudescence of philosophy occurred, an Academy was founded and Michael Psellos (1018-1078?) commented the works of Aristotle including among others the *Isagoge* of Porphyry, the *Categories* and *De Interpretatione.*

For some time afterwards a summary of Aristotelian logical science was believed to have been written by Psellos. This wrong attribution was due to C. Prantl who built a complete theory on it, but later the summary was found to be nothing other than a translation made by Georgios Scholarios (1400-1464) of the famous treatise *Summulae Logicales* which had been written by Peter of Spain (1226-1277). That treatise, comprising the whole of mediaeval logic, ran through about 176 editions at the time and served as the foundation for scholastic logic.

Be that as it may, it is certain that the Byzantines exerted an influence on the West, beginning at the time of their first contacts with the East. Apart from Psellos the following may be cited: John Italos who was his contemporary; *Summulae Logicales* who was Mitropolites of Nicaea and author of several Aristotelian works including the *Analytica Priora*; Nikephoros Blemmides († 1272) who produced a *Compendium of Logic*; Georgios Pachymenes (1242-1310) who compiled a work expounding the whole philosophy of Aristotle which has come down to us only in its Latin translation. Another "*Epitome*" of Aristotelian logic was written by Georgios Aneponymos and printed at Augsburg in 1600, in addition to which there were yet further writers of logical commentaries such as Sophronias, Theodoros Metochites († 1330), Maximos Planudes (1260-1310) who knew the Latin language, etc.

This succession of Byzantine philosophers continued later in the 14th, 15th, 16th and 17th centuries, influencing Italian thought and leading to the foundation of the Florentine Platonic schools as well as initiating the neo-Peripatetic movement in Padua and Ferrara.

I lay stress on this Byzantine influence because, generally speaking, it has not been ascribed the importance it deserves. The link between the West and Byzantium is apparent in the earliest of the contacts established with this additional focus of Mediterranean civilisation. Bulaeus (in his *Historia Universitatis Parisiensis,* III, p. 10) referred to it as follows: *Post expugnatam Constantinopolim a Francis et Venetiis sacro foedere iunctis Philippo Augusto rege Lutetiae conditum est collegium Constantinopolitanum ad ripam Sequanae prope forum Malbertinum* (...) *ut Graecorum liberi Lutetiam venientes una cum lingua latina paullatim vetus illud et patrium in Latinos odium deponeret* ("After Constantinople had been conquered by the French and Venetians joined in a sacred federation during the reign of Philip Augustus (1165-1223), the *Collège Constantinopolitain* was established close to the Malbertin Forum on the banks of the Seine, so that translation of the Greek books into Latin might gradually assuage the long sustained ancestral hatred of the Latins,

3. *The Specific Ideas of Scholastic Logic.* — The main peculiarities of mediaeval logic will now be pointed out although it will not be possible here to go into details of them.

A. *The Nature of Logic.* The schoolmen drew a distinction between two kinds of sciences: the "real" sciences concerned with what exists (*de ente*) and the sciences which, not being concerned "*de ente*", are not real but "*sermocinales*" as follows:

1. *Grammatica, quae docet recte loqui*;
2. *Rhetorica, quae docet ornate loqui*;
3. *Logica, quae docet vere loqui.*

Logic, therefore, is what teaches us to speak "truthfully" (*vere*). But in this sense it is not itself a science; it is the "mode" of all the sciences, *modus scientiarum.* Seeing that *modus* has the sense, in English, of "mode", "manner", "procedure", "principle" it follows that logic was conceived as embodying the principles that underlie all of the sciences (*modi scientiarum omnium*). The main argument for this, itself of a logical character, was formulated as follows (Albertus Magnus, *De Praedicabilibus, De natura logicae*): "As a corollary to other arguments it can be added that the *modus* of something cannot be associated with that thing in the division of the genus to which that thing belongs" - *Addunt etiam quod nullius rei modus cum re, cuius modus est, venit in generis sui divisionem.* In other words, since logic is the science of the sciences - *the scientia scientiarum* - it cannot be *one of* the sciences, for in that case it would have to be a genus and at the same time one of the species under that genus.

Logic so conceived is a "formal" science. It is, not however, a direct study of Aristotle's "morphes" but a study of the "*formalitates*" which are mental images of things and which do not rise above the level of abstract conceptions. The purpose of logic is to study these concepts abstracted from things - these *intentiones secundae* - but in order to go beyond them and arrive at the "morphes" of Aristotle it is necessary to have "faith".

B. *The Problem of Universals.* The capital problem - the *prima quaestio* - was the nature of "general concepts". Why so? Because what science meant to the scholastics - like what it meant to Aristotle - was knowledge of the universal: *scientia est universalium,* and all knowledge was considered to be dependent on the nature of the universal. The problem had been stated by Porphyry in the *Isagoge* and it appeared in Latin translations of that little work, particularly in the translation made by Boethius. The amount of attention devoted to this question was fully justified by the need to consolidate the foundations of the sciences comprised in the universal.

Many different solutions were proposed. The 12th century treatise by John of Salisbury entitled *Metalogicus* enumerates thirteen of them, but they can be reduced to four as follows:

(1) *Nominalism* which totally denies the existence of any general ideas, whether within things or outside of them, holding that generalised concepts are simply words (*voces*) used to designate concrete individuals. Nominalism would seem to have been first represented by Roscelin (born in 1050); it culminated and achieved its victory in William of Occam († 1347).

(2) *Conceptualism*, as argued by Abelard, starting from Aristotle's idea that the universal is what can be predicated from the existence of pluralities - *quod natum est de pluribus praedicari* - and that this property is a specific and "native" one - *a nativitate sua.* Abelard pointed out that neither words nor arguments (*discours*) can have this inherent "nativity", for they are but human institutions, whereas things were originated at the instant when their nature was created. That being so, wherein does "the general" reside? The answer is that "the general" acquires a meaning solely from someone's action in predicating it by making a judgement - *sermo-* thereon.

(3) *Transcendental realism*, as formulated quite definitely by Anselm of Canterbury (1083-1108) and erected into a Platonic doctrine of universals by Guillaume de Champeaux (1070-1120). According to this view, universals do have a transcendant existence of their own. It raises many difficulties which in turn have evoked many explanations; special mention must be made of the explanations put forward by the brothers Bernard and Thierry of Chartres.

(4) *Trimodal realism*, of Arab inspiration, as elaborated by Albert the Great (1193-1280) and his disciple Thomas Aquinas (1225-1274). Albert the Great - Albertus Magnus - allowed universals a triple existence, distinguished as:
- *ante rem*, where the universals are the pre-existent cause (*causa praehabens*) for the existence of things;
- *in re*, where the universals are accompaniments of things, conditioning thought about things and the names given to things, being endowed with a potential function;
- *post rem*, where the universals are the abstract form elaborated by the intellect, this form being itself universal and having the characteristic feature that it is communicable.

C. *Scholastic terminology.* Aristotelian logic was studied specifically with a view to meeting didactical and methodological needs. But more must be seen in it than this. The mnemonic devices so abundantly invented by the mediaeval monks were not only wanted as teaching aids; they provided at the same time an algebraic method of thinking about logical problems. The details of this terminology (which have been fully set out, with the relevant Latin verses, in the present author's *Istoria Logicii*, in Roumanian, Bucarest, 1969) cannot be entered upon here but a single example will be given: the verses indicating the figures and moods of syllogism. Here are these well known verses:

Barbara, Celarent, Darii, Ferioque prioris;
Cesare, Camestres, Festino, Baroco, secundae;
Tertia, Darapti, Disamis, Datisi, Felapton;
Bocardo, Ferison habet; quarta insuper addit;
Bramantip, Camenes, Dimaris, Fesapo, Ferison.

Although this sort of terminology had previously been held up to ridicule, the mathematician Gergonne commented on it in his *Annales des Mathématiques* (VII, 1816-1817) that "some little excuse for the crudeness of this artificial versification may perhaps be found in the multiplicity of conditions it was sought to satisfy".

Such, then, was the way the schoolmen scrutinised the *Categories*, *De Interpretatione*, the *Analytics*, *Topica* and *De Sophisticis Elenchis*.

D. *Parva Logicalia.* Brief reference has already been made to what were called the minor problems of logic and these will now be annotated.

1) *Suppositio.* This meant the replacement of one significant term by another. The scholastics listed every possible variety of *suppositiones* and laid down the rules they must obey in order to be correct. The first and most important of these rules was that terms could be substituted for one another only in accordance with the requirements of the "*Copula*" - *juxta exigentiam copulae.*

In modern treatises of logic this problem is omitted, which is really a loss.

2) *Syncategoremata.* There are certain terms such as nouns and verbs that have autonomous meanings: these are called *Categoremata.* There are other terms whose meanings are not self-contained, these being *syncategoremata* or "consignificant particles". All such particles were minutely classified and examined, laying down rules for their correct use. In our own times this problem is looked upon as belonging rather to grammar, which once again is something of a loss to logic.

3) *Consequentiae.* This is the heading under which theoretical consideration was given to what is nowadays called propositional calculus. The main theorems of this calculus (or tautologies) were very familiar to the mediaeval scholars; they knew the difference between formal and material implication and about De Morgan's laws, modal consequences, etc.

4) *Insolubilia.* Problems called "insoluble" were the subject of a whole *tractatus* centered upon the antinomy of "the liar" and its variants; nineteen such variations and fifteen solutions to them were formulated. The theory of logical levels made its appearance in the work of the logician Pierre d'Ailly (1350-1425) and deontic logic appeared in that of contemporary logicians who referred to it as a theory of obligations - *Obligatoria.* John Buridan († 1358) used it as a solution to the so-called *insolubilia.* Finally, Russell's rule for the *circulus viciosus*, stating the impossibility of defining a class by reference to members thereof which presuppose the existence of the class itself, had already been enunciated by Albertus of Saxonia († 1390) in the form "the part can never signify the whole of which it forms a part" - *nunquam part potest significari totum cuius est pars.*

4. *General considerations.* Scholastic logic has been too much neglected and this mediaeval treasury is worth examining now. There cannot be any doubt that the schoolmen were great logicians; moreover were the custodians of a great heritage, the Aristotelian legacy. They did not look upon logic as merely a game played with symbols: their *logos* consisted both of *sermo* and of *ratio;* its study meant deepening the significance of the *sermo* (verbal expression) while at the same time strengthening the *ratio* (reason). Though recognising that the "*formalitates*" which logic pursues are *intentionae secundae* they did not deny that the ultimate ontological basis for *formae exemplares ante rem* can be reached by an act of faith. But right down to that final depth, reason

reigns completely. Scholastic rationalism is truly an affirmation of the *ratio* embodied within the *sermo*.

XII. RENAISSANCE. — The causes that led to the eclipse of scholasticism and the emergence of the experimental sciences are well known. The new mental attitude turned away from the "essences" of the scholastics and the "universals" of Aristotle towards the immediate exploration of what is concrete. This new mentality was reflected in the direction taken by logical investigations. At first there occurred an extension of scholastic logic but later several different schools became discernible.

1. *Ciceronian rhetorical logic.* The first humanist to launch an attack upon scholastic logic was Laurentius Valla (1407-1457). In his principal book, *Dialecticae disputationes contra Aristotelicos*, he used the works of Cicero and Quintilian (*De institutione oratoria*) as a basis for propounding a new and more rhetorically inclined logic. The reaction against scholastic logic found definite expression in the writings of Rudolphe Agricola (1442-1485) a professor at Heidelberg.

2. *The Melanchton school.* In Germany an outstanding man, Philip Melanchton (1447-1560), sought his logic in the writings of Aristotle but read these in a new spirit fertilised by Cicero and Agricola.

3. *The Ramist dialectic.* In Paris a violent assault on Aristotelianism was led by Pierre de la Ramée (1515-1572) a pupil of Jean Sturm who had been taught by Agricola. Ramée maintained firstly that the writings attributed to Aristotle were not authentic, secondly that they contained an accumulation of errors. The most important of his own works on logic were entitled *Institutiones dialectices* and *Scholae in liberales artes.* Ramism spread rapidly through Europe in England, in Germany, etc.

4. *Aristotelians and Averroists.* Another trend of thought at this time favoured maintaining Aristotle's teaching in all its purity. This trend was strongest in Italy where it developed from an earlier peripatetic movement which had started at Salerno where, since the twelfth century, a medical group had been cultivating the doctrine of Galen. Around 1300 this movement assumed an Averroist form centered upon the theory of proof and passed over to Padua. Another direction taken by the new Peripatetism was impelled solely by the views of the Greek commentators, especially Alexander of Aphrodisias.

Among the Italian Peripatetics, mention should be made of Angelus Politianus (1454-1496), Petrus Pomponatius (1462-1524), Caesar Cremonius (1550-1631), Julius Caesar Vanini (1585-1619), Laurentius Maiolis, etc.

5. *Spanish scholasticism.* Though scholastics was broken up by the humanist movement it survived in Spain where a series of thinkers continued to be active in the field of logic. These logicians grouped themselves around Jean Majoris Scotus (1478-1540) who, as a professor in Paris, revived interest in thomist logic and laid particular stress on the *Parva Logicalia.* His pupils developed in the direction of eclecticism and this was the atmosphere breathed by a series of Spanish thinkers including Bartolome de Castro who published

Temini Logicales (1512); Rodrigo del Cueto who was the author of *Primus tractatus summularum* (1524); Ferdinand Enzinas to whom we owe *Tractatus Syllogismorum* (Paris, 1526); Pedro Ciruelo who produced *In Summulas Petri Hispani* (Salamanca, 1537), etc.

Two peaks of Aristotelian scholasticism in Spain are associated with the names of Dominicus Soto who was Thomist and Petrus Fonseca whose inclination was towards the pure doctrine.

6. *The Systematicians of the Seventeenth Century.* In the seventeenth century a new phase was entered, logic being seen to have two characters: on the one hand that of system and theoretical structure in the Aristotelian sense, on the other hand that of method and methodical procedure.

The more prominent logicians in this respect were Bartholomaeus Keckermannus whose (posthumous) work was *Systema Systematum* (Hanovra, 1613); Jean Forster who published *Systema* problematum theologicorum (Wittebergae, 1610); Clemens Timpler to whom we owe *Physicae seu philosophiae naturalis systema methodicum* (Hanovra, 1605); Adam Scherzer who wrote *Systema theologicae* (Leipzig, 1704). Finally, the most important of the "systematicians" was Jean Henricus Alstedius, the author of *Logicae systema harmonicum* (Herbonnae, 1614).

7. *The School of Raymond Lulle.* There was a remarkable man in the Middle Ages who concerned himself with logic on original lines. This was Raymond Lulle (1235-1315), a precursor of modern mathematical logic.

What Lulle proposed was a system of logic comprising a primitive "alphabet" of ideas along with a mechanical method of so combining those ideas that the manipulator would be enabled to arrive at all possible judgements. This "logic" he called the *Ars Magna.*

At the time of the Renaissance the *Ars Magna* attracted many followers. Among those who worked to improve upon Lulle mention should be made of Degui with his *Lectura super partes artis generalis* (Padova, 1453); Jacobus d'Olesia who wrote *Commentaria super artem Raymondi Lulli* (Palma, 1516); Bernhardus de Lavinheta with his *Dialectica seu niva logica Lulli* (1516); Jacob Faber Stapulensis as the editor or publisher of Lulle's works, etc. Yet others of importance at that time who took up Lulle's art were Agrippa von Nettesheim, Pedro Ciruelo, Giordano Bruno, Antonius Belverius and many more.

8. *The School of Korydaleos.* A less well known activity is that of the learned Greek commentator Theophile Korydaleos (1574-1646) who was educated at Padua under Cesare Cremonini and who after completing his studies in Italy returned to Athens where he founded a University and an Academy.

The Paduan movement aimed at a modernised interpretation of Aristotle more consonant with the scientific temper of the times. Later on, Korydaleos organised another Academy at Constantinople. The disciples of this learned Greek spread all over South-Eastern Europe, lecturing at the Academies of Constantinople and Jannina and particularly at the Princely Academies of Jassy and Bucarest which became cultural foci for the whole of the Balkan peninsula. At that time some of the greatest oriental scholars were in the countries previously forming the Eastern Roman Empire and there are very many (several hundred, Korydalean documents which bear irrefutable witness to the influence of Aristotle and the *Organon* in this area.

XIII. - METHODOLOGY. — After the Renaissance the splendid attainments of the sciences led on the one hand to the abandonment of scholastic logic and on the other to a quest for methodological principles to govern the conduct of experiments systematically. The precursors of scientific methodology were Roger Bacon (1214-1292) who was inspired by an early vision of the scientific method; Leonardo da Vinci (1452-1519) who even then formulated the Baconian canons of *concordance, differences* and *concomitant variations*; Galileo Galilei (1564-1642) who was both a scientific genius and a theoretician of scientific method.

Francis Bacon (1561-1626) criticised scholastic logic including its acceptance of Aristotle's *Organon*, he himself providing a *Novum Organum* specifically slanted in the direction of experimental research.

René Descartes (1596-1650) wrote two books on scientific research and the rules it should follow: *Regulae ad directionem ingenii* (unfinished) and *Discours de la méthode*. He dreamed of a *Mathesis Universalis* which was not to be mathematics but contained in mathematics.

The *Discours* can be reduced to two principles and four rules. The two principles require that methodical doubt be allied with clarity and distinctiveness of ideas.

The four rules are as follows: (1) accept nothing without evidence, (2) divide every problem into its constituent partial problems, (3) proceed from simple questions to those that are more complex, (4) enumerate all of the pertinent considerations, omitting none.

Cartesianism spread all over Europe and conquered the scientific world, leaving Aristotle's logic as a mere collection of dead rules.

Two treatises belonging to this period call for mention: *La logique de Port-Royal* (1662) produced by A. Arnauld and P. Nicole under the influence of Cartesian ideas and *Logica Hamburgensis* (1638) by Joachim Jungius which played a corresponding role in Germany although independently of the aforementioned.

Isaac Newton (1642-1727) propounded, in his fundamental work *Philosophiae Naturalis Principia Mathematica*, a number of rules for the conduct of scientific research. Others in England who may be cited as pursuing investigations along similar lines are John Herschel (1792-1871), William Whewell (1795-1866), Thomas Reid (1710-1796); but the most important "systematiser" of all such rules was John Stuart Mill (1806-1873) who criticised syllogism, following up a sceptical argument according to which syllogism is a *petitio principii*. In his book *Logic deductive and inductive* (1843), Mill pointed out that since syllogism proceeds from one particular to another particular its basis is induction: hence the need to organise experimental research, for which purpose he provides five tables. Francis Bacon had given three tables - of presence, of absence, of degrees - which Mill showed were really modes of reasoning and to which he added two more: the double method of concordance and difference and the method of residuals.

Herbert Spencer (1820-1903) may be cited as an opponent of syllogism who pursued a similar line of thought.

There was also a group of French thinkers concerned with induction who advanced many good and fruitful ideas. These were Jules Lachelier (1832-1918), André Lalande (1867-1963), Claude Bernard (1813-1878).

In Germany, reference must be made to the psychologist Wilhelm Wundt (1832-1920) whose monumental work *Logik* (1883) in three volumes bore the significant subtitle "An enquiry into the principles of knowledge and the methods of scientific research".

Other investigations along these lines are owed to E. Darbon who wrote *L'Explication mécanique et le nominalisme* (1911) and M. Dorolle, author of *Les problèmes de l'induction* (1926), also J. Nicod who published *Le problème logique de l'induction* (1924) etc.

We have now reached the methodology of science in our own times, regarding which it should be stated that the emergence of non-Euclidean geometries, the theory of relativity, quantum physics, etc. have completely changed the picture presented by the theory of science, which is tending to become more and more a non-Cartesian one.

What is now being sought is not so much a canon - or canons - for scientific research but rather a logic of science capable of providing a framework which can be used to display the extraordinary and sometimes contradictory results that are the outcome from experiment.

The most important modern theoreticians of science are H. Poincaré, E. Meyerson, G. Bachelard, H. Weyl, F. Gonseth, R. B. Haldane, A. N. Whitehead, C. D. Broad, Stephane Lupasco, Paulette Février, Jean-Louis Destouches, etc., in addition to whom we must of course not forget the neo-positivist school which includes Moriz Schlick, R. Carnap, F. Frank, H. Reichenbach, etc. and American thinkers such as W. Quine, N. Goodmann, etc.

In the Soviet Union this field of research is much cultivated and notable results have been obtained by V. A. Smirnov, A. A. Zinoviev, G. I. Ruzavin, D. P. Gorski, etc.

In Italy such researches have been pursued since the beginning of the century, notably by F. Enriques (*Problemi della Scienza*, 1906) who, with E. Rignano, founded "*Scientia*", the international journal of scientific synthesis.

Other Italian workers in this sector include the following: L. Geymonat, P. Filiasi Carcano, G. Pretti, whose journal is *Analysis* (Turin); S. Ceccato, F. Rossi-Landi, V. Somenzi and G. Vaccarino, who have been active in Rome and Milan, producing their journal *Methodos*; Aldo Testa, working at Bologna and connected with the journal *Il Dialogo*; R. Borsari and F. Spisani whose medium is the *Rassegna internazionale di Logica.* Finally, a new generation is constituted by A. Pasquinelli, E. Casari, E. Melandri, E. Agazzi, C. Mangione, C. Celluci. (See the admirable survey of this movement published by Prof. V. Somenzi: *Philosophy and Methodology of Science in Italy to-day*).

XIV. - PHILOSOPHY OF LOGIC. — The extraordinary results of science have had the effect of pushing logic into the background of research. After Descartes, logic became nothing more than a collection of venerable rules learned for the sake of tradition but to which no importance was attached. In the present age logic is becoming a philosophical problem of establishing its value and scope principally as a contribution to the theory of knowledge.

In that field, logic is regarded as fundamental. It was so presented in *Kritik der reinen Vernunft* by Kant (1724-1804). Kant's logic is described as "formal and transcendental" - the latter because it was concerned solely with the laws of intelligence and reasoning, the laws existed *a priori* - before there had been any experiênce - and which made experience possible.

Exponents of Kant have offered us variations of his ideas. Among these may be mentioned especially those of the Marbourg logico-methodological school such as H. Cohen, E. Cassirer, Jonas Cohn, A. Kronner, E. Lask.

German philosophers since Kant have pursued the philosophical problem which logic raises. Among them, J. G. Fichte (1762-1814) soon propounded the idea of the logical triad thesis-anthithesis-synthesis. F. W. Schelling (1775-1854) found it impossible, in view of the progress made by the experimental sciences, to agree with Kant and Fichte that thinking is independent of the object thought about: thought is determined by its object, but its object is also determined by thought.

Finally, G. W. F. Hegel (1770-1831) turned logic into a metaphysics which he called "dialectic". Logic, according to him, is an open concept which cannot be defined, a concept which generates itself through its own development and which cannot, therefore, be postulated beforehand. It can, however, be said that logic is the science of pure thought occurring among elements of pure thought. An element of pure thought is a concept, and concepts are successive forms of an idea. That is to say, they are at the same time successions in existence and successions in thought. As the precondition for knowledge of an idea is not only initial knowledge of an absolute, but also knowledge of what the idea becomes (i.e. its materialisation), it follows that the ultimate outcome from knowing an idea will be knowledge of all the phases through which the concept has passed. The "becoming" of a concept is effected by way of the triad thesis-antithesis-synthesis. Thus the becoming of the concept is the becoming of reality. Hegel's logic is set out in his *Wissenschaft der Logik* (1812-1816).

Hegel has many followers and his influence has been felt everywhere in Europe. In Italy, Hegelianism has been given a particular slant by Benedetto Croce and G. Gentile.

On the left wing it has undergone a thorough revolution by inverting Hegel's dialectic and giving it a materialist basis. This was the work of Marx (1818-1889) and Engels (1820-1895). According to Lenin (1870-1924) as argued in his *Philosophical Copy Books*, logic is not the science which deals with the external forms of thought but "the science of the laws that govern the development of all material things...... of everything concrete in the world and our knowledge thereof". So conceived, dialectic starts with a system of categories: but the system as now described is an open one (not closed as according to Kant); "becoming" proceeds by way of the dialectical contradiction through the phases of the triad thesis-antithesis-synthesis; this process of "becoming" is subject to dialectical laws whereof the most important are the lew of "unity and the conflict of contraries", the law of "passage from quantity to quality" and the law of "negation of negation".

It is accepted that the marxist *Organon* remains to be written. For a long time an interesting debate has been going on in the Soviet Union, concerning the nature and organisation of the categories. Such a systematization was proposed by V. P. Tugarinov in his book entitled in Russian *Correlation of the categories of Dialectical Materialism.* Other works dealing with the same question are those of M. Rozental, *Kategorii materialisteskij dialektik* (Moscow 1957); V. S. Bibler, *O sisteme Kategorii dialekticeskoj dialektikii* (Stalingrad 1958); K. S. Brakradze, *Sistem i metod filosofii Gegelja* (Tbilisi 1958).

At the same time and in parallel with all the foregoing a psychological conception of logic has been developed, logic being regarded as a section of psychology. According to this view the object and operations of logic are explicable by psychological factors and laws, and indeed by subjective factors.

Among those who have thought on these lines, mention should be made in the first place to the English school comprising Thomas Hobbes (1588-1679), John Locke (1632-1704), George Berkley (1685-1753), David Hume (1711-1776), Thomas Reid (1710-1796), and, in the Scottish school, Dugald Steward (1753-1828). After these we should cite William Hamilton (1788-1856), James Mill (1773-1836), John Stuart Mill (1806-1873), Herbert Spencer (1820-1903), Alexander Bain (1818-1903).

The French sensualist school began with Condillac (1715-1780) and attached itself to the English psychological movement, the same trend of ideas being shared by J. G. Cabanis (1757-1808), Destutt de Tracy (1754-1836), P. Laromiguière (1756-1837), etc.

Pragmatism appeared in the United States with William James (1842-1910); this movement was joined by noted logicians such as C. S. Peirce (1839-1914), J. Dewey (1859-1952) and the Englishman F. C. S. Schiller (1864-1937).

Further, we must not omit to mention the French anti-intellectualists such as Jules Lachelier (1834-1918), Emile Boutroux (1845-1921), Henri Bergson (1859-1941), etc.

Finally, along this line of thought, *Psychologie du Raisonnement* by Eugenio Rignano (Paris 1920) should not be forgotten.

Yet other schools see in logic a theory of values, a normative theory or an art or technique. Representatives of a few such may be mentioned here, including some logical treatises that have become well known:

— Christof Sigwart (1830-1904) whose two-volume treatise on *Logik* appeared in 1873-1878 (Tübingen).

— Wilhelm Dilthey (1833-1911): *Grundriss der Logik* (Berlin 1865).

— Herman Lotze (1817-1891) whose *Logik: drei Bücher vom Denken, von Untersuchungen, und vom Erkennen* was published in 1843 at Leipzig.

— Wilhelm Windelband (1848-1915): *Die Prinzipien der Logik* (Heidelberg, 1913).

— Wilhelm Jerusalem (1851-1936): *Der kritische Idealismus und die reine Logik* (Wien, 1905).

— Beno Erdmann (1851-1921): *Logik* (Halle, 1892).

— Heinrich Maier (1867-1938) who wrote several books on logic, the best known being *Die Syllogistik des Aristoteles* (1896-1900).

— Edmond Goblot (1858-1935) whose *Traité de Logique* (Paris 1918) is celebrated.

— André Lalande (1867-1963): *Logique normative et vérités de fait* (Paris, 1929).

The reaction to the psychological movement in logic was twofold: it led to a phenomenological conception on the one hand and to mathematical logic on the other.

It was Bernard Bolzano (1781-1848) who drew a clear distinction between psychology and logic in his four-volume *Die Wissenschaftslehre* (Sulzbach, 1837). The latter's ideas influenced Edmund Husserl (1859-1939) who saw, in the merging of psychology with logic, "one of greatest errors in philosophy". He himself conceived of logic as "a pure *a priori* science". His works on the subject are *Logische Untersuchungen* (Halle, 2 vol. 1900); *Formale und transzendentale Logik* (Halle, 1929), etc.

XV. - MATHEMATICAL LOGIC. — This brings us to modern logic which has assumed a mathematical form and appears to be not only a reaction against the psychological approach of the last century but also a consequence of the success achieved by mathematics in the same century, a success which has made the mathematical sciences the ideal of all others, including logic.

The following is a translated definition of mathematical logic as given by D. Hilbert and W. Ackermann: "Theoretical logic, also called mathematical or symbolic logic, is an extension into the logical field of the formal methods proper to mathematics".

Mathematical logic has two main characteristics: its appearance as an algebraic calculus and its purely formal aspect. Considered as an algebraic calculus its presentation is that of an "axiomatic system" wherein the initial ideas and the axioms are chosen *ad placitum* and which enables new formulae to be constructed by obedience to a few rules of "composition" and "deduction". The "formalist" character of this logic is due to its use of signs which have no meaningful content at all, the only rules to which the signs are subject being those that regulate their use when they enter into combination with others.

The conception of logic as a calculus is an old one and ideas similar to these are found even in antiquity, but the first logician to approach anything of the kind was Raymond Lulle who has already been mentioned here. Hobbes and Condillac had the idea of a "logical calculus" but did not carry it far enough for them to arrive at anything worth mentioning. The first real precursor of mathematical logic, therefore, was Leibniz (1646-1716) who, even if he did not actually realise a "system", had many ideas now rediscovered in logic. Ideas which approach those of Leibniz very closely are to be found also in other treatises published at or after his time, such as the following:

Gerolamo Saccheri (1666-1733) published *Logica Demonstrativa* (1697), a work which lay forgotten for many years but which G. Vailati recalled to the memory of logicians in his study entitled *Di una opera dimenticata del P. Gerolamo Saccheri* (G. Vailati, Scritti, 1911).

The influence of Leibniz on J. H. Lambert can be seen in that author's *Neuer Organon* (1771) and his influence on J. A. Segner in the latter's *Specimen Logicae universaliter demonstrata* (1740).

S. Ploucquet wrote his *Fundamenta Philosophiae speculativae* (1759) in the same spirit.

During the same period many mathematicians were using geometrical figures to represent logical operations. Euler (1707-1783) denoted different types of syllogisms by circles. The mathematician J. D. Gergonne (1771-1859), starting from Euler's results, again took up the geometrical study of syllogisms. Similar studies of syllogisms were undertaken by John Venn (1834-1923) and C. L. Dodgson (1832-1898).

Salomon Maimon (1754-1800), a philosopher influenced by Kant, published a book *Versuch einer neuen Logik, der Theorie des Denkens* in which he endeavoured to construct a logical algebra.

In the context of the problem of judgements and their quantity, mention must be made of W. Hamilton (1788-1886) who propounded a theory of "quantification of the predicate".

We now arrive at the attempt to construct an algebraic logic set out by George Boole (1815-1864) in his works *The mathematical analysis of logic* (1847) and *An investigation of the laws of thought* (1854). Other "algebraists" of logic have been:

— W. Stanley Jevons (1835-1882): *Pure Logic or the logic of quality apart from quantity* (1864) and *The principles of science* (1874).

— Augustus de Morgan (1806-1871): *Formal Logic of the calculus of inference, necessary and probable* (1847).

— Hugh McColl (1837-1909): *The calculus of equivalent statements and integration limits* (1877), *Symbolic Logic and its application* (1906).

And, in another group:

— R. Grassmann (1815-1901): *Der Begriffslehre oder Logik* (1872); *Die Logik und die anderen logischen Wissenschaften* (1890).

— C. S. Peirce (1839-1914): *On an improvement in Boole's Calculus of Logic* (1867); *Grounds and validity of the laws of logic* (1868); *On the algebra of logic* (1880); *The regenerated logic* (1896).

— Ernest Schröder (1851-1902), author of very important works on algebraic logic: *Der Operationskreis des Logikkalkuls* (1877); *Ueber das Zeichen* (1890); *Vorlesungen uber Algebra der Logik* (3 vol. 1890-1895).

Finally, we come to Gottlob Frege (1848-1925) at the culminating point of mathematical logic even though this was to develop in other hands. His own chief works were:

— *Begriffschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* (1879, Halle); *Die Grundlagen der Arithmetik* (1884, Breslau); *Funktion und Begriff* (1891, Jena); *Ueber Sinn und Bedeutung* (1892); *Grundgesetze der Aritmetik* (2 vol. 1893-1903, Jena).

Frege's fundamental idea was that mathematics is a branch of logic. To think this out he needed a logico-mathematical instrument as precise as mathematics itself.

Mathematical logic owes to him the propositional calculus which he reduced completely to axioms, also the discovery of the logic of predicates and, above all, that of the logical function which is its central concept.

According to Frege the propositional calculus is based upon two primitive ideas - those of negation and implication - and upon propositional variables which can be either true or false, six axioms (initially he assumed nine axioms), two rules of deduction, substitution and the *modus ponens.* With these symbolic ideas he obtained theorems which were already known to the Stoics and Scholastics. He then proceeded to bring in the idea of the functional proposition with some rather hesitating and complicated explanations. In this way the judgement of predication is replaced by the logical function, "x has the predicate f" being symbolised by f(x). Frege's symbolism is complicated and even inadmissible.

At the same time a logico-mathematical movement appeared in Italy, due to Giuseppe Peano (1858-1932) and his school. Peano wrote *Arithmetica Principia novo methodo exposita* (Torino, 1889); *Notations de logique mathématique* (Torino, 1894); *Studi di logica matematica* (Torino, 1897); *Formulario matematico* (1897, afterwards 1905-1908). Although nowadays less weight is given to Peano's researches they played a decisive role in the later development of mathematical logic and in elucidating the foundations of arithmetic (as Russell himself acknowledged). Peano, like Frege, wanted to provide a basis for the theory of whole numbers; with this aim in view he needed and he constructed a more adequate logic. He looked for a way of juxtaposing logic with mathematics.

Peano looked upon mathematical logic as a study of logical laws assisted by the formulae and methods of the mathematical sciences. In his writings the axiomatic structure of logic is very clear. His symbolism is very simple and traces of it are to be found throughout the later development of the science.

Peano's school includes well known logicians and mathematicians among whom the following may be cited:

— Cesare Burali-Forti (1853-1951) who wrote: *Logica Matematica* (Milano, 1893); *Una questione sui numeri transfiniti* (Palermo 1897); *Sur quelques propriétés des ensembles d'ensembles* (1895), etc.

— Giovanni Vailati (1863-1909); *Le proprietà fondamentali delle operazioni della logica* (1891); *Dependenza fra le proprietà delle relazioni* (1891); *Il metodo deduttivo come Istrumento di ricerca* (1897); *La logique mathématique dans sa dernière phase de développement* (1899) etc.

— Alessandro Padoa (1888-1937): *La logique deductive dans sa dernière phase de développement* (1922); *Il contributo di Peano all'ideografia logica* (1933); *Ce que la logique doit à Peano* (1936).

— Giovanni Vacca (1872-1953); *Sui precursori della logica matematica* (1896); *Logica Matematica* (1929).

The following are other Italian logicians who, although they cannot be called "followers" of Peano, have dealt with the logical porblems he posed:

— Annibale Pastore (b.1868), the most important logician since Peano, who wrote: *Sopra la teoria della Scienza* (1903); *La logica del potenziamento* (1936); *Logica sperimentale* (1939); *Logicalia* (published posthumously in 1957).

— Federigo Enriques, from whom we have *Problemi della Scienza* (1906); *L'évolution de la logique* (1926); *La signification de l'histoire de la pensée scientifique* (1935), etc.

— Ludovico Geymonat, a professor at the University of Milan, teaches a neo-rationalism very close to logical empiricism. His works include: *Le idee direttive del neo-empirismo* (1939); *Analisi critica del recente indirizzo di logica formale di Carnap* (1940-42); *Studi per un nuovo razionalismo* (1945) etc.

We may further cite:

Franco Spisani: *Logica Produttiva*; A. Pasquinelli: *Introduzione alla logica Simbolica*; E. Casari: *Lineamenti di logica matematica*; M. Boldrini: *Teoria della statistica*; E. Persico: *Fondamenti logici della Scienza*; Ev. Agazzi: *La logica simbolica.*

Also, the philosophy of cybernetics is enriched by many new ideas from the thinking of V. Somenzi, as in the volume produced under his direction entitled *La filosofia degli autonomi* (Torino, 1965).

Among logicians of our own time, special mention should be made of Francesca Rivetti Barbó, a professor at Perugia whose works include *L'antinomia del Mentitore nel pensiero contemporaneo* (1961); *Il senso e il significato di Frege* (1963); *Le antinomie concettuali ed il paradosso di Russell* (1957), etc.

Finally, after Frege and Peano we come to the monumental joint work by B. Russell (1872-1970) and A. N. Whitehead, *Principia Mathematica* (3 vol. 1910-1913), in which mathematical logic is built up as a complete whole. The first volume of this deals with the problems involved as follows:

1. The theory of deduction, which is the propositional calculus.

2. The theory of apparent variables, which is the calculus of propositional functions and which also includes logico-mathematical paradoxes along with the theory of types (simple and ramified).
3. Classes and relations.

The logical calculus is completely axiomatised. For the calculus of propositions, Russell accepts two primary ideas (negation and disjunction) and five axioms (later reduced to four). For the rules of deduction, the *Principia* allow the rule of substitution and that of the *modus ponens*, thereby obtaining the "true formulae" which Wittgenstein has called *tautologies*.

Russell had same idea as Frege: that of constituting logic entirely as a mathematical science and of then deriving mathematics from logic. This attempt, however, ran up against great difficulties from the start, arising particularly from the appearance of logico-mathematical paradoxes. These called for the introduction of a new theory, known as the theory of types, and for the principle of reducibility.

The emergence of paradoxes opened up many important lines of research and gaze rise to many investigations. Thus mathematical logic branched in three directions corresponding to three different philosophical conceptions:

1. *Logicism*, the Frege-Russell theory according to which mathematics are a branch of logic, having for its chief proponents Chwistek (b.1884); Ludwig Wittgenstein (1889-1951), author of the famous *Tractatus logico-philosophicus*; J. Nicod, P. Ramsey, R. Carnap, etc.

2. *Formalism*, which seeks to establish the foundations of mathematics with the aid of symbolic logic "because the image of logical thought is reflected in an algebraic calculus"; hence logic and mathematics should both be constructed at the same time, the two sciences merging into one another. This approach is due to David Hilbert (1862-1943) whose chief work was *Axiomatisches Denken* (1928) along with W. Aeckermann: *Grundzüge der theoretischen Logik* (Berlin, 1928); *P.* Bernays, *Grundlagen der Mathematik* (Berlin, 2 vol. 1934-1938), etc. Associated with the same school are W. Ackermann, Paul Bernays, J. von Neumann, etc.

3. *Intuitionism* is due to L. Brouwer and A. Heyting in the Netherlands, according to whom mathematical activity is primary to all thinking and whatever is exactly known has a mathematical character. Therefore to found mathematics on logic, or on any other science, would be a vicious circle. Supporters of this idea allow a weakening of the principle of the excluded middle which, they say, applies only in finite fields. Intuitional logic has been formalised by Heyting.

Later development of mathematical logic has been in the direction of polyvalent and modal logic. If the primary ideas and axioms of logic can be suitably chosen it should be possible to imagine a proposition admitting on two alternative values (the true and the false). Systems of logic have in fact been constructed which allow for propositions having *n* values of truthfulness. *Polyvalent logics*, as these are called, include the systems devised by Lukasiewicz and by Botchvar, the modal logic of Moisil, etc.

Formal systems of logic have been studied by applying them to themselves, thus achieving what may truly be called a formal technique. The formal systems are constructions made up of conventional symbols which are void of any interpretation. Two phases of their construction are distinguished:

First, a study of how the formal system as such is constructed, of the conditions which the constructed formulae must satisfy in order to be correct, and of their deductive possibilities. This is called the *logical syntax* of the system. That expression was first used by Wittgenstein, but the theory underlying the syntax of a system was set out by R. Carnap in his book *Logische Syntax der Sprache* (Wien, 1934).

Secondly, a study of the system conjoined with ideas of truth and falsehood, called *semantics*. The links between semantics and the interpretations of a formal system are obvious. The theory of semantics was worked out by A. Tarski who introduced this term in his *Grundlagen der Wissenschaftlichen Semantik* (1936) but who developed semantics itself in *Der Wahrheitsbegriff in den formalisierten Sprachen* (Leopoli 1936).

The biggest defect of formal systems is the existence within them of "indeterminable" formulae whose relevance to a system can neither be proved nor disproved. This is the paradox that perplexed Kurt Gödel who dealt with it in his *Ueber formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme* (1931).

At the present time there are three recognised constructional modes in mathematical logic:

1. Logic as a formal axiomatic system (Russell, Hilbert, etc.).
2. Logics with deductive schemes.
3. Combinatory logics.

Schematic systems of logic do not use axioms but only schemes showing derivation. In this kind of formalism, logic is no longer a theory of propositions but a theory of demonstration. Schematic logics were constructed for the first time by G. Gentzen in his work *Untersuchungen über das logische Schliessen* (1934) and by S. Jaskowski *On the rules of suppositions in formal logic* (1934).

Combinatory logics conceive intuitive logical operations as "pure" operations and try to find an equivalent formula for them. Pure formalisms for combinatory logic have been constructed by Haskel B. Curry in *Grundlagen der Kombinatorischen Logik* (1930), *Combinatory Logic* (1958), etc.

Another direction in which combinatory logic has been developed is what is known as calculations of λ-conversion, started by Alonzo Church, S. C. Kleene, J. B. Rosser, etc.

To sum up, mathematical logic which set out with the object of consolidating the foundations of mathematics has reached the point of being regarded as a "well finished" language, complete with syntactical and semantic rules. It is the logic of scientific language in the most general sense, but it runs up against so many difficulties that one may well ask whether it does gain more weight than it sheds.

It will be clear to anyone who has carefully followed the above explanation that mathematical logic does not cover the whole ground of thinking. This is why many voices have latterly been raised calling for greater comprehensiveness of logic throughout its range, not regarding it solely as a "scientific language". Among these voices of reaction the following must be mentioned:

— I. Piaget, in his books, *Classes, Relations, Nombres* (1942) and *Traité de Logique* (1949), etc.

— René Poirier: *Le Nombre* (1938), *Logique et Modalité* (1952) etc.

— Robert Blanché: *Raison et Discours* (Paris 1965) and *Structures Intellectuelles* (Paris 2nd ed.1969), etc.

— Leo Gabriel: *Integrale Logik* (Wien, 1965), etc.

— Petre Botezatu: "*Esquisse d'une logique naturelle*" (1969), Bucarest (in Roumanian).

— Anton Dumitriu: *Logique formelle et logique formalisée* (Bucarest 1966) and *Theory and System* (Bologna 1970), etc.

XVI. - Conclusion. — The stages in the evolution of logic throughout its history have now been recounted.

At each stage, as we have seen, a particular aspect of logic has been developed. To say that any one aspect alone is valid would be a blind submission to prejudice, for the history of logic has taught us that each of its modes - each *Gestalt* as Bochenski calls it - is of value in its own way. Logic, so regarded, is its own history; it is the history of the *logos* in process of taking shape and expanding over the world. The *logos* has an undefined number of modes but is itself the integral summation of them all. Even where the modes sometimes appear to contradict one another they need to be understood complementarily.

From a very general point of view, logic can be divided into two great chapters:

1. Logic as an analysis of the mechanism of thought, and

2. Logic as an analysis of the mechanism of the expression in which thought is embodied.

Its exposition here will have served to display the continued oscillation of logical investigations between logico-grammatical constructions and the structure of the mechanism whereby thought occurs. However, these two directions of the investigations are reciprocal and complementary: the mechanism of thought and the form of that mechanism are interconnected, they are two facets of the same thing. That is why, with the scholastics, the *logos* consisted of *sermo et ratio*, language plus reason.

Thus the place occupied by logic among the other sciences is seen to be one of pre-eminence. Logic being the rational infrastructure of all the other sciences cannot be identical with any particular science. As we have seen in this survey, neither Aristotle nor the scholastic logicians conceived of logic as being a science like any other: they regarded it as the science of the sciences, *scientia scientiarum, ars artium, doctrina doctrinarum*. They called it by these names not because of its importance but in the first place because its character was seen to be such as to constitute a principle in relation to the other sciences. Just as every science needs principles for its own internal constitution, so the sciences need a science whereby they as a whole can theoretically be constituted. Thus the role of logic in the context of the sciences as a whole is seen to be altogether special.

This peculiarity becomes obvious when we consider what is the purpose of logic. Each science presupposes a field of data (whether concrete or abstract) which are objects of thought, but in the logical field the object of thought is thought itself. Thought, as regards its function of scrutinising thought itself

- the function which is logic - is thus revealed as the most extraordinary occurrence in the world: it not only thinks out everything, but by a process of turning back upon itself it proceeds to think out what the thing is that does the thinking. Such is the highest point of all in the scale of values, and Aristotle had good reason to declare that the purest thought is the purest good. This declaration was the highest title of honour ever bestowed upon thought, and for Aristotle it meant that thought has a character which is "divine".

Thus thought in its highest dignity is "thought about thought".

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A. DUMITRIU

CRONACHE - REPORTS

L'uomo e la biosfera

Una indagine scientifica, promossa dall'Unesco e condotta da studiosi di tutti i paesi del mondo, tenterà di stabilire quali conseguenze abbia avuto l'azione dell'uomo sulla biosfera, e quali ulteriori modificazioni siano da prevedersi in seguito all'evolversi della civiltà tecnologica. Le ricerche verranno condotte in tutte le regioni della Terra, dalle tundre artiche alle foreste tropicali, dalle isole disabitate degli oceani alle megalopoli che in qualche modo prefigurano la società del futuro.

L'attuazione del progetto, denominato « L'uomo e la biosfera », è stata decisa nel corso di una riunione svoltasi recentemente a Parigi presso la sede dell'Unesco; erano presenti settantadue delegati dei venticinque paesi che fanno parte del consiglio di coordinamento del programma, oltre a rappresentanti delle Nazioni Unite, del Consiglio Internazionale delle Unioni scientifiche e della Unione Internazionale per la conservazione delle risorse naturali. Gli scopi dell'indagine sono stati così definiti: precisare, con gli strumenti offerti dalle scienze naturali e sociali, in qual modo si possano utilizzare razionalmente e conservare le risorse della biosfera e in qual modo possa essere migliorato il rapporto globale fra l'uomo e l'ambiente; prevedere le conseguenze dell'azione umana sul mondo di domani. Ci si propone, si afferma, di sviluppare « l'idea della realizzazione piena dell'uomo in associazione con la natura e l'idea della responsabilità dell'uomo nei confronti della natura ».

Il metodo scientifico del programma si fonda essenzialmente sullo studio degli ecosistemi. Come è stato ricordato nel corso della riunione, « la struttura fondamentale di un ecosistema è quella di una macchina capace di intercettare l'energia irradiata dal sole, di convertirla in energia chimica attraverso la fotosintesi e di distribuire l'energia chimica in modo da assicurare il mantenimento della sua struttura funzionale. Le piante verdi sono strumenti della fotosintesi, gli erbivori ed i predatori contribuiscono alla distribuzione di energia e di materia, e gli agenti di decomposizione permettono la distruzione delle sostanze organiche morte, rendendo in tal modo di nuovo disponibili per le piante gli elementi minerali contenuti nelle sostanze organiche ». Ed il ciclo prosegue indefinitamente, finchè non venga alterato dall'intervento dell'uomo, che ha profittato della adattabilità degli ecosistemi per modificarli a proprio vantaggio, non senza, però, ripetere l'esperienza dell'apprendista stregone: esiste infatti un limite di tolleranza degli ecosistemi all'intervento umano.

Per esaminare il complesso dei rapporti fra l'uomo e la biosfera sono stati decisi tredici programmi di ricerca, distinti ma complementari, concepiti in modo da conservare una certa flessibilità tanto nei contenuti quanto nei metodi. Restano estranei al vasto programma di indagini i problemi pratici connessi alla lotta contro gli inquinamenti o allo sviluppo dei grandi complessi urbani: si è infatti ritenuto che essi cadessero più direttamente nella sfera di competenza di altri organismi specializzati delle Nazioni Unite. Ci si studierà tuttavia di stabilire in qual modo gli inquinamenti alterino il funzionamento degli ecosistemi.

Il primo di questi progetti è inteso a studiare gli effetti che le attività umane in continuo sviluppo producono sugli ecosistemi rappresentati dalle foreste tropicali e subtropicali. In queste regioni le terre sono utilizzate in modo irrazionale: l'antico metodo della rotazione delle coltivazioni, che permetteva una continua reintegrazione della fertilità del terreno, ha ceduto il posto a coltivazioni permanenti che impoveriscono il suolo e lo rendono compatto. Il solo fatto che l'uomo penetri in certe zone è causa della decadenza delle foreste: nel corso della riunione è stato ricordato che in Brasile si vedono fasce semisterili estendersi per gradi, ai margini delle autostrade costruite di recente attraverso l'Amazzonia.

Altro tema di ricerca sarà l'effetto delle attività umane, e in particolar modo dell'utilizzazione agricola della terra, sui pascoli naturali. Tra gli scopi che questo studio si propone, è l'esame comparato - da condursi principalmente nelle savane - degli animali domestici e selvatici, al fine di trovare i migliori produttori di proteine. Allo stesso modo saranno oggetto di esame gli effetti che lo sfruttamento dei giacimenti petroliferi e l'impianto di nuove vie di comunicazione producono sulle tundre.

Per quanto riguarda le zone aride e semi-aride, è stato deciso di condurre uno studio sull'influenza che l'opera dell'uomo, e in particolare l'irrigazione, esercitano sugli ecosistemi che vi hanno sede. Secondo una stima ritenuta attendibile, prima della fine di questo secolo la superficie delle terre irrigue nel mondo potrà raggiungere i trecento milioni di ettari; e tuttavia già si lamenta la perdita di milioni di ettari, a causa di una irrigazione mal concepita o mal praticata che porta a fenomeni di dispersione dell'acqua, di salinizzazione e quindi di sterilizzazione del terreno. Ci si propone, quindi, di studiare il modo di utilizzare con il massimo profitto le limitate quantità d'acqua destinate all'irrigazione, evitando d'altro canto di produrre danni irreparabili al suolo.

L'acqua è del pari al centro del programma di studio sugli effetti delle attività umane sui laghi, le paludi, i corsi d'acqua, i delta, gli estuari e le zone costiere. La costruzione di grandi dighe modifica il regime dei fiumi e determina una diminuzione dell'apporto di quegli elementi organici alla cui presenza i delta devono la loro fertilità. Allo stesso modo, l'apporto di nitrati e di fosfati provenienti dalle terre fertilizzate chimicamente e dagli scarichi urbani provocano l'eutrofizzazione, ossia quel particolare tipo di inquinamento delle acque dei laghi che ha come conseguenza la comparsa di specie vegetali prive di interesse e la contemporanea scomparsa dei pesci.

La continua avanzata della civiltà giustifica altri due progetti, che si propongono di prendere in esame le isole oceaniche e le regioni montuose che - essendo rimaste finora isolate ed ospitando comunità biologiche particolarmente vulnerabili dall'azione dell'uomo - debbono essere tutelate per quanto è possibile.

Un altro progetto prevede la creazione di una rete mondiale di zone protette. È stata posta in particolare rilievo, a questo riguardo, la necessità che tali riserve non rappresentino soltanto degli indici attraverso i quali studiare i mutamenti ecologici: esse, al contrario, dovranno consentire innanzitutto la conservazione del patrimonio genetico delle specie vegetali ed animali e dei microrganismi altrimenti votato alla distruzione.

Saranno anche studiate, nel corso della vasta indagine, le conseguenze che un uso non sempre avveduto degli insetticidi e dei fertilizzanti chimici va producendo sull'ambiente naturale. È noto che, a questo riguardo, si pone un duplice ordine di problemi: da un lato gli effetti talvolta imprevedibili dell'impiego di insetticidi e di altri preparati chimici sulla flora e la fauna; dall'altro la decrescente efficacia dei preparati stessi. Si dovranno, quindi, valutarne i vantaggi e gli svantaggi dei metodi fin qui usati.

Oggetto di ricerca sarà anche l'influenza esercitata sull'ambiente naturale dalle grandi opere - dighe, autostrade, impianti di varia natura - che si vanno moltiplicando nel mondo. Saranno presi in esame alcuni specifici casi, così da stabilire quali mutamenti abbiano portato e in qual modo si possano evitare gli eventuali danni.

Particolare rilievo avrà, nel quadro del programma, lo studio degli aspetti ecologici dell'utilizzazione dell'energia nei sistemi urbani ed industriali. Il documento conclusivo approvato dall'Unesco afferma in proposito: « Nell'affrontare il problema si deve tener conto del fatto che le interazioni fra l'uomo e il resto della biosfera sono proporzionali al numero degli uomini e alla quantità di energia consumata da ciascun individuo. L'esperienza mostra che tali interazioni sono troppo spesso deleterie. Sembra dunque opportuno promuovere lo studio del fabbisogno di energia per ciascun individuo in differenti agglomerati urbani; l'obiettivo principale di questi studi sarà la ricerca di particolari tecniche che consentano di associare un elevato tenore di vita ad una utilizzazione più razionale dell'energia ».

Fin qui il progetto, per ogni verso lodevole, che l'Unesco si accinge ad attuare. Esso, però, sarà poco più che un tributo pagato alla moda che ha fatto dell'ecologia una scienza mondana, se a guidare gli studi annunciati non sarà la consapevolezza - che è, in qualche modo, anche politica - della parte determinante che determinate strutture sociali ed economiche e, soprattutto, la logica del profitto hanno avuto nella devastazione e nella degradazione dell'ambiente naturale. Se restasse priva di tale consapevolezza e se si limitasse ad indicare i meccanismi che hanno prodotto i guasti, senza identificare le circostanze e le forze che li hanno messi in moto, l'iniziativa promossa dall'Unesco sarebbe - come le molte altre che in questi anni l'hanno preceduta - un tentativo velleitario compiuto per eludere più precisi impegni, o, se si preferisce una fertile occasione di esercitazioni suggestive quanto si voglia, ma improduttive e vane.

Man and the Biosphere

A scientific study, promoted by Unesco and carried out by experts from all parts of the world, is to attempt to establish what have been the consequences of man's action on the biosphere and what further modifications are to be expected as a result of the evolution of technological civilisation. Research is to be carried out in every region of the earth, from the Arctic tundra to the tropical forests, from the desert islands in the oceans to the megalopolises that in some ways foreshadow the society of the future.

The project, entitled « Man and the biosphere », is to be put into effect as the result of a decision taken at a meeting held recently in Paris at the Unesco headquarters; the meeting was attended by delegates of the twenty-five countries that form part of the committee for the co-ordination of the programme, in addition to representatives of the United Nations, the International Council of Scientific Unions and the International Union for the Conservation of Natural Resources. The aims of the investigation were defined as follows: to determine precisely, by means of the instruments offered by the natural and social sciences, how the resources of the biosphere can be used rationally and conserved and how the overall relationship between man and his environment can be improved; to predict the consequences of man's action on the world of tomorrow. The proposal - it is stated - is to develop « the idea of the full realisation of man in association with nature and the idea of man's responsibility towards nature ».

The scientific method to be followed in the programme is essentially based on a study of the ecosystems. As was mentioned in the course of the meeting, « the fundamental structure of an ecosystem is that of a machine capable of intercepting the energy radiated by the sun, converting it into chemical energy through photosynthesis and distributing this chemical energy in such a way as to ensure the maintenance of its structural function. Green plants are instruments of photosynthesis, herbivorous and predatory animals contribute to the

distribution of energy and matter and the agents of decomposition enable the dead organic substances to be destroyed, thus making the minerals contained in the organic substances available again for the plants». And the cycle continues indefinitely, until it is altered by the intervention of man, who has profited by the adaptability of the ecosystems to modify them to his own advantage, though not without repeating the experience of the sorcerer's apprentice: in fact, there is a limit to the tolerance of ecosystems to human intervention.

In order to study the relationships between man and the biosphere, as a whole, thirteen research programmes have been arranged; the programmes are distinct but complementary and are conceived in such a way as to preserve a certain flexibility both in content and in methods. The vast programme of studies does not include the practical problems connected with the fight against pollution or the development of the great urban complexes: in fact, it was considered that they came more directly into the sphere of authority of other specialised organisms in the United Nations. However, studies will attempt to establish how pollution alters the functioning of ecosystems. The object of the first of these projects is to study the effects that human activities in continuous development have on the ecosystems represented by the tropical and subtropical forests. In these regions, the ground is utilised irrationally: the ancient method of the rotation of crops, which allowed a continuous reintegration of the fertility of the soil, has given way to permanent cultivations that impoverish the soil and make it compact. The mere fact that man penetrates into certain areas is a cause of the decadence of the forests: in the course of the meeting it was mentioned that, in Brazil, semi-sterile belts are seen to be spreading by degrees at the edges of the motorways recently built through the Amazon basin.

Another subject of research will be the effect of human activities - particularly the use of the ground for agriculture - on the natural pastures. One of the aims of this research is the comparative study - to be carried out mainly in the savannahs - of the domestic and wild animals, in order to discover the best producers of proteins. In the same way, a study will be made of the effects produced on the tundras by the exploitation of oil deposits and the establishment of new ways of communication.

As regards the dry and semi-dry areas, it was decided to make a study of the influence that the works of man - and particularly irrigation - exert on the ecosystems established there. According to an opinion that can be considered reliable, before the end of this century the area of all the irrigated land in the world may reach about 750 million acres. However, there is already cause to lament the loss of millions of acres as a result of irrigation that has been either badly conceived or badly carried out, leading to phenomena of loss of water, salination and the consequent sterilisation of the ground. It is therefore proposed to study the way in which the limited quantities of water intended for irrigation may be used as profitably as possible, but preventing irreparable damage being caused to the soil.

Likewise, water forms the hub of the programme of studies on the effects of human activities on lakes, marshes, watercourses, deltas, estuaries and coastal areas. The construction of great dams modifies the behaviour of rivers and causes a reduction in the contribution of those organic elements responsible for the fertility of the deltas. In the same way, the accumulation of nitrates and phosphates resulting from chemically fertilised areas and from town waste cause eutrophisation, or that particular type of pollution of the waters of lakes that results in the appearance of vegetable species of no interest and the simultaneous disappearance of the fish.

The continuous advance of civilisation justifies two other projects in the programme: the study of the mid-ocean islands and of the mountain regions, which must be protected as far as possible, since they have so far remained isolated and contain biological communities that are particularly vulnerable to the action of man.

The aim of another project is to create a world network of protected areas. In this respect, particular emphasis has been placed on the necessity that these reserves shall not represent merely a means for studying ecological changes; on the contrary, their primary purpose must be to preserve the genetic heritage of the vegetable and animal species and of the micro-organisms, otherwise fated to be destroyed.

In the course of the vast investigation, a study will also be made of the consequences on the natural environment that are now resulting from the not always judicious use of insecticides and chemical fertilisers. In this respect, a two-fold order of problems arises: on the one hand, the effects - at times unpredictable - of the use of insecticides and other chemicals on the flora and fauna; on the other hand, the decreasing efficacy of these chemicals. There will therefore have to be an assessment of the advantages and disadvantages of the methods used op to now.

Another subject of study will be the influence exerted on the natural environment by the great works - dams, motorways, or other constructions - that are becoming more and more numerous throughout the world. Specific cases will be studied, in order to establish what changes they have caused and in what way possible harm can be avoided.

In the programme, particular importance will be attached to a study of the ecological aspects of the utilisation of energy in urban and industrial systems. On this subject, the conclusive document approved by Unesco states: « In approaching the problem, account must be taken of the fact that the interactions between man and the rest of the biosphere are proportional to the number of men and the amount of energy consumed by each individual. Experience shows that these interactions are too often deleterious. It therefore seems opportune to promote a study of the energy requirement for each individual in different built-up urban areas; the main objective of these studies will be to investigate particular techniques that may enable a high standard of living to be combined with a more rational use of energy ».

This, as it stands, is the project - laudable from every aspect - that Unesco is preparing to put into effect. However, it will be little more than a tribute paid to the « fashion » that a wordly science has made of ecology if the proposed studies are not guided by an awareness - in a certain sense, also political - of the determining role that particular social and economic structures, and above all the logic of profit, have played in the devastation and degradation of the natural environment. Without that awareness, and if the study were limited to an indication of the mechanisms that have caused the damage without identifying the circumstances and forces that have set them in motion, the enterprise promoted by Unesco would be - like the many others that have preceded it in recent years - a weak-willed attempt made to elude more precise commitments, or, if one prefers, a fertile occasion for exercises, however impressive, but vain and unproductive.

In crisi gli studi di fisica

Se si deve credere a giudizi non privi di autorevolezza, gli studi di fisica nel mondo, e in primo luogo nei centri di ricerca tedeschi e francesi, attraversano una grave crisi. Quali ne sono le cause e le manifestazioni? Da più parti si lamentano l'esaurirsi dello slancio della ricerca, la mancanza di entusiasmo delle giovani leve studentesche delle grandi università europee ed americane, i risultati modesti o irrilevanti che da molti anni vengono ottenuti. K. Kudzinski ha scritto recentemente sulla « Frankfurter Allgemeine » che le ricerche sulla materia da lungo tempo non hanno segnato sostanziali progressi. In pratica, egli precisa, da quando Werner Heisenberg, più di dieci anni or sono, elaborò la sua formula, non sono stati compiuti apprezzabili passi avanti. Se è vero che, con il sussidio dei superacceleratori, si sono scoperte nuove particelle elementari, è anche vero che i rapporti che le legano sono ancora in larga parte sconosciuti e che in proposito non si è formulata alcuna teoria soddisfacente.

L'attuale stato di cose, aggiunge Kudzinski, sembra confermare il giudizio del professor Melvin Schwartz, fisico dell'università di Stanford in California e membro del presidium del Weizmann Institut di Israele, il quale attribuisce la causa dell'attuale situazione all'esaurirsi dello slancio dei giovani fisici. All'origine di questo stato di frustazione sarebbe la « struttura sociologica » della fisica delle alte energie. Ostacoli di natura economica non facilmente superabili (costo dei grandi acceleratori, spese per il loro mantenimento, durata degli esperimenti, spesso superiore alle mille ore) limitano le ricerche, il cui numero non supera la ventina per ogni anno e che, naturalmente, sono ristrette ai gruppi già costituiti. I giovani, in tal modo, restano esclusi dai ruoli più attivi della ricerca, alla quale non hanno modo di dare l'apporto delle loro energie, della loro spregiudicatezza, della loro capacità creativa.

Per porre rimedio a questo stato di cose il professor Schwartz propone di assicurare ai più promettenti fra i giovani fisici finanziamenti modesti, ma sufficienti a consentir loro di condurre ricerche autonome e di verificare sperimentalmente la validità delle loro teorie; di sciogliere i gruppi di ricerca che hanno già assolto ai loro compiti, per costituirne di nuovi, con nuovi programmi: tanto dovrebbe valere non soltanto per la fisica delle alte energie, ma anche per tutti gli altri campi della ricerca scientifica.

La situazione degli studi di fisica non sembra essere sostanzialmente diversa in Francia, dove le forze paralizzanti che agiscono sulla ricerca sono state identificate da osservatori autorevoli nella indifferenza generale che circonda gli studiosi e, peggio ancora, nell'ostilità dei poteri pubblici. A ciò si aggiunge il ritardo nel rinnovamento dell'industria francese e la totale mancanza di entusiasmo che i detentori del potere accademico riscontrano nelle giovani leve studentesche (non si potrà, però, non aggiungere, a questo riguardo, che non si vede per qual motivo i giovani dovrebbero percorrere con entusiasmo una strada che vedono sbarrata da rigide strutture di potere e di privilegio). Queste amare osservazioni sono state fatte anche recentemente nel corso del « Colloquio di Evian » promosso dalla Société Française de Physique. Uno dei partecipanti al Colloquio, il professor Friedel, ha addirittura affermato che la ricerca fondamentale si va avviando, in Francia, ad una regressione catastrofica, sebbene negli ultimi quindici anni fossero stati ottenuti risultati che, già di per sè, costituivano un successo di portata internazionale.

Aspre critiche sono anche state rivolte, ad Evian, al nuovo Piano francese ed ai criteri che lo ispirano, nè si è mancato di ricordare che il Piano precedente fu realizzato soltanto per il sessanta per cento. A determinare le proteste dei fisici francesi è fra l'altro il fatto che il governo sembra deciso a dedicare alla ricerca fondamentale stanziamenti pari alla stessa quota del reddito lordo nazionale che fu adottata nel 1965. In tal modo, è stato osservato, nel 1975 la Francia avrà ottenuto il risultato, senza dubbio lusinghiero, di essere tornata indietro di dieci anni. Sta di fatto, comunque, che tra gli studiosi e il potere pubblico si è creato un rapporto di reciproca sfiducia che non si può supporre giovevole ai fini della ricerca.

Altrettanto preoccupante è il disinteresse dei giovani per gli studi di fisica. Il piano di « orientamento scolastico » al quale il governo francese affidava molte speranze, sembra essere totalmente fallito: non può che essere inteso come l'indizio di un fallimento il fatto che quest'anno, a Grenoble, sono rimasti scoperti i due terzi dei posti disponibili dei laboratori di fisica per il terzo ciclo universitario. Dal canto suo il ministero dell'educazione si accinge ad aggravare questo stato di cose con una riforma delle scuole secondarie che, se da un lato favorisce la matematica, dall'altro assegna alla fisica ed alle scienze naturali il ruolo di discipline facoltative.

È certo, d'altro canto, che al di là delle innumerevoli cause di ordine contingente alle quali può essere addebitato il decadere degli studi di fisica, sta l'ovvia realtà della insufficienza delle strutture accademiche tradizionali: attendersi che esse possano soddisfare le aspirazioni dei giovani studiosi sarebbe ingiusto prima ancora che sommamente ingenuo.

Physics Studies in a Critical State

If one is to believe certain opinions that are not lacking in authority, physics studies throughout the world, and above all in German and French research centres, are going through a very critical phase.

What are the causes of this, and how does it appear? On various sides one hears complaints of the waning of the impulse of research, the lack of enthusiasm among the ranks of the young students in the big European and American universities and the scanty or nugatory results that have been obtained for many years now. Recently, in the « Frankfurter Allgemeine », K. Kudzinski has written that no real progress has been made in research on matter for some time. In practice, he states, since Werner Heisenberg, over ten years ago, elaborated his formula, no appreciable advances have been made. Even if it is true that, with the aid of super-accelerators, new elementary particles have been discovered, it is also true that the relationships that unite them are still largely unknown and that no satisfactory theory on the subject has yet been formulated.

The present state of affairs, adds Kudzinski, seems to confirm the opinion of Prof. Melvin Schwartz, a physicist at Stanford University in California and a member of the Committee of the Weizmann Institut in Israel, who attributes the cause of the present situation to the waning of impulse among young physicists. He claims that this state of frustation originates from the « sociological structure » of high-energy physics. Obstacles of an economic nature that are not easily overcome (e.g. the cost of the big accelerators, the expense of their maintenance, the duration of the experiments, which often exceeds a thousand hours) limit research studies, which number no more than twenty in any year and are naturally confined to the research groups already constituted. In this way, young physicists remain excluded from the more active roles in research, to which they are unable to give the contribution of their energies, their unprejudiced approach and their creative abilities.

In order to remedy this state of affairs, Prof. Schwartz proposes that the more promising among the young physicists be assured grants which, even if not large, would be sufficient to allow them to engage in research on their own and to verify experimentally the validity of their theories. He also proposes that research groups that have already accomplished their tasks should be dissolved and that new ones should be set up, with new programmes. This should apply not only to high-energy physics but also to all the other fields of scientific research.

The situation in physics studies does not seem to be appreciably different in France, where the paralysing forces acting upon research have been identified by authoritative observes in the general indifference that surrounds experts and, still worse, in the hostility of the public powers. To this may be added the delay in the revival of French industry and the total lack of enthusiasm that the academic powers find in the ranks of young students (though, in this respect, it must be added that one cannot see why these young people should show any enthusiasm in following a road that they see is barred by rigid structures of power and privilege). These bitter observations have been made even recently, in the course of the « Evian Meeting » promoted by the Société Française de Physique. Prof. Friedel, who was among those taking part in the Meeting, actually stated that, in France, basic research is regressing in a catastrophic manner, even if the last fifteen years have seen results that, in themselves, constituted a success of international scope.

Again at Evian, harsh criticisms were expressed regarding the new French Plan and the criteria that inspire it, and it was pointed out that the previous Plan was only sixty per cent put into effect. The protests of the French physicists were prompted, among other things, by the fact that the government seems determined that the sum total of the funds to be allocated to basic research is to be the same amount of the gross national revenue as that which was allocated in 1965. It this way, it was observed, in 1975 France will have achieved the alluring result of having gone back ten years! In any case, it is a fact that, between the experts and the public powers, there has arisen a relationship of mutual mistrust that cannot be considered advantageous to the aims of research.

Equally worrying is the indifference of the young towards physics studies. The plan of scholastic orientation in which the French government placed so many hopes seems to have been a total failure: it is impossible not to regard as an indication of failure the fact that this year, at Grenoble, two thirds of the places available in the physics laboratories for the third university cycle remained unfilled. The Ministry of Education, on its part, is preparing to aggravate this state of affairs with a reform of the secondary schools, which, even if it favours mathematics, assigns to physics and natural sciences the role of optional disciplines.

On the other hand, it is certain that beyond the innumerable contingent causes to which the decline in physics studies may be due lies the obvious reality of the inadequacy of the traditional academic structures: to expect that these can fulfil the aspirations of young scientists would be unfair, as well as exceedingly ingenuous.

La scuola in Europa

Merita d'essere segnalata - molto più per i problemi che inconsapevolmente solleva che per i risultati che promette - una riunione alla quale hanno preso parte, nello scorso novembre a Bruxelles, i ministri dell'istruzione dei paesi membri della Comunità europea. L'incontro si proponeva uno scopo altamente plausibile: quello di promuovere una più stretta collaborazione fra le diverse strutture scolastiche nazionali; e a tale processo di integrazione - parallelo a quello che si è già attuato sul piano economico e che si tenta di realizzare anche sul terreno politico - si attribuiva particolare importanza nel momento attuale, che vede tutti i paesi europei impegnati nello sforzo di rinnovare i loro ordinamenti scolastici, già da tempo scossi da una grave crisi.

Si tentava dunque, di iscrivere i mali che oggi affliggono la scuola in un contesto non più nazionale, ma europeo. Ciò sarebbe stato un fatto altamente positivo, se fosse nato da una precisa individuazione delle cause della crisi: cause che non possono essere riferite ai singoli paesi unicamente perchè legate, non a particolari vicende o a particolari istituzioni, ma ad un determinato assetto della società. Erano quindi da porre in discussione, fondamentalmente, il tipo di cultura che nella scuola si amministra, il modo in cui la si amministra, la funzione stessa della scuola all'interno della società.

I ministri riunitisi a Bruxelles si sono invece limitati a considerare i problemi della scuola in termini di pura efficienza e di produttività; hanno riconosciuto che è necessario un più efficace collegamento fra gli organismi scolastici dei diversi paesi; che è ormai divenuto preminente il problema della migliore utilizzazione dei fondi destinati alla scuola; che è indispensabile garantire la massima mobilità dei docenti e degli allievi all'interno della Comunità. Hanno, soprattutto, affermato che lo sviluppo dell'insegnamento risponde ai fini del trattato di Roma, almeno in quanto è condizione dell'espansione economica.

In sostanza, le vicende della scuola sono state esaminate secondo una logica imprenditoriale che è spesso prevalsa in seno alla Comunità, e non sempre con risultati ottimi; non sembra, comunque, che essa potesse offrire strumenti efficaci ad una valutazione attendibile del disagio esistente nella scuola. Nè i mezzi che i ministri si propongono di adottare per garantire il buon funzionamento della scuola sembrano sostanzialmente diversi da un palo cui si chiede di sorreggere un edificio vacillante. Difficilmente potranno avere maggiore efficacia o miglior fortuna.

The School in Europe

Mention should be made - much more for the problems that it unconsciously raises than for the results that it promises - of a meeting held last November in Brussels, which was attended by the Ministers of Education of the member countries of the European Community. The meeting set itself a highly praiseworthy aim: to promote a closer collaboration between the different national school structures; and to this process of integration - parallel to that which has already taken place on the economic plane and has also been attempted on the political level - particular importance was attributed at the present moment, when all European countries are engaged in an effort to reorganise their school systems, which have for some time been in a serious state of upheaval.

An attempt was therefore made to set the evils that today afflict the school in a context that is no longer national but European. This would have been something highly positive, if it had originated from a precise identification of the causes of the crisis: causes that cannot be referred solely to the individual countries because they are associated not with particular events or particular institutions but with a particular condition of society. Fundamentally, therefore, the points to be discussed were: the type of culture that is administered in the school, the way in which it is administered and the very function of the school within society.

However, the ministers who met in Brussels confined themselves to considering the problems of the school in terms of pure efficiency and productivity; they recognised: that more effective association is necessary between the school organisms of the different countries; that one of the main problems is now that of the better utilisation of the funds intended for schools; and that it is indispensable to guarantee the maximum mobility of teachers and pupils within the Community. Above all, they affirmed that the development of teaching is in accordance with the aims of the Rome Treaty, at least in the sense that it is a condition of economic expansion.

In short, school matters were examined according to a contractorial logic that has often prevailed within the Community, and not always with the best results; however, it does not seem that it could offer effective instruments for a reliable assessment of the uneasy situation existing in schools. Nor do the means that the ministers propose to adopt to guarantee the proper functioning of the school seem very different from a post that is expected to prop up a tottering building. They are unlikely to be more effective or to have a better result.

Perchè invecchia l'organismo

Una teoria per più di un verso ardita sulle cause dell'invecchiamento dell'organismo umano è stata elaborata dal Dr. Steven Pelc, del servizio di ricerca biofisica del consiglio britannico della ricerca medica. Secondo tale teoria la senescenza sarebbe dovuta al progressivo deterioramento del codice genetico - o almeno di alcune copie di esso - inscritto in ciascuna cellula dell'organismo.

È noto che l'invecchiamento non colpisce l'organismo nel suo complesso, ma ciascuna delle innumerevoli cellule che ne fanno parte: ne consegue che lo studio di questo fondamentale fenomeno inscindibilmente connesso alla vita, deve rivolgersi alle cellule. Anzi, poichè la struttura fondamentale è sempre uguale, qualunque sia la funzione cui esse adempiano, chiarire il processo che ha luogo in una singola cellula può valere a spiegare in larga parte il meccanismo dell'invecchiamento.

Già la scoperta del DNA - l'acido, contenuto nel nucleo della cellula, che conserva in sè il codice genetico - aveva indotto diversi scienziati a supporre che proprio il graduale deteriorarsi di quest'acido nel corso della vita, per esempio a causa delle radiazioni, potesse essere la causa dell'invecchiamento e infine della morte della cellula. Tale teoria è stata ora precisata dal Dr. Pelc, il quale vede una conferma della sua teoria nel fatto che la parte centrale del DNA si riproduce o raddoppia fin dall'inizio della vita della cellula. Tale parte centrale riceve poi protezione da uno strato di proteine che le avvolge fino al momento in cui la cellula dovrà dividersi. Ne segue, evidentemente, che il codice genetico viene trasmesso intatto alla cellula figlia e non presenta le lesioni o i segni di decadimento che conducono alla morte la cellula madre.

Il Dr. Pelc afferma, su queste basi, che si potrebbe impedire, o almeno limitare, l'invecchiamento impedendo l'estendersi del decadimento. Egli ritiene indicativo, a questo riguardo, il caso del paramecio, un animale unicellulare che possiede due nuclei, uno maggiore e l'altro minore: secondo il ricercatore inglese, il nucleo piccolo conterrebbe il codice genetico originale che viene trasmesso intatto alle generazioni seguenti, mentre il nucleo grande conterrebbe gli esemplari del DNA utilizzati durante la vita dell'individuo: proprio qui si produrrebbe quel processo di decadimento che provoca l'invecchiamento e, infine, la morte. La teoria del Dr. Pelc, in definitiva, tende a stabilire uno stretto nesso fra il comportamento del codice genetico e la senescenza, aprendo prospettive del tutto nuove alla ricerca di mezzi che consentano di controllare questo fenomeno.

Why the Organism Grows Old

A daring theory - in more than one sense - on the causes of the ageing of the human organism has been elaborated by Dr Steven Pelc, of the biophysical research service of the British Council of Medical Research. According to this theory, senescence is due to the progressive deterioration of the genetic code - or at least of certain pairs of it - inscribed in each cell of the organism.

It is known that ageing does not affect the organism as a whole, but rather each of the innumerable cells of which it is composed: it follows that a study of this fundamental phenomenon, inseparably connnected with life, must be directed towards the cells. Indeed, since the fundamental structure is always the same, whatever the function that the cells perform, a clarification of the process that takes place in a single cell may help to explain, to a great extent, the mechanism of ageing.

The discovery of DNA - the acid, contained in the nucleus of the cell, that holds within itself the genetic code - had already induced various scientists to suppose that it is, in fact, the gradual deterioration of this acid in the course of life - for example, on account of radiations - that might be the cause of ageing and, in short, of the death of the cell. This theory has now been stated in precise terms by Dr Pelc, who sees a confirmation of his theory in the fact that the central part of the DNA reproduces or doubles from the beginning of the life of the cell. This central part then receives protection by a layer of proteins that envelop it until the moment when the cell has to divide. It follows, obviously, that the genetic code is transmitted intact to the daughter cell and has not those lesions or signs of decay that lead to the death of the mother cell.

On these bases, Dr Pelc affirms that one could hinder, or at least limit, ageing by preventing the decay from spreading. In this respect, he considers indicative the case of the paramecium, a single-celled animal that possesses two nuclei, a larger one and a smaller one: according to the English biophysicist, the small nucleus contains the original genetic code that is transmitted intact to the following generations, while the large nucleus contains the DNA elements utilised during the life of the individual: it is here, according to him, that lies the site of the process of decay that causes ageing and, in short, death. In other words, Dr Pelc's theory tends to establish a close connection between the behaviour of the genetic code and senescence, opening up entirely new prospects for the search for methods of controlling this phenomenon.

RECENSIONI - BOOK REVIEWS

T. Dobzhansky, *Genetics of the Evolutionary Process*, Columbia University Press, London, 1970, xiv+506 pp., figs. and tables, £4.95.

In 1937, Dobzhansky published the volume « Genetics and the Origin of Species », which marked an important date in the history of the interpretation of evolutionary phenomena on a genetic basis. The later editions of 1941 and 1951 were revisions completed and brought up to date by the author, and that they can be considered as new books, though preserving the original structure. In embarking upon the preparation of a fourth edition, the author found himself faced with so many innovations in evolutionary biology that he deemed it necessary to give his book a new layout, and hence a new title.

However, as he points out, one should not conclude that what was considered true in 1951 has now been found to be inaccurate. On the contrary, a great number of the ideas acquired at that time are fully confirmed - indeed, they have become common knowledge in present-day biological culture - such that it is not worth wasting words in demonstrating many of those propositions, for which at that time detailed proof was required. Today, states the author, it is not necessary to spend time in convincing the reader that mutations are not only products of the laboratory but are also found among wild populations; or that the differences between the sub-species of animals and plants in the wild state are essentially genetic differences; or that the differences between the species are of the same elementary nature as the intraspecific and individual differences.

On the other hand, an analysis of the processes by which natural selection takes place, which has been developed during recent years, calls for a more careful and detailed treatment; the discoveries of molecular biology - since they relate to the substance on which the genetic information is inscribed and shed light on the basic process of evolution: the production of mutations - have provided further information on the dynamics of evolution.

In fact, the volume that we are now offered - the result of Dobzhansky's tireless activity - may be said to be completely new in comparison with the previous volumes, and it takes account of the essential bibliography on the vast subject of evolution up to the end of 1969. One of the most significant characteristics of this volume is the thorough treatment of the problems of selection, to which the four central chapters are devoted, a distinction being made between conservative or normalising selection, balanced selection, diversifying selection and directive selection. Several pages are devoted to the much-discussed problems of genetic load and neutral mutations, with an abundance of information on the various ideas expressed on the subject. One chapter is devoted to a thorough examination of « genetic drift » and the « founder principle ». The last chapters offer a critical discussion of the concepts of species, race and population; the mechanisms of reproductive isolation; the methods of formation of new species; and the methods of evolution considered synthetically.

The volume, apart from being thoroughly documented, makes pleasant reading. The author's vast learning, not only from the biological but also from the philosophical and historical points of view, enables him to set the analytical aspects of the study within the framework of the vast problems of a biological, philosophical and social nature that evolution opens up to our minds. The mathematics involved, which have become highly abstruse for the layman, are reduced to a minimum; Dobzhansky, who admits that he does not feel completely at home in mathematics, has the ability to master the conclusions and make them clear to the reader, without getting involved in a maze of algebra.

All biologists, geneticists or otherwise, and all wishing to make a serious but rapid study of the present state of evolutionary genetics should be grateful to Dobzhansky for this new work of his.

Together with the three previous editions, the present volume constitutes a sort of encyclopedia of modern evolutionary concepts, written in a clear and accurate manner by a naturalist of wide culture, to whom are due fundamental contributions in the field of classical genetics and particularly of the genetical interpretation of evolution.

Roma, Università, Istituto di Genetica.

G. Montalenti

P. Weingartner and G. Zecha (eds.), *Induction, Physics, and Ethics* (Proceedings and Discussions of the 1968 Salzburg Colloquium in the Philosophy of Science), D. Reidel Publishing Company, Dordrecht, 1970, x+382 pp., figs., Dfl. 65.

This volume is a collection of papers presented at the colloquium on the philosophy of science held at Salzburg from 28th to 31st August 1968 and discussions on them. The colloquium was organized in three main sections: induction and probability; the foundations of physics; science and ethics: the moral responsibility of scientists. The volume consists therefore of three parts corresponding to these three sections of the colloquium.

The papers in the first section were presented by some of the most eminent investigators of questions concerning the foundations of probability and inductive logic such as, for instance, B. de Finetti and J. Hintikka. The prepared papers and the ensuing discussions alike were at a very high level and for this reason the first part of the book provides extremely interesting reading for anyone concerned in these matters. All points of view were represented at the colloquium (subjectivist, logical, statistical-frequentist) so that the various papers provide a full panorama of the present state of researches into the foundations of probability.

Great interest attaches also to the second part, which is concerned with the foundations of physics, making a wide survey is made of problems connected with the foundations of the general theory of relativity and of quantum theory. Besides this the articles by Strauss and Bunge on the problematics of the relations between theories are of particular interest.

The part concerned with the moral responsibility of scientists is the least satisfying of the three. In the opinion of this reviewer a problem as important and topical as this should have been treated more fully and certainly in greater depth than was the case.

Perugia, Università, Istituto di Matematica.

D. Costantini

W. K. Essler, *Induktive Logik* (Grundlagen und Voraussetzungen), Verlag Karl Alber GmbH, Freiburg, 1970, 378 pp., figs., No price stated.

Inductive logic was rigorously systematized for the first time in a series of publications by R. Carnap during the forties, the most important of which was his celebrated volume *Logical Foundations of Probability*. Carnap also, with other authors, subsequently amplified and modified the systematization, but without reaching any conclusive result, so that studies aiming to formalize inductive logic remain far short of having given it a definitive appearance even today.

In this volume, W. K. Essler has surveyed a large part of the results achieved in this discipline to date in a systematic manner bearing a stamp of originality. But the originality of the book under review is not limited to this, for it relates also to some specific results. For instance, theorem 13 shows that one of Carnap's axioms of relevance is dependent on the other axioms. This dependence had not hitherto been known.

All things considered, and even disregarding the interest of the original results, Essler's work supplies an admirable presentation of the problematics and of the products from the most recent and meanwhile most fascinating branches of modern symbolic logic. While obviously its greatest interest will be for those who are directly concerned with inductive logic it can also be an excellent means of making contact with that field of research.

Essler's work is deficient in only one respect: not enough space has been devoted to the Finnish school of logicians headed by J. Hintikka whose works, in our opinion, represent the most advanced and interesting contributions made to inductive logic during recent years.

Perugia, Università, Istituto di Matematica.

D. Costantini

A. Koyré, *Dal mondo chiuso all'universo infinito* (Series: *Storia della scienza*), Feltrinelli Editore, Milano, 1970, 216 pp., figs., L. 2.500.

Generally the works of A. Koyré give no certain clue to his philosophical and methodological views, « From the closed world to Infinite Universe » is an exception: the philosopher's God, he says, is correlated with his world. This proposition is both the starting point for the book and the result reached in it after a detailed analysis of the philosophical and metaphysical positions taken up by the protagonists of the « cosmological revolution » which reached its peak in Newtonian mathematical philosophy. To Koyré, however, the history of scientific thought (like the history of ideas in general) is the history of a continuous dialectical interaction - certainly not pre-ordained - between « individual mental structure » and « mentality » in the sense of L. Lévy-Bruhl, and in order to understand better any single person's contribution to the history of thought that person must be continually related to the « great spiritual currents » in which he is immersed. This does not mean that « currents of ideas » are incapable of modification. The contrary is true, for what remains standing is « the formal identity of the categories of thought ». Thus the argument of H. Moore, setting out to prove that space is something distinct from matter, is constructed on the same model as the Cartesian one which was meant to prove the non-existence of any internal locus and therefore that space and matter are completely identical. In point of fact, both to the Cartesians and to the followers of Newton there can be no attribute without a real substance to sustain it. Hence the Cartesians are compelled to deny any relationship (other than that of creation) between material extension on the one hand and God on the other, whereas the Newtonians who argue that space (the container) and matter (the content) are completely heterogeneous, and who consider space to be an attribute of God, must needs assert that God is continuously active in the world if they want to salvage the phenomena of attraction and repulsion (gravity). Although these two conclusions are opposed to one another the two contestants have a single aim: that of saving the dignity of God in his relations with the world! The Newtonians see no difficulty in accepting the reality of spiritual substance, a substance which in addition to the three normal dimensions of matter has a particular kind of « essential dimension ». Hence they are unable to accept the reducing programme of Descartes (which is what Newton meant by « *hypotheses non fingo* »). For the Cartesians, on the other hand, the relative character of space is indisputable. Yet the time is not yet ripe for the presupposition - again a metaphysical one - that matter and extension are identical to be followed out to its extreme conclusion. Victory rests with the Newtonians for whom « the infinite universe inherits all the ontological attributes of divinity ».

Roma, Università.

V. CINANNI

K. R. POPPER, *Logica della scoperta scientifica* (Il carattere autocorrettivo della scienza), Editore Einaudi, Torino, 1970, xxx+550 pp., L. 3.400.

This is the Italian translation from the English edition (1959) of Popper's book with a new introduction added by the author. The main theme of the work consists in criticism of inductive methods and the part they play in scientific discovery. In the author's view hypothesis is not the outcome from inductive procedures: it is something first suggested and then controlled by *deductive* methods. This does not, however, imply adherance to conventionalized concepts, for experience can and ought to « falsify » theories and that is what impels theorizers to look for better theories and enables us to understand the importance which Popper attaches to « *deduction* » as a means of reconciling the physical world with the « theoretical network ». It is not a case of reducing theories to the language of elementary or protocolar propositions which are part of the language of our sense; in fact the only « elementary propositions » that call for consideration are those of scientific experiment and the only connection between these and the theories consists in their deducibility. This fact implies the impossibility of reduction between two heterogeneous levels: that of theory and that of experience (and hence the need for a *principle of demarcation* between the two). The chapter of probability has a place to itself in the book in which the author, recognising the psychological limitations of subjectivistic and logical interpretations, proposes a new theory of frequency which takes account of the criticisms of axioms (of limits and disorder) justly advanced by von Mises. He completely rejects the axiom of limit but he accepts a « *modified axiom of disorder* ». From this objectivistic point of view he proceeds to analyse the metaphysical indeterminism of the quantum theory. He criticizes also the claimed reduction of the probability of hypotheses to the probability of events: hypotheses, whether they are deterministic laws or probabilistic laws, can only be corroborated, and this only to the extent that they can be confirmed.

Roma, Università.

V. CINANNI

G. BALANDIER, *Anthropologie politique*, 2nd rev. ed., Presses Universitaires de France, Paris, 1969, XII+240 pp., 11 F.

This essay by Balandier is important as marking a new departure in the field of ethnological science. It develops a particularly modern branch of social anthropology, namely political anthropology, which was born in or around the forties and has rapidly taken shape as a discipline fruitful of results both on the anthropological and the general epistemological plane. By contrast with the kind of ethnology that has tarried in positions now obsolete, Balandier rejects the hold opposition of societies with what is called « cyclical » development to those with « evolving » development, these terms being used to distinguish between segmented, hereditary, acephalous, primitive, « traditional » societies and those which are state-constituted, «class-stratified», « modern ». The permanence of « historicity » even in economically retarded countries, that is to say evolutionary dynamism allied with historical consciousness anyway at the communal if not at the individual level, is forcibly re-evaluated. Balandier's analysis of the forms of power in what are called « acephalous » or non-state societies is carried well beyond the scope of his own experience of African conditions in order to consider relevant problems of an epistemological kind, enabling the author to reach some remarkable conclusions. The existence of « power » even in the simplest societies; the conflicting, competitive, antagonistic features of their social structures; the movements pervading them both internally and in their external relations, between different societies; the impossibility of separating the political from other spheres of culture such as the kinship and descent systems or the ritual and religious activities, etc.: all these factors are brought to light by a «situational» mode of analysis which is polemically the opposite of the formal analyses pursued in the various « structuralisms ». The latter have proved totally inadequate for the purposes of political anthropology, concerned as this is with the ever changing tensions, disequilibrium and restoration of balance. The approach chosen by Balandier is and could not be otherwise than dynamic. It demands a re-evaluation of history as regards societies too often regarded as existing « outside of history » or in the limbo of history. This type of political anthropology involves repudiation and correction of concepts and limits imposed by all ethnocentric philosophies and by traditional historiography. Here is something that goes beyond the « morphology » of political forms and manifestations that are the substance of some classical manuals of ethnology and social anthropology (Lowie, etc.).

Problematics are the core of this book which questions the validity of such currently basic concepts as « politics », « power », « social class », « state », « feudalism », « traditionalism » and a re-evaluation of sacred myths in respect with their political function. The excellence of Balandier's contribution lies in his critical and challenging awareness which will interest not only the ethnologist but also the philosopher, the historian and the thoughtful reader in general.

Roma.

V. LANTERNARI

A. M. MUSU, *Motivi e strutture dell'istruzione* (Series: *Problemi del nostro tempo*), Vallecchi Editore, Firenze, 1970, 290 pp., L. 3.000.

An educational problem which, especially in Italy, has attracted only rather casual attention, or has been considered only in order to demonstrate that it is secondary with respect to the philosophical problem, is that of teaching. Here teaching is not to be understood in the specialised sense of the term but rather, as the Author would have it, with an existential qualification, aimed at redeeming and re-establishing a scheme of learning that would have a less purely instrumental character and occupy a less subordinate position than that to which it has been relegated by recent educational philosophies.

To give a free translation of the Author's definition, teaching is organisation which allows the learner to set his own course and plan his own educative processing. The function of the school is precisely this organisational one of bringing a system into operation that will promote, in the learner, the appropriate behaviour that constitutes learning.

The Author carefully surveys the motivations which the various sciences (from biology to physiology and psychology) are able to offer as stimuli for the self-educational efforts that each educand is called upon to make in his preparation for life, all being consummated in the individual's capacity to face the « risks » involved in his existence.

In the context of today's havoc of the school (not only in Italy) this book is to be seen as the prolegomenon for a science of the school that would be truly capable of giving educational expression to the scientific and technological outlook of a world undergoing rapid changes.

The Author trenchantly argues the need for the school to create a relationship between teaching and learning under proper technical conditions for enabling the learner to measure the actual « profit » he gains from the information received towards offsetting the risks of his life plan.

It is clear that instruction, understood in this sense, calls for much more up-to-date programming than is available at present. The Author should also be recognised as one of the very few educationists who refer educational discussion to the latest developments of information theory in the light of epistemological understanding, for without the latter it would seem impossible to define what should be the content of a science of the school aiming primarily, as he desires, to be the science of man as learner.

Salerno, Università.

V. Tonini

J. F. Crow and M. Kimura, *An Introduction to Population Genetics Theory*, Harper & Row, Publishers, New York, 1970, xvi+592 pp., figs. and tables, No price stated.

Shortly after the rediscovery of Mendel's laws (1900), there began the study of the kinetics of the genes in the successive generations of a population.

In 1908 the English mathematician G. H. Hardy and the German physician W. Weinberg independently discovered the principle of the constancy of the gene frequencies in a population, which is today universally known as the Hardy-Weinberg principle. The discovery passed by practically unnoticed. Between 1920 and 1930, three geneticists, J. B. S. Haldane, R. A. Fisher and S. Wright, working independently, elaborated the bases of the mathematical theory of population genetics. Thus they opened up the way to the interpretation of evolution in genetic terms. Later, this branch of genetics underwent vigorous development, as regards both theory and its practical application to phenomena observed in nature or obtained experimentally.

The authors of the present volume have both made very important contributions to population genetics. In this work they have given an account, for the benefit of graduate students in genetics, of the foundations of the mathematical theory relative to this subject, in a highly analytical manner and with various examples. However, the experimental part has been deliberately omitted.

The first chapter is concerned with the growth models of various types of population; the second is devoted to the kinetics of the genes in populations in which crosses occur at random. The following chapters deal with consanguinity, selection, populations in equilibrium, the stochastic processes causing the variations of the gene frequencies, and the distribution of the gene frequencies in the populations, whether they be small or infinitely large. An appendix is devoted to a mention of the statistical and mathematical methods most frequently employed in the course of the volume. The bibliography is very complete.

This volume, which appears 15 years after the classical work by C.C.Li on the same subject, also gathers together and summarises the latest developments in this field (for example, the concept and methods of calculation of the genetic load), which is in a state of active and rapid evolution. It can therefore be warmly recommended both to students, for whom it may be found rather difficult reading, and to geneticists, as a text on which to prepare courses on population genetics.

Roma, Università, Istituto di Genetica.

G. Montalenti

L. Margulis, *Origin of Eukaryotic Cells* (Evidence and Research Implications for a Theory of the Origin and Evolution of Microbial, Plant and Animal Cells on the Precambrian Earth), Yale University Press, London, 1970, xxii+350 pp., figs. and tables, £6.75.

Multa renascentur, quae jam cecidere. About the year 1920, some authors (Portier, 1918; Wallin, 1927) formulated a theory according to which some of the cellular organelles (mitochondria, plastids) were considered as initially autonomous micro-organisms, which had adapted themselves to life in symbiosis within the cells. In Italy this theory found supporters in U. Pierantoni and, late in life, in the eminent histologist Camillo Golgi, who considered that this theory could be applied to the yolk granules of the oocytes. The theory was received with scepticism and was the object of ironical comments. Today, it has returned to the limelight, repropounded by the Nobel prizewinner J. Lederberg (1952) and others, and is claiming for increasing attention, in the light of the recent demonstration of the existence of deoxyribonucleic acid (DNA) bearer of the genetic information both in mitochondria and in plastids (up to a few years ago it was thought that DNA was only present in the nucleus).

In the present volume, the author assembles and discusses with « infectious enthusiasm » (as G. E. Hutchinson puts it, in the preface) all the morphological, biochemical and paleontological arguments that demonstrate the substantial difference between the prokaryotic organisms (bacteria and cyanophycea) and the eukaryotic organisms (i.e. those having a typical cellular structure: namely, all plants and uni- or pluri-cellular animals), as well as the arguments according to which it is inconceivable that the latter are derived from the former, unless it is assumed that, at a certain moment, before the Cambrian period (i.e. over 600 million years ago), processes of symbiosis resulted in the constitution of the cell with the structure that we recognise in it today.

In the phyletic history outlined by the author, on the basis of the existing extensive documentation, much is still hypothetical and conjectural. In reading the various arguments and regarding the phylogenetic trees presented, one inevitably recalls the famous genealogical trees that H. Haeckel had drawn up about a century ago. Also the name « monera », of Haeckelian memory, has recently returned to the limelight in the world of science (R. H. Whittaker, 1969) as one of the five « kingdoms » of classification (Monera, Protista, Fungi, Animalia, Plantae). It must be acknowledged that some of the genealogies postulated by Haeckel, in which little faith had been placed, have shown themselves to be near the truth. Thus it is probable that many of the present author's arguments, which are always based on experimental and observational data, may constitute a guide for an interpretation of the first steps of biological evolution: namely, of the phenomena that occurred during the long lapse of time of 3000 million years or more, which precedes the explosion of living forms that took place in the Cambrian period. In this period, the first in the Paleozoic era, there appear, in fact, all at once, as many as six of the eight main animal *phyla* (the other two appear in the Ordovician era, immediately subsequent to Cambrian) that continue up to the present day. This apparent explosion of life in the Cambrian period constitutes one of the great problems in the history of life upon the Earth, and is one of the problems to which the author sets out to find a solution.

The work of synthesis that she has achieved, by amalgamating and comparing the data of disciplines apparently so far removed as systematics, classical and electronic cytology, biochemistry, molecular biology and paleontology, is of considerable value and profound significance, and the results - even if they cannot all be accepted without reservations - are such as to give rise to reflection and stimulate the imagination of every biologist.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

L. VALZELLI, *Elementi di psicofarmacologia sperimentale e clinica*, Carlo Manfredi Editore, Milano, 1970, XII+282 pp., figs. and tables, L. 8.400.

This book is devoted to psychopharmacology, seen from an experimental and clinical point of view. Psychopharmacology is presented in this study in an interdisciplinary approach that can interest pharmacologists, neurophysiologists, psychologists, psychiatrists, as well as general practitioners.

The first part of the volume is devoted to several neurophysiological mechanisms which operate in the brain: mechanisms of sleep and wakefulness, memory organization, the neuronal membrane, synaptic transmission. In Chapter 3 the various mediators that act in the brain are described - Serotine, Noradrenalin, Dopamine, Acetylcoline, Histamine - and their distribution is discussed on an experimental basis. Chapters 4 and 5 concern an approach to current methodology in psychopharmacology and to neurochemical correlations of behaviour.

The part of the book that may have a particular interest for psychiatrists and general practitioners is that in which the psychodrugs are classified. Sedative drugs, the major tranquillizers (Fenotiazine, Reserpina, Butirrofenoni), the minor tranquillizers (Meprobamati, Benzodiazepine), antidepressives (MAO inhibitors etc.) are introduced and discussed in their general aspects, mechanisms of action in the brain and behavioural and therapeutic effects.

The book has an extensive and fairly complete bibliography, deals clearly and simply with a complicated matter such as psychopharmacology and will unquestionably be useful to experimental pharmacologists and therapeutists.

Milano, Università, Laboratorio di Fisiologia Umana.

M. MANCIA

E. R. Hilgard and G. H. Bower, *Le teorie dell'apprendimento*, Franco Angeli Editore, Milano, 1970, 850 pp., figs. and tables, L. 14.000.

The Italian edition of this book, which appeared in the United States in 1966, is presented by D. F. Romano, whose introduction is a clear historical, methodological and political analysis of the behavioural sciences which have dominated psychological researches in The United States.

The book starts with basic theories of learning, the connectionism theories of Thorndike and the classical conditioning studies of Pavlov. These parts, together with the operational conditioning of Skinner, are well represented and discussed. Subsequently, several pages are centered on Hull's systematic theory and on the mechanisms of learning discussed by Tolman.

After a brief presentation of the Gestalt theories, a full chapter is devoted to Freud's psychodynamic theory, particularly to the psychoanalytic concepts which have influenced experiments and theories of learning. The part of the book dealing with the mathematical theory of learning and that discussing the behavioural theories based on cybernetic processing of information appear very interesting.

The last part of the book introduces some neurophysiological ideas on the understanding of learning and some technological problems of learning and instruction.

The book will be of great interest to psychologists, physiologists, neurophysiologists and anyone wishing to know about the basic approaches to the dominating theories of behaviour.

Milano, Università, Laboratorio di Fisiologia.

M. Mancia

P. Ganter and G. Jollès, *Histochimie normale et pathologique*, Volume I and II, Gauthier-Villars, Paris, 1969-1970, xvi+1904 pp., figs. and tables, 195 F. each volume.

Amid the recent efflorescence of texts on histochemical technique, this volume by Ganter and Jollès occupies a place apart, on account of the broad and orderly manner in which the subject matter is presented and the completeness of the information contained in it.

The strictly histochemical part is preceded by an extensively developed chapter on the basic histological methods, a subject somewhat marginal to true histochemistry. There follow two general chapters: « The purposes and methods of histochemistry » and « General reactions of histochemistry ».

In each of the following chapters the authors examine the individual groups of substances, according to their normal division. Each chapter begins with an extensive chemical survey of the group of substances and continues with the properly histological part, conducted on the basis of general characteristics for the group and particular characteristics for the individual substances; it concludes with an ample bibliography. The last of these chapters, entitled « Various substances », comprises a study not only of urea and uric acid (which perhaps might have been considered in an appendix to other groups of substances) but also of various substances extraneous to the organism, such as silica and asbestos, which may be important for pathology. In the whole of this analytical part there is an abundance of very clear and well planned schematic tables.

A long chapter, running to over three hundred pages, is devoted to the applications of histochemistry to normal and pathological histology, considered in an analytical and detailed manner; unfortunately it is confined to certain particular subjects. Lastly, as is also found in other texts, there is a chapter devoted to the formulae of the principal recommended reactions, together with a list of the main chemicals used in histochemistry, with the relative empirical and structural formulae and some of their characteristics, as well as buffer formulae and other common tables of chemical data.

The volume is completely lacking in any mention of enzymatic histochemistry; however, in the preface to the work, by J. Delarue, it is announced that the study is to be completed by the publication of a third volume devoted to the histochemistry of enzymes. Likewise, the volume merely lists some of the more advanced physical processes that may be useful in histochemistry; the authors remark that, since the methods involved necessitate apparatus that is costly, highly specialised and not easy to use, they have not considered it worthwhile to include a discussion of them. This lacuna is unfortunate because a part corresponding in size to the other parts of the volume would have clearly illustrated the importance of these methods even to anyone that merely wished to have a clear and complete idea of them.

Nevertheless, on account of the nature and extent of its subject matter, this handsomely printed volume is to be warmly recommended.

Pavia, Università, Istituto di Anatomia Comparata.

M. VIALLI

J. ROSSEL and E. JEANNET, *Physique Générale*, Editions du Griffon, Neuchâtel, distributed by Dunod, Paris, 1970, 582 pp., figs., 69 F.

There have in recent years been many attempts to revive traditional teaching methods in education at all levels. This includes in particular a number of American books on physics, which at first seemed destined to have considerable success, but are now subject to some doubts.

It therefore seems of interest to mention this textbook of general physics by J. Rossel of Neuchâtel University, which differs appreciably from the usual university books, though not indulging the revolutionary methods of teaching that are now in fashion.

The author seeks to meet two kinds of needs: the need for a supplementary textbook to accompany university courses, with the subject condensed into a single easily handled volume, and the need for a satisfactory means of bringing up to date technicians and research workers after some years of practical work, who do not have time to keep regularly informed of the continual development of modern physics.

The whole of the material is, then, brought into a single volume of some 500 pages. An introduction dealing with mathematical needs and with some basic and current aspects of physics (what physics is about; experiment and theory; interaction of elementary particles; atoms, molecules and crystals; states of matter), the conventional branches of physics are discussed, in a modern and very effective manner: classical mechanics and relativity; heat and thermodynamics; wave physics; electricity and electromagnetic properties of matter; atomic systems; interaction of radiation with matter. The last two chapters are preceded by a simple and brief account of the essential concepts of quantum mechanics.

The typography is excellent. In the reviewer's opinion, the book is to be highly recommended for supplementary use by students and all those whose work calls for even occasional use of physical concepts.

Milano, Università, Istituto di Scienze Fisiche « A. Pontremoli ».

P. Caldirola

M. H. Pirenne, *Optics Painting and Photography*, Cambridge University Press, London, 1970, xxiv+200 pp., figs., £4.

The contribution of Pirenne to Physiological Optics is well known. Paramount among his work is that devoted to visual threshold: experimental data carefully recorded and dealt with in terms of quantal explanation, after a careful inspection of all the possible sources of biological variability. Since a few quanta at the site of retinal absorption are sufficient to evoke a light sensation, one can have an idea of the high degree of sophistication of these experiments.

Now, there is an aspect of Pirenne's personality which up to now has been less known: the humanistic side. We say « has been », because his recent book « Optics, Painting and Photography » reveals that Pirenne's participation in the fields of Aesthetics and History of Art is not fortuitous.

The author interprets the very meaning of some « illusions » (for example, the ceiling of the Church of St. Ignatius in Rome, by Andrea Pozzo) by taking into account the contribution of the eye as an image-forming system and the perceptual organization of the visual space. All these concepts are inserted in their historical framework, in conjunction with the most recent views.

The point to be emphasized is that Pirenne, although accounting rigorously for visual facts, does not forget the aim of the artist: that the emotion produced by a picture may remain unaltered even if the observer is made aware of the geometrical mechanism subserving it.

A glance at the Table of contents reveals that the reader is given the basic clues for a full understanding of any « illusion »: the simple theory of retinal perspective, the deformation problem, the projection of straight lines and curved lines, subjective curvature, the perception of depth, in spite of the awareness of surface pattern, etc.

The last chapter is devoted to the limitation of pictorial representation and to some psychological problems of the artist. Amongst these, one may mention an interesting problem: the uncertainty of interpretations given by scientific observers on the basis of insufficient information, when the true shape of an object is unknown. This concept is well illustrated by the drawings of the planet Saturn made by astronomers previous to Huygens, as compared with those made by Huygens himself, after having solved many « puzzles ».

In our opinion, this book is suitable for any reader, even one with a limited specific background in the Science of Vision. However, experts in this science will realize the real value of every sentence.

Arcetri-Firenze, Istituto Nazionale di Ottica.

L. Rositani Ronchi

K. Motokawa, *Physiology of Color and Pattern Vision*, Springer-Verlag, Berlin, 1970, 284 pp., figs. and tables, DM 72.

In this book the author has collected 25 years' research by himself and other workers in the physiology department of the School of Medicine, Tohoku University.

The guiding thread, or rather the basic technique, in all this research has been the electrical phosphenes: the minimum current needed to produce a phosphene depends on the conditions of observation, on the waveform, intensity and wavelength of the stimulus, etc. The results of this psychophysical method are in every case compared with those of other authors using different methods, and are often supported by electrophysiological studies of frogs. Some aspects with which the customary electrophysiological methods were unable to deal have been studied in more detail, including optical illusions, induction effects and sensations of movement.

The chapter titles will better show the range of the field of research covered by the author and his group. The first three chapters (Retina; Visual Pathway; Pattern Vision) form a kind of introduction. The phosphene method is introduced in chapter 4 (Electrical Excitability of the Eye as an Indicator of Vision), and is applied in the next two (Retinal Induction; Wave Nature of Retinal Induction). In chapter 7, alternating instead of direct current is used in order to ascertain the effect of frequency on the threshold current for the perception of a phosphene; in chapter 8, the method is modified for the investigation of colour vision, and the various theories of chromatic vision are critically analysed. The last three chapters (two of which have already been mentioned) are entitled Optical Illusions and Figural Aftereffect; Movement Sensation and Stereoscopic Vision; Simulation of the Visual System.

The scientific rigour of this research and the author's own fame will certainly urge visual electrophysiologists to study the book, and perhaps will thereby fulfil the wish expressed by the author in his preface, that his method may prove useful in the study of other aspects of vision.

Arcetri-Firenze, Istituto Nazionale di Ottica.

M. T. Zoli

D. S. Larson, *Quasars and Pulsars*, North Pacific Publishers, Portland, 1971, xii+180 pp., figs. and tables, $6.50.

Anyone who may open this book in search of an account of the remarkable and strange properties of quasars and pulsars will soon see that Larson's title is, in a way, deceptive. It is true that quasars and pulsars are described in Chapters VIII to XII, but the author's main object is to show a reader already familiar with the topic how these astronomical bodies fit into his pet theory of the universe in motion. Such ideas can be traced back to Descartes, and have more recently been discussed by various writers. Larson's theory and its relationships to astronomy are explained in Chapters I to VII. Essentially, Larson places space and time on an equal footing, relating them in an undifferentiated expansion by the 'reciprocity' formula: space/time = velocity of light. In this substrate, an oscillation represents a photon, and a rotation of the oscillation (i.e. of a photon) leads to matter and gravitation; gravitational action is instantaneous, and gravity waves are not part of physical reality. The various properties of rotation then lead to the properties of matter (electricity, magnetism, elementary particles, etc.). These general remarks give only a crude idea of the theory, which is in fact subtle and ingenious.

Larson's ideas cannot be discussed in a simple review of 'one' of his books, but some comments are in order. The theory is based on a set of postulates which are to yield all the observed and observable phenomena in the universe by pure logical deduction, without qualitative or quantitative reference to what is observed. It proposes to give a 'representation' of the whole of physical reality. Now this view of things, though adopted by such great thinkers as Eddington, Milne, and Heisenberg, has been sharply criticized by Bondi on practical grounds. Moreover, there are domains entirely unattainable (at least so far) by direct experience, such as the structure of the photon or of an elementary particle, but nevertheless used in the analaysis, which thus goes beyond physical science and deviates completely from the phenomenological trend of modern science, even if it seeks to satisfy the natural curiosity of the human mind.

Going back to quasars and pulsars, it may simply be mentioned that the author is entirely convinced of his theory's accordance with the observed facts and of its ability to remove certain difficulties which occur when the usual concept of the universe is employed.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai

M. H. P. Bott, *The Interior of the Earth*, Edward Arnold, London, 1971, 316 pp., figs., £7.50.

« The book aims to present the modern picture of the Earth's interior, including the structure and the processes which occur in it. This must be based on a synthesis of the well-established older knowledge with the newer discoveries. The physical and chemical structure of crust, mantle and core are described in the first five chapters. The last four chapters deal with processes within the Earth. The interior supply of heat is considered to be the main source of energy as described in Chapter 6. This is followed by a geometrical discussion of the large horizontal displacements which affect the ocean floor and move the continents about, and the relation of these movements to the origin of the Earth's primary surface features. The mechanism of fracture and flow which allows large horizontal and vertical movement to occur, and the primary mechanism which converts thermal energy into strain energy form the subjects of the last two chapters.

The book is mainly concerned with deep structure and mechanism and consequently details of surface geology have been kept to a minimum. The origin of the primary surface features such as ocean ridges, young fold mountains, trenches and rift valleys is described in relation to their deep structure and the ocean-floor spreading hypothesis, but detailed description of them has not been given. The origin of secondary tectonic features such as sedimentary basin subsidence has been omitted ».

In these words, the author describes the content of the book, which is of interest on account of the geophysical and geological discoveries in the last ten years which have brought about an explosion of new general ideas concerning the major problems of the earth sciences.

The author surveys within a limited compass the present state of knowledge, deliberately omitting any mathematical or geological analysis. The geophysical research techniques which together have given such a noteworthy quantity of results are critically described, with reference also to their limitations. The reader can thus see the extent to which certain results are hypothetical, and hence the problems which remain to be solved. The book is meritorious also in the balance between the various parts and in its unified and critical view of the essential features of results drawn from a literature that has expanded enormously in the last few years.

The new ideas on the spreading of the ocean floor and the drift of continents are studied as the convergence of individual studies differeing as regards both topics and methods. With the advance of knowledge of the earth's interior, phenomena are now seen to be connected which previously were studied separately or were simply inaccessible to investigation. In this way, the dynamical processes within the earth are now beginning to show beyond their complexity the outlines of an overall picture which is simple and coherent.

Pisa, Università, Istituto di Geologia e Paleontologia.

L. Trevisan

Resources and Man (A Study and Recommendations by the Committee on Resources and Man), W. H. Freeman and Company Ltd., Berkshire, 1969, xii+260 pp., figs. and tables, £1.20.

The volume is the official report of a Committee on Resources and Man, instituted in 1966 by the Academy of Sciences of the United States. The subject is covered in eight chapters, dealing separately with the themes of man's position in the eco-system, the interaction between man and his resources, the assessment of population development, the resources of energy, the food resources from the land and from the sea, and the mineral resources from the continents and the seas.

The individual subjects are discussed on a world scale, even if many appraisals originate from experiments performed in the United States. Each chapter contains an examination of the present and past situation and an appraisal of the resources for the future, assessed on the basis of the technological development to be expected, and hence of the ability to discover and utilise new sources and to increase the productive capacity of the already existing sources. Of interest are the chapters devoted to the resources of energy, food and minerals.

Other highly interesting chapters - even if they make more difficult reading, on account of the complexity of the problems - are those devoted to the assessment of population development in the United States and in the world as a whole.

The text, which is clearly of a general nature, is fully illustrated by means of accompanying data and figures, and the style, which is consistently scientific, rarely indulges in rash or unfounded extrapolatory assertions. Many of the subjects have been developed as a result of three lectures, delivered between 1966 and 1967, on problems of human ecology and of renewable and non-renewable resources. From the study emerge a series of proposals for immediate and deferred action in research, social organisation, general policy and the policy of exploitation of the resources.

Firenze, Università, Facoltà di Agraria.

G. SANESI

Errata Corrige

Un errore del quale ci scusiamo con i nostri lettori ha lievemente alterato il testo dell'articolo « Algorithmic Aspects in Complex Systems Analysis » di A. De Luca e S. Termini apparso nel fascicolo 7-8 di « Scientia ». A pag. 665, ultima riga, si leggeva « subclass of it, by means of any map w that associates with any element ». La dizione corretta era invece « subclass of it, by means of any one-to-one map w that associates to any element ».

We apologise to our readers for an error that has slightly altered the text of the article « Algorithmic Aspects in Complex Systems Analysis » by A. De Luca and S. Termini, which appeared in volume 7-8 of « Scientia ». At the last line on p.665, instead of « subclass of it, by means of any map w that associates with any element », the text should read « subclass of it, by means of any one-to-one map of w that associates to any element ».

LIBRI RICEVUTI - BOOKS RECEIVED

Le opere elencate nella rubrica "Libri ricevuti,, non vengono raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

The works listed in the section "Books received,, are not grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

ABRAGAM A. and BLEANEY B. - Résonance paramagnétique électronique des ions de transition.

Actes du XII^{e} Congrès International d'Histoire des Sciences.

ARDENNE M. v., STEINFELDER K. and TÜMMLER R. - Elektronenanlagerungs-Massenspektrographie organischer Substanzen.

BHATIA N. P. and SZEGÖ G. P. - Stability Theory of Dynamical Systems.

BLAZY P. - La valorisation des minerais.

BLEEKER C. J. and WIDENGREN G. (eds.) - Historia Religionum.

BOCKRIS J. O'M. and CONWAY B. E. (eds.) - Modern Aspects of Electrochemistry.

CARRUCCIO E. - Mondi della Logica.

CARRUTHERS P. A. - Spin and Isospin.

CHON-THÔN PHAN. - L'éthylène.

COLEMAN W. - Biology in the Nineteenth Century.

COULSON J. M. and RICHARDSON J. F. - Chemical Engineering.

COUSTEAU J.-Y. and DIOLÉ PH. - La vie et la mort des coraux.

CRAIG R. A. - Alla soglia dello spazio.

CRIBIER F. - La grande migration d'été des citadins en France.

FER F. - Thermodynamique macroscopique.

Fichtenholz G. M. - The Indefinite Integral.
Filyand M. A. and Semenova E. I. - Handbook of the Rare Elements.
Fischer H. A. and Werner G. - Autoradiographie.
Fleury P. and Mathieu J. P. - Trattato di fisica generale e sperimentale.
Florkin M. and Stotz E. H. (eds.) - Comprehensive Biochemistry.
Frajese A. and Maccioni L. (eds.) - Gli elementi di Euclide.
Friedrich H. (ed.) - Comportamento animale e umano.
Frings H. and M. - La comunicazione animale.
Fuller W. (ed.) - The Social Impact of Modern Biology.
Girard D'Albissin N. - Genèse de la frontière franco-belge.
Grant E. - Physical Science in the Middle Ages.
Haider M. (ed.) - Neuropsychologie.
Hall W. A. and Dracup J. A. - Water Resources Systems Engineering.
Hocquenghem A., Jaffard P. and Chenon R. - Mathématiques.
Hoff C. C. and Riedesel M. L. (eds.) - Physiological Systems in Semiarid Environments.
Howard R. (ed.) - Solar Magnetic Fields.
Hsu T. C. and Benirschke K. - An Atlas of Mammalian Chromosomes.
Joja Ath. - Recherches logiques.
Joja Ath. (ed.) - Recherches sur l'Organon.
Kaelble D. H. - Physical Chemistry of Adhesion.
Kordig C. R. - The Justification of Scientific Change.
Kraft V. - Mathematik, Logik und Erfahrung.
La fisica di Berkeley.
Landolt-Börnstein.
Leithäuser J. G. - La seconda scoperta del mondo.
Les apports récents des cultures et des greffes en biologie animale.
Lewis G. M. - Neutrinos.
Lipson H. S. - Crystals and X-Rays.
Marmo V.† - Granite Petrology and the Granite Problem.
Marty P., De M'Uzan M. and David C. - L'indagine psicosomatica.
Masini R. - Dall'astronomia all'astronautica.
Morris K. B. - Fundamental Chemical Equilibria.
Mostafawy A. - Pediatric Sonoencephalography.
Nebbia G. (ed.) - L'uomo e l'ambiente.
Nomura T. - Atlas of Cerebral Angiography.
Onicescu O. - Principes de logique et de philosophie mathématique.
Pollock N. C. - Studies in Emerging Africa.
Postuma J. A. - Manual of Planktonic Foraminifera.
Prodan D. - Supplex Libellus Valachorum.
Ray P. M. - La vita delle piante.
Reinert J. and Ursprung H. - Origin and Continuity of Cell Organelles.
Rompe R. and Steenbeck M. (eds.) - Ergebnisse der Plasmaphysik und der Gaselektronik.
Rosenblum L. A. (ed.) - Primate Behavior.
Rossi B. - Ottica.
Rossi P. - I filosofi e le macchine (1400-1700).

Schenkel R. and Schenkel-Hulliger L. - Ecology and Behaviour of the Black Rhinoceros.

Scientica.

Shulman A. R. - Optical Data Processing.

Smith B. L. - The Inert Gases.

Sneed J. D. - The Logical Structure of Mathematical Physics.

Steen J. B. - Comparative Physiology of Respiratory Mechanisms.

Steno N. - Lecture on the Anatomy of the Brain.

Suchet J. P. (ed.) - Appareillages et techniques de caractérisation des composés minéraux solides.

Talbot C. H. - Medicine in Medieval England.

Taylor A. W. B. - Superconductivity.

Tombes A. S. - An Introduction to Invertebrate Endocrinology.

Wallace R. H. - Understanding and Measuring Vibration.

Weitbrecht H. J. - Compendio di psichiatria.

Wenninger M. J. - Polyhedron Models.

Whitten N. E. Jr. and Szwed J. F. (eds.) - Afro-American Anthropology.

Wolff E. (ed.) - La synthèse des ARN et son rôle dans le développement primitif et la différenciation de l'embryon.

ABRAGAM, A. and BLEANEY, B., **Résonance paramagnétique électronique des ions de transition** (Series: *Bibliothèque des Sciences et Techniques Nucléaires*), Presses Universitaires de France, Paris, 1971, xx+912 pp., figs. and tables, No price stated.

Contents. - I. Introduction. *I: Introduction à la résonance paramagnétique électronique.* - II. Aspects généraux. *II: Le phénomène de résonance. - III: L'Hamiltonien de spin et le spectre. - IV: Double résonance électronique et nucléaire* (*Endor*). *- V: Le groupe del lanthanides* (*4f*). *- VI: Le groupe des actinides* (*5f*). *- VII: Ions du groupe 3d dans des champs de ligands intermédiaires. - VIII: Ions de groupes d dans des champs de ligands forts. - IX: Interaction spin-spin. - X: Interaction spin-phonon.* - III. Aspects théoriques. *XI: L'interaction Zeeman électronique. - XII: Aperçu sur la théorie des groupes. - XIII: Théorie des groupes. Le groupe des rotations. - XIV: Le groupe du cube et quelques autres groupes. - XV: Renversement du temps et dégénérescence de Kramers. - XVI: Théorie élémentaire du champ cristallin. - XVII: Structure hyperfine. - XVIII: Ions dans un champ cristallin faible* (*electrons f*). *- XIX: Champs cristallins intermédiaires* (*le groupe du fer*). *- XX: Les effets de la liaison covalente. - XXI: L'effet Jahn-Teller en résonance paramagnétique.*

Actes du XII^{e} Congrès International d'Histoire des Sciences (held at Paris in 1968), Librairie Scientifique A. Blanchard, Paris, 1970/1971, Vol. I: 432 pp., 50 F; Vol. II: 120 pp., 22 F; Vol. III/A: 152 pp., 28 F; Vol. III/B: 182 pp., 34 F; Vol. IV: 184 pp., 35 F; Vol. V: 128 pp., 25 F; Vol. VI: 134 pp., 26 F; Vol. VII: 94 pp., 18 F; Vol. VIII: 228 pp., 45 F; Vol. IX: 62 pp., 14 F; Vol. X/A: 128 pp., 30 F; Vol. X/B: 108 pp., 22 F.

L'Union Internationale d'Histoire et de Philosophie des Sciences, dans son assemblée

générale de 1965, lors du XI^e^ Congrès international d'histoire des sciences, en Pologne, avait chargé le Comité national français d'histoire et de philosophie des sciences d'organiser le Congrès suivant. En dépit des difficultés de l'année 1968, le XII^e^ Congrès a pu se tenir à Paris, au Conservatoire National des Arts et Métiers, du 25 au 31 août. Les travaux des congressistes ont été répartis entre les séances de onze sections, et les huit colloques pour lesquels des rapports avaient été demandés et imprimés avant le Congrès: - Les communications, plus de trois cents, suivent la répartition adoptée pour le Congrès. Problèmes généraux d'histoire des sciences et épistémologie; - Science et philosophie: A) Antiquité, Moyen Age, Renaissance; B) XVII^e^ et XVIII^e^ siècles; - Histoire des mathématiques et de la mécanique (depuis l'Antiquité); - Histoire de la physique (y compris l'astronomie), XIX^e^ et XX^e^ siècles; - Histoire de la chimie depuis le XVIII^e^ siècle; - Histoire des sciences de la Terre; - Histoire des sciences naturelles et de la biologie; - Histoire des sciences de l'homme; - Histoire des techniques et des instruments scientifiques; - Sciences et sociétés. Relations, Influences, Ecoles; - Monographies de savants.

ARDENNE, M. v., STEINFELDER, K. and TÜMMLER, R., **Elektronenanlagerungs-Massenspektrographie organischer Substanzen,** Springer-Verlag, Berlin, 1971, viii+404 pp., figs. and tables, DM 68.

CONTENTS. - *I: Einleitung. - II: Grundlagen der EA-Massenspektrographie. - III: Beschreibung des EA-Massenspektrographen normaler Ausführung. - IV: Untersuchungstechnik. - V: Massenspektrum der negativen Ionen. - VI: Vergleich von EA-Massenspektren negativer und positiver Ionen. - VII: Anwendung der EA-Massenspektrographie. - VIII: Systematik der untersuchten Substanzen. - IX: Literaturverzeichnis. - X: Tabellen. - XI: Autorenverzeichnis. - XII: Substanzverzeichnis. - XIII: Sachverzeichnis.*

BHATIA, N. P. and SZEGÖ, G. P., **Stability Theory of Dynamical Systems,** Springer-Verlag, Berlin, 1970, xii+226 pp., figs., DM 58.

This book contains a systematic exposition of the elements of the theory of dynamical systems in metric spaces with emphasis on the stability theory and its application and extension for ordinary autonomous differential equations. The book could serve as a suitable text for courses and seminars in the theory of dynamical systems at the advanced undergraduate and beginning graduate level, in mathematics, physics and engineering. The Authors have included most of the important new results and developments of the past 20 years. The extensive bibliography at the end should enhance the usefulness of the book to those interested in the further exploration of the subject.

BLAZY, P., **La valorisation des minerais** (Manuel de minéralurgie), Presses Universitaires de France, Paris, 1970, 416 pp., figs. and tables, No price stated.

CONTENTS. - I. La préparation des minerais. *I: La fragmentation des solides. - II: Le criblage ou classement direct. - III: La classification par sédimentation ou classement indirect.* - II. Les méthodes d'enrichissement. *I: Les méthodes basées sur la gravité. - II: Les méthodes basées sur le magnétisme. - III: Les méthodes basées sur l'électrostatique. - IV: La flottation et l'étude des interfaces. - V: Les réactifs de flottation. - VI: La cinétique du phénomène de flottation. - VII: Les machines de flottation et les circuits. - VIII: Les séparations par flottation. - IX: L'hydrométallurgie. - X: Les traitements microbiologiques.* - Annexe 1: *Les critères de contrôle et les représentations graphiques des opérations de traitement.* - Annexe 2: *Tableau de conversion des mesures anglaises.*

BLEEKER, C. J. and WIDENGREN, G. (eds.), **Historia Religionum** (Handbook for the History of Religions), VOLUME I: **Religions of the Past,** VOLUME II: **Religion of the Present,** E. J. Brill, Publisher, Leiden, 1969/1971, viii+692 and vi+716 pp., guilders 120 each volume.

In preparing this book the editors have attempted to present the religions of the world in such a way that the reader can easily find where structural similarities exist, and, in order to achieve this aim, have asked each contributor to follow the same scheme in writing his article. The basic scheme followed by the contributors is: First, a short

description of the essence of the religion; then, its historical development; conception of the deity; worship; conception of man; subsequent influence (for religions of the present: present religious situation); short history of the study of the religion; and a select bibliography. The handbook covers all religions of any importance in the world. The first volume contains contributions concerning the religions of the past, and the second volume those dealing with the religions of the present.

BOCKRIS, J. O'M. and CONWAY, B. E. (eds.), **Modern Aspects of Electrochemistry,** No. 5, The Butterworth Group, London, 1969, XII+498 pp., figs. and tables, £8.80.

CONTENTS. - *I: Hydration Effects and Thermodynamic Properties of Ions,* by J. E. DESNOYERS and C. JOLICOEUR. - *II: The Metal-Gas Interface,* by R. A. ORIANI and CH. A. JOHNSON. - *III: Potentials of Zero Charge of Electrodes,* by R. S. PERKINS and T. N. ANDERSEN. - *IV: The Study of Simple Consecutive Processes in Electrochemical Reactions,* by A. BEWICK and H. R. THIRSK. - *V: Mechanistic Analysis of Oxygen Electrode Reactions,* by A. DAMJANOVIC.

CARRUCCIO, E., **Mondi della Logica** (Series: *Matematica Moderna,* No. 9), Zanichelli Editore, Bologna, 1971, 152 pp., figs. and tables, L. 1.000.

Questo è un libro di matematica, anche se include argomenti comuni ad altri campi di indagine e a quel complesso di discipline tradizionalmente noto come filosofia: il tema trattato è assai vicino ad altri, quali i fondamenti della matematica, la teoria degli insiemi, le strutture matematiche. Il libro traccia un profilo, storico e sistematico insieme, dei principali orientamenti emersi via via nella storia della logica; presenta il calcolo delle proposizioni, il calcolo delle classi, il calcolo dei predicati, le logiche polivalenti; contiene, fra l'altro, un breve profilo storico iniziale, un riassunto della logica aristotelica e della sillogistica, un intermezzo che tratta delle relazioni tra logica matematica e algebra astratta, un finale che tocca la discussione, in primo piano nella cultura contemporanea, su simbolismi, linguaggi e metalinguaggi. Nel corso del libro vengono proposti problemi che il lettore è invitato a risolvere e che vengono in parte ripresi e discussi in fondo al volume.

CARRUTHERS, P. A., **Spin and Isospin** (In Particle Physics), Gordon and Breach, London, 1971, x+258 pp., figs., £8.10.

This book reviews useful theoretical methods for the description of the spin and isospin coordinates of elementary particles. The representations of the Lorentz and Poincaré groups are studied and related to various kinds of wave functions for particles of arbitrary spin. An extensive discussion of the discrete symmetry transformations of charge conjugation and space and time inversions is given for fields carrying internal symmetries. Applications are made to typical strong interaction problems such as crossing, structure of vertex functions, invariant amplitudes and so on. The book is primarily intended for graduate students specializing in elementary particle physics.

CONTENTS. - *I: The Four-Dimensional Orthogonal Group SO(4).* - *II: The Homogeneous Lorentz Group, I. Basic Ideas. Irreducible Representations.* - *III: The Homogeneous Lorentz Group, II. Spinor Calculus and Relativistic Wave Equations.* - *IV: The Poincaré Group, I. Representations and States.* - *V: The Poincaré Group, II. Fields.* - *VI: Internal Symmetries, Antiparticle Conjugation and Space-Time Inversion.* - *VII: Crossing Properties of Scattering Amplitudes and Vertices.* - *VIII: Projection Operators, Generalized Isospin Matrices and Crossing Symmetry.* - *IX: Spin Structure of Vertex Functions.* - *X: Invariant Amplitudes and Kinematic Properties of Two-Body Reactions.* - Appendix A: *Notation and other Conventions.* - Appendix B: *Properties of the Functions* $d^{j}_{m'm}(\theta)$.

CHON-THÔN PHAN, **L'éthylène** (Métabolisme et activité métabolique), (Series: *Monographies de Physiologie Végétale,* No. 8), Masson & C.ie, Paris, 1971, 130 pp., figs. and tables, 65 F.

CONTENTS. - I. Historique. - II. Dosage de l'éthylène. - III. Physiologie de la production d'éthylène. *I: Production d'éthylène par les fruits. - II: Production d'éthylène par les fleurs. - III: Production d'éthylène par les organes végétatifs. - IV: Production d'éthylène par les graines et les germinations. - V: Production d'éthylène par les micro-organismes. - VI: Conclusion.* - IV. Origine métabolique de l'éthylène. *I: Glycolyse et production d'éthylène. - II: Composés sulfhydrylés. - III: Formation de l'éthylène à partir des acides gras. - IV: Dégénérescence cellulaire et production d'éthylène. - V: Formation de l'éthylène accompagnant la fixation biologique de l'azote. - VI: Discussion.* - V. Activité physiologique de l'éthylène. *I: Effets de l'éthylène sur les fruits. - II: Effet de l'éthylène sur les fleurs. - III: Effet de l'éthylène sur les organes végétatifs.* - VI. Mode d'action de l'éthylène. *I: L'éthylène est-il absorbé par les tissus végétaux? - II: Sur quoi l'éthylène exerce-t-il son action? - III: Comment la molécule d'éthylène s'attache-t-elle aux substances qu'elle active?*

COLEMAN, W., **Biology in the Nineteenth Century** (Problems of Form, Function, and Transformation), (Series: *History of Science*), John Wiley & Sons, Inc., London, 1971, x+188 pp., figs., £3.50.

CONTENTS. - *I: Biology. - II: Form: Cell Theory. - III: Form: Individual Development. - IV: Transformation. - V: Man. - VI: Function: The Animal Machine. - VII: The Experimental Ideal.* - Bibliography. Index.

COULSON, J. M. and RICHARDSON, J. F., **Chemical Engineering,** VOLUME 3: Edited by J. F. RICHARDSON and D. G. PEACOCK, Pergamon Press, Oxford, 1971, XIV+624 pp., figs. and tables, £4.90.

In this book the coverage of chemical engineering is brought up to date with an emphasis on the design of systems in which chemical and even biochemical reactions occur. It includes chapters on adsorption, on the general principles of the design of reactors, on the design and operation of reactors employing heterogeneous catalysts, and on the special features of systems exploiting biochemical and microbiological processes. Many of the materials which are processed in chemical and biochemical reactors are complex in physical structure and the flow properties of non-Newtonian materials are therefore considered worthy of special treatment. Parallel with the growth in complexity of chemical plants has developed the need for much closer control of their operation, and a chapter on process control is therefore included.

CONTENTS. - *I: Reactor Design-General Principles. - II: The Design of Catalytic Reactors. - III: Process Control. - IV: Computers and Methods for Computation. - V: Biochemical Reaction Engineering. - VI: Non-Newtonian Technology. - VII: Sorption Processes.*

COUSTEAU, J.-Y. and DIOLÉ, Ph., **La vie et la mort des coraux,** Flammarion, Paris, 1971, 302 pp., figs. and coloured plates, No price stated.

Le Commandant Jacques-Yves Cousteau, pionnier de la vie sous-marine, raconte les aventures qu'il a vécues avec son équipe de la Calypso parmi les récifs de la mer Rouge et de l'océan Indien. Il décrit le monde redoutable et fascinant des coraux qui occupe dans les mers une étendue grande comme vingt fois l'Europe. Dans les eaux des Tropiques, encore à peine explorées, la vie se déploie sous d'innombrables formes inconnues sur terre. Des animaux aux couleurs intenses, diaprées, étincelantes, agitent leurs tentacules, accrochent leurs ventouses, lancent leur venin tandis que passent des poissons rayés, tachetés, pailletés: poissons docteurs armés d'un scalpel, poissons perroquets mangeurs de corail, poissons papillons, poissons anges, et les requins qui sont partout aux aguets.

CRAIG, R. A., **Alla soglia dello spazio** (L'esplorazione dell'alta atmosfera), (Series: *Biblioteca di Monografie Scientifiche*, No. 38), Zanichelli Editore, Bologna, 1971, 144 pp., figs., tables and plates, L. 1.000.

La « soglia dello spazio », cui è dedicato questo volume, è l'alta atmosfera, cioè a vastissima regione che comincia a circa 10.000 metri di altezza, dove cessa la nostra

familiare atmosfera, sede dei comuni fenomeni meteorologici, e di lì si estende per centinaia di chilometri, sfumando gradualmente nello spazio interplanetario. In questa regione la composizione dell'atmosfera, la sua densità, la temperatura, e altre proprietà fisiche sono completamente diverse da quelle per noi consuete. In questa regione rientra la ionosfera, che tanta importanza ha nelle radiocomunicazioni; qui si accendono le luci delle aurore boreali, soffiano venti intensi dovuti a un fenomeno simile alle maree oceaniche; ai confini di queste regioni soffia il « vento solare ». Nel presentare questo insolito mondo il libro spiega con quali strumenti esso viene esplorato - osservazioni da terra, radar, palloni, razzi, nubi artificiali di sodio, satelliti, ecc. - e illustra l'interesse pratico che possono avere queste ricerche.

CRIBIER, F., **La grande migration d'été des citadins en France** (Series: *Mémoires et Documents*), Editions du Centre National de la Recherche Scientifique, Paris, 1969, 404 pp., figs., tables, plates and a pamphlet with coloured maps, 64 F.

CONTENTS. - I. Les départs de vacances d'été: Un phénomène de masse. *I: L'évasion urbaine de l'été: des origines au phénomène de masse. - II: La structure des départs en vacances des citadins. - III: Le grand rythme saisonnier de la vie urbaine. - IV: La répartition de la population en France pendant l'été.* - II. Etude régionale des migrations de vacances. *I: La migration d'été des citadins du midi méditerranéen. - II: La migration d'été des citadins du sud-ouest. - III: La migration d'été des citadins du nord-ouest. - IV: La migration d'été des citadins du centre et du centre-est. - V: La migration d'été des citadins du nord et du nord-est. - VI: La migration d'été des parisiens en France.* - III. Essai de synthèse: Une géographie des migrations de vacances estivales des français en France. *I: Les aires d'exode estival des villes françaises. - III: L'habitat estival des vacanciers en France. - III: Les régions de vacances et leurs populations estivales en France.* - Conclusion. Annexes.

FER, F., **Thermodynamique macroscopique,** VOLUME 1 (Series: *Cours et Documents de Chimie*), Gordon and Breach, London, 1970, XII+288 pp., figs., No price stated.

CONTENTS. - I. Fondements de la thermodynamique des systèmes fermés. *I: Notions de base. - II: Principe de la conservation de l'énergie (premier principe de la thermodynamique) pour les systèmes fermés. - III: Réversibilité et irréversibilité. - IV: Second principe de la thermodynamique pour les systèmes fermés. Transformations réversibles. - V: Second principe de la thermodynamique pour les systèmes fermés. Transformations irréversibles. - VI: Principe de Nernst. - VII: Variables et fonctions thermodynamiques usuelles. - VIII: Stabilité de l'équilibre.* - II. Applications. *IX: Etude des gaz simples (phase chimiquement inerte). - X: Définition des échelles thermodynamiques de températures. - XI: Théorèmes généraux de la thermodynamique chimique. - XII: Exemples d'application de la thermodynamique chimique.*

FICHTENHOLZ, G. M., **The Indefinite Integral** (Series: *The Pocket Mathematical Library*, No. 6), Gordon and Breach, London, 1971, VIII+140 pp., £5.25.

The present volume of The Pocket Mathematical Library constitutes a concise treatment of indefinite integration, the operation which is the « inverse » of differentiation. The key topic of definite integration is treated in a second volume, called *The Definite Integral.* Together the two volumes give a detailed treatment of elementary integration theory. Further topics in integral calculus (e.g., improper integrals, parameter-dependent integrals, the use of integration to determine length, area and volume) will be pursued in subsequent volumes of the series. There are numerous problems, many equipped with hints and answers.

FILYAND, M. A. and SEMENOVA, E. I., **Handbook of the Rare Elements,** VOLS. II and III, Macdonald & Co. (Publishers) Ltd., London, 1970; Vol. II: XIV+418 pp., figs. and tables, £7.50; Vol. III: XVI+304 pp., figs. and tables, £8.25.

These are the second and the third of three volumes which together collate for the first time all the numerous theoretical and experimental data now extant on the rare

elements. The information has been culled from over 500 different monographs and journals, and is presented on some 1,300 tables, with descriptive text kept to a minimum. Both physical and chemical properties are covered. Each table includes a bibliographical reference for every piece of data printed, and a complete key to the references appears at the end of each volume.

FISCHER, H. A. and WERNER, G., **Autoradiographie,** Walter de Gruyter & Co., Berlin, 1971, x+214 pp., figs. and tables, DM 42.

CONTENTS. - *I: Einleitung. - II: Physikalische Grundlagen. - III: Markierungen mit radioaktiven Isotopen. - IV: Die Autoradiographie makroskopischer Objekte. - V: Die Autoradiographie mikroskopischer Objekte. - VI: Die Autoradiographie elektronenmikroskopischer Objekte. - VII: Die Autoradiographie als quantitative Methode. - VIII: Appendix. - IX: Literaturverzeichnis. - X: Sachregister.*

FLEURY, P. and MATHIEU, J. P., **Trattato di fisica generale e sperimentale,** VOL. 9: **Esercizi di fisica,** Zanichelli Editore, Bologna, 1970, VIII+316 pp., figs. and tables, L. 6.000.

Ispirato al classico trattato di J. Lemoine ed A. Blanc, quello di P. Fleury e J. P. Mathieu ne costituisce un aggiornamento. Anch'esso si propone il medesimo compito: 'esporre i principi fondamentali della fisica per via sperimentale e concreta'. L'esposizione della materia fa perno su descrizioni sperimentali atte sia ad introdurre le 'leggi' sia ad illustrarne gli aspetti più vari. Le nozioni di matematica necessarie per la comprensione della materia dell'intero trattato non superano quelle dei corsi propedeutici delle facoltà scientifiche; e le cognizioni di cui dispongono gli alunni degli istituti medi superiori ad indirizzo scientifico sono già sufficienti per la lettura di gran parte del testo. Di molte questioni riguardanti la fisica moderna, non aggredibili che con strumenti matematici superiori, è dato pertanto, in quest'opera, solo qualche cenno intuitivo e, comunque, sempre di natura descrittiva. Largo uso è fatto, viceversa, del linguaggio e dei metodi espressivi e sintetici della matematica usuale. E, per i lettori cui questa sia meno familiare, sono richiamate, là dove più vivo possa sentirsene il bisogno, le nozioni essenziali dell'analisi matematica e della geometria. Le notazioni ed i simboli adottati sono, salvo rare eccezioni, conformi a quelli raccomandati dalla Società Internazionale di Fisica. Il sistema di unità di misura è quello MKSA (Giorgi); sono però definite anche le altre principali unità di misura attualmente d'uso comune, e per ciascuna di esse sono dati i fattori di conversione con le unità Giorgi. L'opera è resa più maneggevole dalla divisione della materia in otto volumi, ciascuno dei quali può essere consultato indipendentemente dagli altri.

FLORKIN, M. and STOTZ, E. H. (eds.), **Comprehensive Biochemistry,** VOLUME 18S: **Pyruvate and Fatty Acid Metabolism,** Elsevier Publishing Company, Amsterdam, 1971, x+116 pp., figs. and tables, Dfl. 32.50.

This supplementary volume to Section IV on Metabolism contains two essential chapters which were not available to the Editors earlier. The first, entitled "The Pyruvate Dehydrogenase Complex and the Citric Acid Cycle" by J. M. Lowenstein was originally planned for Volume 17 on Carbohydrate Metabolism, and the second, entitled "Fatty Acid Metabolism" by Salih J. Wakil and E. M. Barnes Jr., was planned for Volume 18 on Lipid Metabolism. The chapter by Lowenstein discusses individual reactions in the pathway from pyruvate to acetyl-CoA, and a particularly thorough treatment of the molecular architecture of the pyruvate dehydrogenase complex. Various aspects of control within this complex and comparative behaviour of the complex in different tissues are included. Chapter II by Wakil and Barnes on Fatty Acid Metabolism covers first the general β-oxidation of fatty acids in mitochondria, with step by step description of the individual enzymes involved. The second general section on fatty acid biosynthesis describes our present understanding of acetyl-CoA carboxylase followed by treatment of fatty acid synthetases and properties of the acyl carrier protein.

FRAJESE, A. and MACCIONI, L. (eds.), **Gli elementi di Euclide** (Series: *Classici della Scienza*), Unione Tipografico-Editrice Torinese, Torino, 1970, 1046 pp., figs. and plates, L. 14.000.

Questa nuova traduzione italiana degli « Elementi » di Euclide (un'altra, curata da F. Enriques, apparve fra il 1924 e il 1935) fa parte della collezione dei Classici della Scienza diretta da Ludovico Geymonat. Il testo seguito è quello edito da Heiberg e Menge (Lipsia, 1883-1916) ed è corredato da abbondanti note intese a facilitarne la comprensione, altrimenti non sempre agevole. Il volume comprende una introduzione di A. Frajese, una premessa del traduttore L. Maccioni e un esauriente corredo di notizie biografiche e bibliografiche.

FRIEDRICH, H. (ed.), **Comportamento animale e umano** (Series: *Universale Economica*), Feltrinelli Editore, Milano, 1971, 136 pp., figs., L. 700.

La ricerca comportamentistica comparata, un tempo limitata all'ambito della fisiologia sensoria del mondo animale, è venuta acquistando negli ultimi tempi un rilievo sempre maggiore per l'antropologia e la psicologia umana. È emerso anzi che determinati modi di comportamento e determinate reazioni non sono peculiari delle specie animali, ma condizionano anche l'adattamento dell'uomo all'ambiente che lo circonda. Comportamento sociale, comportamento aggressivo, sensibilità a determinati stimoli, omissioni e azioni mancate si possono ricondurre, nell'uomo come negli animali, a identici o simili presupposti sensorio-fisiologici. Questo libro raccoglie scritti di insigni studiosi del comportamento concernenti il fondamentale rapporto tra comportamento animale e comportamento umano nei suoi molteplici aspetti.

CONTENTS. - *I:* N. TINBERGEN, *Lotta e intimidazione nel mondo animale.* - *II:* K. LORENTZ, *Osservazioni sulle taccole.* - *III:* K. LORENTZ, *La formazione delle coppie nei corvi imperiali.* - *IV:* B. GRZIMEK, *Sulla psiche del cavallo.* - *V:* K. VON FRISCH, *I pasti degli animali.* - *VI:* E. VON HOLST, *Il volo animale e la tecnica del volo nell'uomo.* - *VII:* S. DIJKGRAAF, *Breve viaggio nel mondo dei sensi degli animali.* - *VIII:* H. AUTRUM, *Fin dove possono arrivare gli organi sensori degli animali?* - *IX:* E. VON HOLST, *Il fisiologo e l'animale da laboratorio.* - *X:* H. AUTRUM, *L'uomo come rete d'informazioni.* - *XI:* O. KOEHLER, *Prefigurazioni di mezzi espressivi umani nel mondo animale.* - *XII:* O. KOEHLER, *Il pensiero senza linguaggio.* - *XIII:* O. KOEHLER, *Caso, necessità e piano nel mondo vivente.*

FRINGS, H. and M., **La comunicazione animale** (Series: *Universale Scientifica*, No. 57), Boringhieri Editore, Torino, 1971, 222 pp., figs. and plates, L. 1.500.

Il mondo della comunicazione animale non cessa di richiamare l'attenzione di ogni ordine di studiosi, e Hubert e Mable Frings, attingendo alla loro esperienza di naturalisti, espongono in quest'opera i risultati raggiunti in questo campo della scienza. Lo scambio di informazioni svolge un'importante funzione in ogni aspetto della vita degli animali - dalla ricerca del cibo alla riproduzione - ad ogni livello di complessità della scala zoologica, non esclusi gli organismi unicellulari, ed è altrettanto importante nell'evoluzione e nella differenziazione delle specie. Un'analisi sistematica, sia pure elementare e resa di facile lettura come in questo volume, mostra che la comunicazione animale è la chiave per passare dagli organismi individuali alle strutture di gruppo e che ogni fenomeno sociale deriva e prende forma dai processi di interazione tra gli individui. Interazione complessa che coinvolge vari e differenti individui della stessa specie, di specie affini, di specie nemiche, di specie indifferenti.

FULLER, W. (ed.), **The Social Impact of Modern Biology,** Routledge & Kegan Paul, London, 1971, VIII+256 pp., figs., 50p.

Discoveries in modern biology can radically change human life as we know it. As our understanding of living processes, such as inheritance, grows so do the possibilities of applying these results for good and evil, such as the treatment of disease, the control of ageing, the control of behaviour and genetic engineering. In this book, based on discussions at a conference organized by the British Society for Social Responsibility in Science in November 1970, these discoveries and their implications are discussed by some of the world's leading biologists.

CONTENTS. - *I: Science, Technology and Values.* - *II: Molecular Genetics.* - *III: Human Genetics and Reproduction.* - *IV: Immunology and Cancer.* - *V: Agricultural Botany and the Environment.* - *VI: Science in Society.*

GIRARD D'ALBISSIN, N., **Genèse de la frontière franco-belge** (Les variations des limites septentrionales de la France de 1659 à 1789), Editions A. & J. Picard, Paris, 1970, 436 pp., figs. and maps, 125 F.

CONTENTS. - *I: La frontière du Nord au lendemain du traité des Pyrénées. - II: La situation militaire et économique en Flandre et en Hainaut, après la paix d'Aix-la-Chapelle. - III: De Nimègue à la fin du siècle. - IV: Le traité d'Utrecht. - V: Les traités de limites franco-autrichiens et franco-liégeois au XVIII*e *siècle. - VI: Quelques aspects de la perméabilité de la frontière.*

GRANT, E., **Physical Science in the Middle Ages** (Series: *History of Science*), John Wiley & Sons, Inc., London, 1971, XIV+128 pp., figs., £3.50.

CONTENTS. - *I: The State of Science from 500 A.D. to 1000 A.D. - II: The Beginning of the Beginning and the Age of Translation, 1000 A.D. to 1200 A.D. - III: The Medieval University and the Impact of Aristotelian Thought. - IV: The Physics of Motion. - V: Earth, Heavens, and Beyond. - VI: Conclusion.* - Bibliographical Essay. Index.

HAIDER, M. (ed.), **Neuropsychologie** (Aktuelle Probleme), Verlag Hans Huber, Bern, 1971, 152 pp., figs. and tables, DM 22.

Die interdisziplinäre Zusammenarbeit von Ärzten und Psychologen bei der Kombination neurophysiologischer und psychologischer Untersuchungsansätze wird von Vertretern der verschiedenen Fachrichtungen dargestellt. Es wird dabei vor allem die elektrophysiologische Untersuchung der Gehirnfunktionen und deren Zusammenhang mit Bewusstseinsphänomen und Verhalten diskutiert.

CONTENTS. - *I:* H. ROHRACHER, *Geleitwort. - II:* M. HAIDER, *Einleitung. - III:* O. CREUTZFELDT, *Neurophysiologische Grundlagen des Elektroencephalogramms. - IV:* G. GRÜNEWALD, *Neuropsychologische Untersuchungsansätze. - V:* J. A. GANGLBERGER, *Die Bedeutung stereotaktischer Operationen für Hirnforschung und Neuropsychologie. - VI:* E. GROLL-KNAPP, *Computeranalyse von Hirnpotentialen während stereotaktischer Operationen. - VII:* H. LEGEWIE and W. PROBST, *Computereinsatz in der Neuropsychologie. - VIII:* N. BIRBAUMER, *Das Elektroencephalogram bei Blindgeborenen.* - Namenregister. Sachregister.

HALL, W. A. and DRACUP, J. A., **Water Resources Systems Engineering** (Series: *Water Resources and Environmental Engineering*), McGraw-Hill Book Company, Düsseldorf, 1970, XII+372 pp., figs. and tables, DM 56.70.

This book was conceived from the assumption that there are professional engineers, university faculties, and students who desire an up-to-date treatment of the analysis of water resource systems, using the techniques of operations research. This book provides a timely and comprehensive coverage of both surface water and groundwater systems and their integrated usage. Coverage includes the nature and objective functions of water resource systems; investment timing; large-scale, complex, multiple-purpose systems; groundwater systems; and water quality systems. The many examples of water resource systems provide the reader with an in-depth understanding of both the techniques used and their applications. The book forms a source of reference for professionals in all levels and phases of water resource development.

HOCQUENGHEM, A., JAFFARD, P. and CHENON, R., **Mathématiques,** Two VOLUMES, Masson & C.ie, Paris, 1970/1971; Vol. I: X+552 pp., figs., 58 F.; Vol. II: VI+522 pp., figs., 70 F.

Il s'agit d'une nouvelle édition de l'ouvrage de mathématiques des Professeurs Hocquenghem, Jaffard et Chenon. Mais pour tenir compte de l'évolution des programmes de mathématiques de l'enseignement du second degré, ces professeurs ont été amenés à réviser profondément leur enseignement du Cours de mathématiques générales du Conservatoire national des Arts et Métiers. Il en résulte que le livre correspondant a été presque entièrement réécrit. Evidemment, les connaissances exigées pour étudier ce cours sont celles enseignées dans les lycées, mais la plupart des notions sont reprises au niveau le plus élémentaire en vue de faciliter au maximum les études des élèves. Les auteurs, sans perdre de vue que les étudiants de la Promotion supérieure du Travail ne considèrent les mathématiques que comme un moyen nécessaire à la compréhension des autres sciences, ont cependant mis tout leur soin à donner des définitions précises et à exposer des raisonnements rigoureux. Car le danger dans le traitement mathématique d'un problème technique n'est pas tant la faute de calcul, dont on finit toujours par s'apercevoir, que la légère faille dans le raisonnement qui conduit à des conclusions erronées.

HOFF, C. C. and RIEDESEL, M. L. (eds.), **Physiological Systems in Semiarid Environments,** University of New Mexico Press, Albuquerque, 1969, x+294 pp., figs. and tables, $12.00.

These thirty-four articles by national and international authorities on animal and plant physiology provide contributions to specific areas of research and offer interdisciplinary study for graduate and upper-division undergraduate students. Major articles by D. B. Dill, F. W. Went, S. W. Tromp and G. E. Folk, Jr., supply the background and perspective for research reports directed toward the challenge which the semiarid environment offers to physiological systems in plants and animals. Research reports concerned with mammals include field studies of temperature regulation, kidney biology, and oxygen consumption of heart homogenates of bats; studies concerned with biological rhythms, radioisotope metabolism, and thyroid activity of ground squirrels; and studies of seasonal changes in body composition of kangaroo rats. Desert survival, exercise at altitudes, and working and living in urban complexes are among the topics in human physiology.

HOWARD, R. (ed.), **Solar Magnetic Fields** (IAU Symposium No. 43), D. Reidel Publishing Company, Dordrecht, 1971, XVI+782 pp., figs. and tables, Dfl. 140.

This volume contains the proceedings of IAU Symposium No. 43 and presents the state of our knowledge of the morphology and the physics of magnetic fields in the solar atmosphere. Among the topics discussed are: problems and interpretation concerning measurement; structure and evolution of small-scale magnetic fields; the relation to transient phenomena in solar active regions; large-scale manifestations of solar activity; and the relationship of the large-scale magnetic fields to the 11-year activity cycle. The discussion extends from subsurface layers through the photosphere, chromosphere, and the corona to interplanetary space; both radio and optical measurements of magnetic fields are presented and analyzed. Participants in the symposium represent universities and solar observatories throughout the world.

HSU, T. C. and BENIRSCHKE, K., **An Atlas of Mammalian Chromosomes,** Springer-Verlag, Berlin, Vol. I (1967): x+200 pp., figs. and plates, DM 37,60; Vol. II (1968): xx+200 pp., plates, DM 37,60: Vol. III (1969): xx+200 pp., plates, DM 37,60; Vol. IV (1970): xx+200 pp., plates, DM 54.

In recent years, because of advances in karyological techniques, we have witnessed a remarkable renewal of interest in studies of mammalian chromosomes. These techniques, generally involving the use of tissue culture, colchicine and hypotonic solution pretreatments, allow for a much clearer display of metaphase chromosomes of mammalian cells than the classic direct squash or tissue section methods. Consequently, what was known about the chromosome complement of most mammals must be revised. The findings are now extensive and scattered; they appear in numerous periodicals and newslatters, or they are kept in cytologists' file drawers without being published. It is difficult to have access to pertinent data for comparison among related species or for evaluation of various karyological characteristics within a karyotype. Such evaluations can be done only when reasonably uniform material is collected and placed side by side for comparison accompanied by relative references. This work *Atlas of Mammalian Chromosomes* is aimed to fulfill such a need. The atlas is printed in a loose-leaf form, so that each unit (the karyotype and its descriptions) can be rearranged in order to facilitate the addition of new units.

JOJA, Ath., **Recherches logiques,** Editions de l'Académie de la République Socialiste de Roumanie, Bucarest, 1971, 546 pp., Lei 35.

CONTENTS. - *I: Prolégomènes à l'histoire de la logique. - II: Philosophie de la logique. - III: Proto-histoire de la logique. - IV: La théorie de l'universel (Platon, Aristote). - V: La théorie de la modalité. - VI: La théorie de la modalité dans le* De Interpretatione. - *VII: La théorie de la modalité dans les* Analytica Priora. - *VIII: Langage naturel et langage artificiel.*

JOJA, Ath. (ed.), **Recherches sur l'Organon,** Editions de l'Académie de la République Socialiste de Roumanie, Bucarest, 1971, 174 pp., Lei 17.

CONTENTS. - *I:* D. BADAREU, *Les Catégories d'Aristote. - II:* A. M. FRENKIAN, *L'Ennéade IV, 7 de Plotin et l'édition d'Eustochius. - III:* ATH. JOJA, *Les metaxy comme objet de la recherche syllogistique. - IV:* P. BOTEZATU, *Le syllogisme aristotélique et son actualité. - V:* C. NOICA, *Note sur les formules mnémotechniques grecques pour les modes syllogistiques. - VI:* C. JOJA, *La théorie aristotélicienne de la science. - VII:* M. CONSTANTINESCU, *L'oeuvre analytique d'Aristote et le problème de ses critères. - VIII:* S. VIERU, *Sur l'axiomatisation de la syllogistique d'Aristote. - IX:* D. STOIANOVICI, *Aristotelian Categoricals and Truth-Value Gaps. - X:* A. SURDU, *Considérations sur la définition du syllogisme aristotélique dans les* Analytica Priora.

KAELBLE, D. H., **Physical Chemistry of Adhesion,** John Wiley & Sons Ltd., London, 1971, XII+508 pp., figs. and tables, £13.00.

This comprehensive treatment of adhesion phenomena covers thermodynamics, surface chemistry, polymer physics and rheology, as well as specialized topics in the mechanics of fracture. The purpose of this book is twofold: to introduce, develop, and interconnect these outwardly unrelated topics so that the broad subject of adhesion and cohesion can be analyzed; and to familiarize chemists and physicists with the basic concepts and theories currently dominating these separate fields of study. *Physical Chemistry of Adhesion* is divided into three major sections; surface chemistry, rheology, bonding and fracture. Insofar as possible, the author develops these subjects from molecular theory, which applies concepts of molecular forces and molecular structure common to both subjects. The final chapters on bonding and fracture deal directly with the phenomenological and engineering aspects of adhesion and cohesion phenomena.

KORDIG, C. R., **The Justification of Scientific Change** (Series: *Synthese Library*), D. Reidel Publishing Company, Dordrecht, 1971, XIV+122 pp., Dfl. 33.

In this book the author discusses the justification of scientific change and argues that it rests on different sorts of invariance. Against this background notions of observation, meaning, and regulative standards are considered. The author's position is in opposition to some widely influential and current views. Revolutionary new ideas concerning the philosophy of science have recently been advanced by Feyerabend, Hanson, Kuhn, Toulmin, and others. There are differences among their views and each in some respect differs from the others. It is, however, not the differences, but rather the similarities that are of primary concern here. The author argues that the proposed arguments for radical observational variance, for radical meaning variance, and for radical variance of regulative standards with respect to scientific transitions all fail; further, that these positions are in themselves implausible and methodologically undesirable. An account is sketched of the rationale of scientific change which preserves the merits and avoids the shortcomings of the approach of radical meaning variance theorists.

KRAFT, V., **Mathematik, Logik und Erfahrung,** 2nd ed. (Series: *Library of Exact Philosophy,* No. 2), Springer-Verlag, Wien, 1970, XIV+104 pp., DM 32.

Das Buch behandelt den Wissenschaftscharakter von Logik und Mathematik und die Bedingungen ihrer Anwendung in der Erfahrung, weil beide in ihrer Gültigkeit von

der Erfahrung unabhängig sind. Die Logik enthält die Beziehungen gedanklicher Ordnung. Sie ist in der Erfahrung anwendbar, weil diese Gleiches enthält und damit Allgemeinheit ermöglicht. Die Anwendung der Arithmetik beruht darauf, dass eine natürliche Zahl als eine Klasse empirischer Mengen interpretiert wird. Soweit den operativen Beziehungen zwischen Zahlen empirische Beziehungen zwischen Mengenklassen entsprechen, kann die Arithmetik in der Erfahrung angewendet werden. Die Bedingungen für die Anwendung einer Geometrie erfordern, dass in der Erfahrung Verhältnisse aufzufinden sind, die den Grundbegriffen und -beziehungen einer Geometrie substituiert werden können. Der Konventionalismus behauptet, dass diese frei wählbar sind. Das wird dadurch ausgeschlossen, dass Widersprüche mit der Erfahrung nicht durch "Exhaustion,, aufzuheben sind.

La fisica di Berkeley, VOL. II (in two parts): **Elettricità e magnetismo,** by E. M. Purcell, Zanichelli, Bologna, 1971, XVIII+520 pp., figs., L. 3.200 and L. 4.200.

L'opera si articola in 5 volumi: 1. Meccanica; 2. Elettricità e magnetismo; 3. Onde e oscillazioni; 4. Fisica quantistica; 5. Fisica statistica. Dal 1961, quando nacque l'idea di progettare un corso nuovo di fisica a livello universitario, a tutto oggi, un folto gruppo di fisici, che lavorano nei campi più attuali della ricerca, si è impegnato nella sua realizzazione. È forse proprio per la loro esperienza di ricercatori che questi uomini hanno avvertito la necessità di fornire a chi inizia lo studio della fisica le basi della conoscenza raggiunta in questo campo, introducendo le idee fondamentali, sia della relatività speciale che della meccanica quantistica e statistica, con lo spirito con cui se ne fa uso nella ricerca d'avanguardia. I complementi, i problemi, i numerosi esempi, tutti di interesse attuale, impegnano lo studente ad approfondire, ampliare e fissare gli argomenti trattati, apprendendo metodi di lavoro e acquistando la mentalità con cui il fisico utilizza quel potente mezzo che è la matematica. Il corso trova il suo completamento in un laboratorio antitradizionale che, utilizzando una strumentazione che si sviluppa via via attorno all'oscilloscopio, inizia con l'analisi del moto di particelle cariche in campi elettrici e magnetici per sfociare in esperienze di fisica quantistica e statistica, intese a evidenziarne la correlazione.

Landolt-Börnstein (Numerical Data and Functional Relationships in Science and Technology), *New Series*, GROUP III: **Crystal and Solid State Physics,** VOLUME IV: **Structure Data of Elements and Intermetallic Phases,** Springer-Verlag, Berlin, 1971, XXVIII+1020 pp., DM 620.

This volume continues the compilation of structural data on crystals started with volume III/5. It contains the data on elements and intermetallic phases published up to the end of 1967. Compiled are the space groups, the lattice constants with their dependance on temperature and pressure as well as other informations. A complete presentation of all atomic parameters was not possible within the limits of this volume, but references are given when the cited original paper contains a complete structure analysis. The substances are arranged alphabetically within each chapter. A list of mineral and common names is given at the end of the volume.

CONTENTS. - *I: Introduction.* - *II: Elements.* - *III: Borides, Carbides, Hydrides.* - *IV: Intermetallic Phases.* - *V: Alphabetical Index of Mineral and Common Names.*

LEITHÄUSER, J. G., **La seconda scoperta del mondo** (Series: *Periscopio Minore*, No. 8), Edizioni Massimo, Milano, 1969, 420 pp., plates, L. 1.800.

Le più azzardate previsioni e le più avventate fantasie di scienziati e scrittori sono state superate dagli eventi che noi viviamo: il progresso supera in velocità ogni immaginazione. Dalla macchina a vapore al motore a scoppio, ai viaggi interplanetari, dalla valvola termoionica ai cervelli elettronici, alle più recenti conquiste nel campo biologico e spaziale, l'Autore, illustra il decisivo processo storico che ha portato, quasi, ad « una seconda creazione del mondo » da parte dell'uomo. È una storia di piccoli e primordiali esperimenti e modelli, dai quali si sono sviluppate industrie d'importanza mondiale, di inventori intelligenti ed audaci, i quali nella lotta contro i pregiudizi, l'ignoranza e la paura, vincono o periscono.

Les apports récents des cultures et des greffes en biologie animale (88e Congrès de l'Association Française pour l'avancement des Sciences, Besançon, 3-8 Juillet 1969, Travaux de la Section de Zoologie), Masson & C.ie, Paris, 1971, 354 pp., figs., tables and plates, 60 F.

CONTENTS. - *Conférence inaugurale par Etienne Wolff: Embryologie et cancérologie. Essai méthodique et expérimental.* - I: Organogenèse et différenciation cellulaire: *Différenciation du pancréas, du foie, de la thyroïde, de la glande mammaire, de la glande uropygienne, des membres, de la face. Greffe nucléaire interspécifique chez les Amphibiens.* - II: Organogenèse et physiologie cellulaire: *Physiologie et métabolisme d'organes embryonnaires de Vertébrés (coeur, foie, adénohypophyse).* - III: Différenciation sexuelle: *Gamétogenèse et sexualisation chez les Coelentérés, les Némertes, les Mollusques Gastéropodes (Helix, Calyptraea, Crepidula, Vivipara), les Mollusques Céphalopodes, les Crustacés, les Insectes (Diptères, Coléoptères, Odonates Anisoptères), les Oiseaux (propriétés sécrétrices de l'épithélium germinatif; biosynthèse des hormones sexuelles; stérilisation; transfert de gonocytes ou de gonades).* - IV: Régénération; endocrinologie; pathologie: *Régénération chez les Planaires; facteurs de régénération céphalique chez les Oligochètes; régénération chez les Crustacés Isopodes; cycles d'activités endocriniennes chez les Insectes. Revue des résultats obtenus en pathologie des Invertébrés.* - V: Démonstrations.

LEWIS, G. M., **Neutrinos** (*The Wykeham Science Series*, No. 12), Taylor & Francis Ltd., London, 1970, XII+132 pp., figs., £1.50.

CONTENTS. - *I: Atoms are not Indivisible. - II: Inside the Atom. - III: Creation of the Antineutrino of β-Decay ($\bar{\nu}$). - IV: Matter and Antimatter. - V: Manifestations. - VI: Searches for Free Neutrinos of β-Decay. - VII: Spatial Symmetry and Helicity. - VIII: μ-Neutrinos (ν_μ, $\bar{\nu}_\mu$); and Some Wider Issues. - IX: Neutrino Astrophysics.*

LIPSON, H. S., **Crystals and X-Rays** (*The Wykeham Science Series*, No. 13), Taylor & Francis Ltd., London, 1970, XIV+198 pp., figs., £1.75.

CONTENTS. - *I: The Microscope. - II: X-Rays. - III: Crystals. - IV: Diffraction of X-Rays. - V: Experimental Arrangements. - VI: How Some Simple Structures were Determined. - VII: Fourier Methods. - VIII: The Fourier Explosion. - IX: Information Derived from Crystal Structures. - X: Comparison of X-Ray and Optical Diffraction. - XI: Other Radiations. - XII: Technological Uses of X-Ray Diffraction. - XIII: The Future.*

MARMO, V.†, **Granite Petrology and the Granite Problem** (Series: *Developments in Petrology*, No. 2), Elsevier Publishing Company, Amsterdam, 1971, x+244 pp., figs. and tables, Dfl. 67.50.

The present volume which is divided into two parts: « Granite Petrology » and « The Granite Problem » aims at providing a guide to the better understanding of the various theories on granite origin and to the numerous types of rock which can be considered as being granite. In order to understand the so-called « granite problem » one must first learn how geologists arrived at the conclusions, which form the basis for the existing theories. With this in mind the author reviews and summarizes the ancient and modern theoretical approaches to granite petrology in the first part of the book, devoting the second part to the definition and classification of granites based on genetical and mineralogical criteria.

MARTY, P., DE M'UZAN, M. and DAVID, C., **L'indagine psicosomatica** (Sette casi clinici), (Series: *Psicologia e Psichiatria*), Boringhieri Editore ,Torino, 1971, 284 pp., L. 5.000.

Quest'opera, frutto della collaborazione di tre autorevoli rappresentanti della scuola psicoanalitica freudiana francese, costituisce un originalissimo apporto, tanto di materiale clinico quanto di riflessione tecnica, alla formazione di quella disciplina psicosomatica che viene attualmente definendo come suo campo d'indagine e di terapia (tra medicina, psichiatria e psicoanalisi) certe forme di vita e certi modi particolari di strutturazione della personalità fra i più caratteristici del nostro tempo. Essa, attraverso la presenta-

zione di sette casi, particolarmente rappresentativi, mira a fornire uno strumento tecnico realmente appropriato alle esigenze dell'indagine. A questo fine l'opera procede come procede l'indagine stessa: presentando il materiale clinico nella forma diretta della registrazione, accompagnandolo via via con note che costituiscono un primo stadio di riflessioni, e concludendo quindi con commenti finali che propongono una strutturazione più sintetica e nel contempo un'elaborazione nozionale.

MASINI, R., **Dall'astronomia all'astronautica,** Nistri-Lischi Editori, Pisa, 1971, xiv+176 pp., figs. and tables, L. 3.000.

Il primo capitolo di questo libro può intendersi come un'introduzione sintetica dei primi anni di astronautica; il secondo e il terzo sono dedicati interamente, e anche in forma volgarizzata, a chiarire le basi fondamentali delle conoscenze acquisite sull'Universo, mentre il quarto ci riporta in questo secolo a idee più mature nel campo dell'astronomia e anche sull'origine dei pianeti, e il capitolo V ci fa conoscere i primi risultati dopo la messa in orbita dei satelliti sulle atmosfere dei pianeti. I capitoli VI e VII sono dedicati alla Terra, alle forze estraterrestri e alla Luna.

CONTENTS. - *I: Il satellite artificiale di Newton. - II: Le basi della meccanica celeste. - III: Dalle radiocomunicazioni alla radioastronomia. - IV: L'origine dei pianeti. - V: Atmosfere e temperature dei pianeti. - VI: La terra e la Luna. - VII: La terra e la Luna (continuazione). - VIII: Le esplorazioni strumentali e telecomandate. - IX: La navigazione spaziale pilotata e la radionavigazione. - X: Problemi secolari aperti. - XI: Dall'onda elettromagnetica lanciata attraverso l'Atlantico da Guglielmo Marconi alla navigazione nello spazio: dalla Terra alla Luna. - XII: L'avvenire.*

MORRIS, K. B., **Fundamental Chemical Equilibria** (Nonionic-Ionic), Gordon and Breach, London, 1971, xii+110 pp., figs. and tables, £5.25.

This book is a treatment of an important topic for students of chemistry. The treatment of oxidation-reduction equilibria and voltaic cells eliminates the confusion experienced by many students approaching the subject for the first time. Throughout the monograph examples are used which illustrate the application of principles to the solution of problems. The practice exercises which follow some of the examples in the body of each chapter are intended to aid the student in mastering the principles involved at that point in the text. Accordingly, a smaller number of problems-to-be-worked are given at the end of each chapter. This book will be of some value to students taking courses such as the following: general chemistry; introductory analytical chemistry; introductory physical chemistry; introduction to chemical engineering; and biochemistry for medical and dental students.

MOSTAFAWY, A., **Pediatric Sonoencephalography** (The Practical Use of Ultrasonic Echoes in the Diagnosis of Childhood Intracranial Disorders), Springer-Verlag, Berlin, 1971, xii+138 pp., figs. and tables, DM 78.

CONTENTS. - I. General Sonoencephalography. *I: General Physical Properties of Sound in Liquids and Gases. - II: Technical Considerations of the Ultrasonic Impulse Apparatus. - III: Examination of Pediatric Patient. - IV: Terminology. - V: The Normal Sonoencephalogram (SEG). - VI: The Abnormal Sonoencephalogram.*- II. Special Sonoencephalography. *VII: Brain Trauma. - VIII: Sonoencephalographic Examination of Childhood Hydrocephalus. - IX: Sonoencephalographic Examination in Intracranial Space-Occupying Lesions. - X: Congenial and Developmental Defects and Brain Damage in Early Childhood. - XI: Sonoencephalographic Examination in Various Convulsive Disorders. - XII: Reliability of the Sonoencephalogram in the Diagnosis of Childhood Hydrocephalus. - XIII: Sources of Error and Particulars.* - III. The Combined EEG and SEG. *XIV: The Combined Electroencephalographic and Sonoencephalographic Examination.*

NEBBIA, G. (ed.), **L'uomo e l'ambiente** (Una inchiesta internazionale). Tamburini Editore, Milano, 1971, xii+326 pp., figs. and tables, L. 5.500.

CONTENTS. - *I: Relazioni introduttive.* - *II: Le premesse culturali della crisi ecologica.* - *III: La difesa dell'ambiente è un problema globale.* - *IV: Gli effetti delle azioni umane sui vari settori della biosfera.* - *V: Le modificazioni dell'ambiente nelle società avanzate.* - *VI: Gli aspetti politici e programmatici della difesa dell'ambiente.* - *VII: L'ecologia come problema educativo.* - Appendice: *Dichiarazione del Consiglio d'Europa sull'uso del territorio e dell'ambiente umano.*

NOMURA, T., **Atlas of Cerebral Angiography,** Springer-Verlag, Berlin, 1970, XII+322 pp., figs., plates and angiograms, DM 90.

CONTENTS. - General Part. I. Introduction. - II. History. - III. Techniques. *I: Position and Anesthesia.* - *II: Cerebral Angiography through the Internal Carotid Artery.* - *III: Cerebral Angiography through the Vertebral Artery.* - *IV: Retrograde Brachial Angiography.* - *V: Side Effects.* - *VI: Technique of Radiography and Protection against X-Rays.* - IV. Normal Angiogram. *I: Internal Carotid Artery.* - *II: Anterior Cerebral Artery.* - *III: Middle Cerebral Artery.* - *IV: Vertebral Artery and its Branches.* - *V: Cerebral Veins.* - V. Abnormal Angiograms. *I: Introduction.* - *II: Changes of Siphon Carotidien and Gabel Carotidien.* - *III: Changes of the Anterior Cerebral Artery.* - *IV: Changes of the Middle Cerebral Artery.* - *V: Changes of the Posterior Cerebral Artery.* - *VI: Changes of the Profound Veins.* - *VII: Angiographic Changes Characteristic of the Nature of a Tumor.* - *VIII: Intracranial Hematoma.* - *IX: Vascular Disease.* - *X: Internal Hydrocephalus.* - Case Presentations. - Case Histories. - Special Plates.

ONICESCU, O., **Principes de logique et de philosophie mathématique,** Editions de l'Académie de la République Socialiste de Roumanie, Bucarest, 1971, 230 pp., Lei 20,50.

La première partie du livre est consacrée à la Logique mathématique à une seule valeur—celle de vérité. Elle est constituée par deux Catégories distinctes, celle épistémologique et celle ontologique, dont dérive directement l'Arithmétique. Une seconde partie est consacrée à l'étude des formes de la pensée mathématique dont l'auteur détache spécialement celles qui concernent la création des objets mathématiques, telles que le nombre naturel, les objets géométriques et les objets qui constituent la matière de la théorie de la relativité, ou de la Mécanique invariantive de l'auteur. Pour tous ces objets, la distinction entre l'objet générique et ses réalisations, entre l'aspect épistémologique et celui ontologique est mis en évidence, pour montrer le chemin qui va de l'abstrait au concret dans l'intimité de toute science.

POLLOCK, N. C., **Studies in Emerging Africa,** Butterworths, London, 1971, VIII+342 pp., figs. and tables, £5.00.

Many African states face an increasing range of problems linked with their economic growth which has often been hampered by colonial boundaries and traditions, and by post-independence nationalism and rivalries. This book describes these problems and places them in their natural and human setting. It also analyses their nature and causes and suggests possible courses of action. Subjects such as communications, agriculture, minerals, water, power and industry are dealt with systematically and regionally, with case studies of individual areas and countries. The author has included references to the political and social situation without which any study would be incomplete. Politics impinge at every point in present-day Africa, while education is also a vital force and a broadly based educational structure—that will enable people to face successfully the economic and political changes that are rapidly occurring—is an overriding necessity. In any consideration of Africa's present or future her unique heritage is also a factor which cannot be ignored.

POSTUMA, J. A., **Manual of Planktonic Foraminifera,** Elsevier Publishing Company, Amsterdam, 1971, VIII+420 pp., figs. and charts, Dfl. 90.00.

The study of planktonic Foraminifera, as a means of establishing the geological age of strata and of long-distance correlation, although a very young science, has become established as one of the most precise and reliable instruments of Cretaceous to Tertiary stratigraphy. During the last twenty years an ever-increasing flow of publications has appeared on the subject. Among the great number of new species and subspecies described, many proved to be useful guide fossils, while others turned out to be synonyms and a large number are unrecognizable by other workers in this field. Besides there being disagreement on the genetic and phylogenetic relations of the planktonic Foraminifera, authors do not hold the same views on the definitions of species. Such diverse treatment of this stratigraphically important group of Foraminifera has lead to considerable confusion and it has become almost impossible for the industrial palaeontologist to judge the published data. His job is not made easier by their being distributed over a large number of publications. The present manual contains a selection of some 160 species which are considered most important for correlation. A small percentage of these are extant species that are nowadays confined to the warmer seas around the world. Each species is profusely illustrated by highly magnified photographs and professional artist's drawings of choice specimens, both in full and in a thin section; the latter being important for the identification of planktonic Foraminifera in limestones.

PRODAN, D., **Supplex Libellus Valachorum** (The Political Struggle of the Romanians in Transylvania during the 18th Century), (Series: *Bibliotetheca Historica Romaniae*, No. 8), Publishing House of the Academy of the Socialist Republic of Romania, Bucharest, 1971, 476 pp., figs., Lei 33.

CONTENTS. - *I: Importance of the Petition. - II: Contents of the Petition. - III: The Fate of the Petition. - IV: The Political System of the Principality. - V: Origin of the Notion of « Romanian Nation » in Transylvania. - VI: The Michael the Brave Moment. - VII: The Evolution of Relations in the 17th Century. - VIII: The Union with the Roman Catholic Church. - IX: Inochentie Micu: The Pioneer of the National Political Struggle of the Romanians in Transylvania. - X: The Struggle after Inochentie Micu. - XI: The Josephine Era - Enlightened Despotism. - XII: The Uprising of Horea. - XIII: The Intellectuals and the People. - XIV: The New Socio-Political Ideas at their Source: France. - XV: The Spread of the New Ideas. - XVI: The New Ideas in the Romanian Lands. - XVII: Conclusions.*

RAY, P. M., **La vita delle piante** (Series: *Biologia Moderna*, No. 9), Zanichelli Editore, Bologna, 1971, 160 pp., figs., L. 1.000.

In quest'opera l'Autore illustra, in una rapida sintesi, le attività biologiche fondamentali del mondo vegetale. L'attenzione dello scrittore è concentrata sulle tipiche piante verdi che crescono nel terreno, vale a dire sulle piante che fioriscono, senza trascurare - però - le piante inferiori. In particolare, il libro tratta la fotosintesi, il trasporto dell'acqua e le risposte alla luce nonchè tutte quelle altre attività che si possono studiare particolarmente bene sulle piante, come la nutrizione minerale e la regolazione della crescita. Alla fine di ogni capitolo appare un elenco di letture utili a chi volesse estendere e completare l'argomento.

REINERT, J. and URSPRUNG, H., **Origin and Continuity of Cell Organelles** (Series: *Results and Problems in Cell Differentiation*, No. 2), Springer-Verlag, Berlin, 1971, XIV+342 pp., figs. and tables, DM 72.

CONTENTS. - *I: Assembley, Continuity, and Exchanges in Certain Cytoplasmic Membrane Systems*, by W. G. WHALEY, M. DAUWALDER, and J. E. KEPHART. - *II: Origin and Continuity of Mitochondria*, by R. BAXTER. - *III: Origin and Continuity of Plastids*, by W. STUBBE. - *IV: Origin and Continuity of Golgi Apparatus*, by D. J. MORRÉ, H. H. MOLLENHAUER, and C. E. BRACKER. - *V: Origin and Continuity of Cell Vacuoles*, by R. BUVAT. - *VI: Origin and Continuity of Polar Granules*, by A. P. MAHOWALD. - *VII: Centrioles*, by CH. FULTON. - *VIII: Origin and Continuity of Microtubules*, by L. G. TILNEY. - *IX: Origin and Continuity of Desmosomes*, by R. D. CAMPBELL and J. H. CAMPBELL. - *X: On Relationships between Endosymbiosis and the Origin of Plastids and Mitochondria*, by E. SCHNEPF and R. M. BROWN, JR. - *XI: Cell Organelles and the Differentiation of Somatic Plant Cells*, by F. A. L. CLOWES.

ROMPE, R. and STEENBECK, M. (eds.), **Ergebnisse der Plasmaphysik und der Gaselektronik,** VOLUME II, Akademie-Verlag, Berlin, 1971, X+446 pp., figs. and tables, DM 96.

Contents. - I. Elektrodynamik der mittleren Felder in turbulenten leitenden Medien und Dynamotheorie, by F. Krause and K.-H. Rädler. I: *Einführung*. - *II: Grundgedanken der Elektrodynamik der mittlern Felder. - III: Elementare Behandlung eines einfachen Beispieles. - IV: Allgemeine Untersuchungen über die Struktur von* $\overline{v'xB'}$. - *V: Allgemeines Verfahren zur Berechnung von* $\overline{v'xB'}$. - *VI: Korrelationstensoren für* v'. - *VII: Spezielle Ergebnisse. - VIII: Bemerkungen über die weitere Ausarbeitung der Elektrodynamik der mittleren Felder. - IX: Das Dynamoproblem. - X: Dynamowirkung einer homogenen isotropen nichtspiegelsymmetrischen Turbulenz. - XI: Grundgleichungen der mittleren Felder in rotierenden Systemen. - XII: Gleichfelddynamos. - XIII: Wechselfelddynamos. - XIV: Schlussbetrachtungen.* - II. Elektronen-Transportkoeffizienten in Gasen und Niederdruckplasmen, by J. Wilhelm and G. Wallis. *I: Einleitung. - II: Zur Theorie der Transportkoeffizienten. - III: Vergleich zwischen berechneten und gemessenen Transportkoeffizienten in Edelgasen. - IV: Transportkoeffizienten in unedlen Gasen und Gasgemischen. - V: Anhang.* - III. Impulslichtquellen, by I. S. Marschak. *I: Die Entwicklung der Entladung. Der elektrische Durchschlag des Gases bei Drücken in der Nähe des Atmosphärendruckes. - II: Elektrische Parameter des stromstarken Stadiums der Impulsentladung. - III: Die Expansion des Entladungskanals. - IV: Strahlungseigenschaften. - V: Vorgänge an den Elektroden. - VI: Technische Eigenschaften von Impulslampen.*

ROSENBLUM, L. A. (ed.), **Primate Behavior** (Developments in Fields and Laboratory Research), Volume I, Academic Press, New York, 1970, xiv+400 pp., figs. and tables, $17.50.

This is a regular publication devoted to the burgeoning field of primate behavior. The series contains reviews of the literature and extended research programs, conceptual and theoretical papers, and major individual research efforts. Its scope ranges from considerations of particular dimensions of primate behavior (e.g., learning) subjected to detailed quantitative analysis within the rigorous confines of the laboratory to the broad sweep of the behavior of a species observed under ecologically complex field conditions.

Contents. - *I: Learning Skills of Anthropoids*, by D. M. Rumbaugh. - *II: Primate Status Hierarchies*, by I. S. Bernstein. - *III: Unlearned Responses, Differential Rearing Experiences, and the Development of Social Attachments by Rhesus Monkeys*, by G. P. Sackett. - *IV: Behavior of Tree Shrews*, by M. W. Sorenson. - *V: Abnormal Behavior in Primates*, by G. Mitchell. - *VI: The Nilgiri Langur* (Presbytis johnii) *of South India*, by F. E. Poirier.

ROSSI, B., **Ottica** (Series: *Fisica pura e applicata*), Tamburini Editore, Milano, 1971, xxii+542 pp., figs., L. 9.500.

Il volume porta a conoscenza del lettore i fenomeni ottici fondamentali ed illustra i principi basilari che ne permettono l'interpretazione. È richiesta una conoscenza degli elementi fondamentali del calcolo differenziale ed integrale; in alcuni casi, in cui vien fatto uso di semplici equazioni differenziali, queste sono presentate in modo tale da risultare comprensibili anche a coloro che non hanno affrontato tale argomento nei corsi di analisi matematica. Due capitoli che trattano la teoria elettromagnetica della luce richiedono la conoscenza preventiva dei fondamenti dell'elettromagnetismo; tale conoscenza non è tuttavia richiesta per il resto del volume. Il libro è basato sull'interpretazione ondulatoria della luce. Vengono trattati successivamente differenti gruppi di fenomeni ottici, così da giungere passo per passo alla scoperta delle proprietà cinematiche e della natura fisica delle onde luminose.

ROSSI, P., **I filosofi e le macchine (1400-1700)**, (Series: *Storia della Scienza*, No. 219), Feltrinelli Editore, Milano, 1971, xii+194 pp., L. 3.000.

Questo libro affronta in particolare il problema del mutamento profondo delle ideologie che si accompagnò, fra il 1400 e il 1700, allo sviluppo della tecnica nel mondo moderno e al progressivo affermarsi della macchina. Le discussioni di Bodin, di Bruno e di Bacone sul progresso delle scienze, le polemiche di Descartes, di Mersenne e di Galileo contro il concetto aristotelico di natura e di scienza, la disputa sugli antichi e i moderni; i programmi delle prime accademie scientifiche; il progetto leibniziano di un rinnovamento del sapere; l'opera di Diderot e degli Enciclopedisti sono alcuni tra i grandi temi trattati in questo libro. Ma non minore importanza ha l'analisi, che costituisce il tessuto connettivo di tutto il volume, di testi meno noti e spesso lasciati in ombra dalle storie tradizionali della filosofia e della scienza.

SCHENKEL, R. and SCHENKEL-HULLIGER, L., **Ecology and Behaviour of the Black Rhinoceros** (*Diceros Bicornis L.*), (A Field Study), Verlag Paul Parey, Hamburg, 1969, 102 pp., figs. and tables, DM 28.

Among many other animals, man also endangers the five surviving rhino species. Only within the last decades, efforts have been made to safeguard the rhinos from extinction and only recently to study their way of life. The present monograph deals with the black rhinoceros. It describes the relevant qualities of its habitat, its ecological situation in a restricted area of Tsavo National Park East in Kenya, and more generally its behaviour and social life. As rhinos are rather solitary animals, the degree of intraspecific intolerance and aggression, and the communication systems providing for the continuity of the population are of special interest. Rhinos show a remarkable degree of adaptability; depending on their habitat, populations live in a more sedentary or more nomadic way. As is shown by the authors, the rhino's reactions are not purely innate. Some are formed by tradition within the local population, i.e. transferred from mother to calf. This is especially obvious for reactions of rhinos to man.

Scientica (Sau stiinta despre stiinta), Editura Politica, Bucuresti, 1970, 390 pp., Lei 12.

CONTENTS. - *I: Scientica - System of the Sociology, Ethics and Politics of Science. Introduction to a New Science*, by O. BĂDINA and O. NEAMTU. - *II: Science as Object of Study in the Social Sciences*, by V. STANCOVICI. - *III: The State and Science. Politics of Science. Planning of Research*, by V. KRASNASESCHI and O. BĂDINA. - *IV: The Mechanism of Research*, by V. KRASNASESCHI and I. M. NESTOR. - *V: Assessement of the Results of Research*, by I. M. STEFAN. - *VI: Dissemination of the Results of Research*, by R. VASILIU. - *VII: Science and Culture.* 1) *Politics of Science*, by C. POPOVICI. 2) *Politics of Culture*, by O. BĂDINA.

SHULMAN, A. R., **Optical Data Processing** (*Wiley Series in Pure and Applied Optics*), John Wiley & Sons, Inc., London, 1970, x+710 pp., figs., £13.00.

This work provides the background for understanding coherent optical data processing techniques, giving practical examples and results of these techniques applied to specific types of data. It is profusely illustrated with diagrams and photographs. The volume presents a thorough introduction to this new field without challenging the reader with advanced mathematical formulations early in the text. A maximum amount of physical interpretation of the basic principles leading to coherent optical processing applications is provided. State-of-the-art techniques are discussed in relation to potential applications. It begins with the basic principles of optics and the many diverse disciplines required, developing the principles of coherent optics for engineers and engineering students. The field of coherent optics is presented in depth to enable the reader to understand the current literature and participate productively in this new field.

SMITH, B. L., **The Inert Gases** (Model Systems for Science), (*The Wykeham Science Series*, No. 16), Taylor & Francis Ltd., London, 1971, XII+172 pp., figs. and tables, £2.50.

CONTENTS. - *I: The Inert Gases. - II: Atomic Models. - III: Gases. - IV: Crystal Structure and Growth. - V: Crystal Properties. - VI: Liquids. - VII: Critical Phenomena. - VIII: Optical and Dielectric Properties. - IX: Inert Gas Compounds. - X: Applications of the Inert Gases.*

SNEED, J. D., **The Logical Structure of Mathematical Physics** (Series: *Synthese Library*), D. Reidel Publishing Company, Dordrecht, 1971, XVI+312 pp., Dfl. 70.

This book is about scientific theories of a particular kind - theories of mathematical physics. Examples of such theories are classical and relativistic particle mechanics, classical electrodynamics, classical thermodynamics, statistical mechanics, hydrodynamics, and quantum mechanics. Roughly, these are theories in which a certain mathematical structure is employed to make statements about some fragment of the world. Most of the book is simply an elaboration of this rough characterization of theories of mathematical physics. It is argued that each theory of mathematical physics has associated with it a certain characteristic mathematical structure. This structure may be used in a variety of ways to make empirical claims about putative applications of the theory. Typically - though not necessarily - the way this structure is used in making such claims requires that certain elements in the structure play essentially different roles. An attempt is made to say, rather precisely, what a theory of mathematical physics is and how you tell one such theory from another - what the identity conditions of these theories are.

STEEN, J. B., **Comparative Physiology of Respiratory Mechanisms,** Academic Press, London, 1971, x+182 pp., figs. and tables, £3.50.

This work attempts to review the problems of comparative respiratory physiology in the light of current knowledge. The central theme is gas exchange, but also considered, because of the interrelationship of all functions, is how the respiratory function fits the organism as a whole. The author compares the respiratory systems of different species in terms of the general principles of gas exchange. He relates respiration to the biology and ecology of species by trying to explain why respiratory systems differ between types of animals - why, for instance, fishes have gills or mammals have lungs. Gas exchange of birds' eggs is included for the first time in a work of this nature.

CONTENTS. - *I: Principal Properties of Gas Exchange. - II: The Respiratory Environments. - III: Aquatic Gas Exchange. - IV: Transitional Breathing. - V: Aerial Gas Exchange. - VI: Respiratory Adaptations to Life at High Altitude. - VII: Secondary Adaptations of Lung Breathers to Aquatic Life. - VIII: Placental Gas Exchange. - IX: Gas Exchange of the Bird Egg. - X: The Swimbladder Function.*

STENO, N., **Lecture on the Anatomy of the Brain,** Nyt Nordisk Forlag, Copenhagen, 1965, xx+208 pp., plates, Danish crone 25,00.

CONTENTS. - *I: Preface. - II: Discours de Monsieur Stenon sur l'anatomie du cerveau. - III: A Monsieur de la Chambre. - IV: Introduction. - V: A Lecture on the Anatomy of the Brain by Nicolaus Steno. - VI: To Monsieur de La Chambre. - VII: The Royal Privilege. - VIII: Niels Stensens Rede über die Anatomie des Gehirns. - IX: Moderne Legenden zu den hirnanatomischen Zeichnungen des Discours.*

SUCHET, J. P. (ed.), **Appareillages et techniques de caractérisation des composés minéraux solides** (Séminaires de chimie de l'état solide, 1969-1970, No. 4), Masson & C.ie, Paris, 1971, VIII+212 pp., figs. and tables, 70 F.

CONTENTS. - *I:* A. AUTHIER, *Caractérisation de la qualité cristalline par topographies aux rayons X. - II:* A. DELAPALME and E. F. BERTAUT, *Caractérisation de composés et de matériaux par diffraction neutronique. - III:* P. RIGNY, *Introduction à la R.M.N. des solides minéraux; applications à certains fluorures solides. - IV:* F. MAURICE, *Nouvelles possibilités et limites des techniques de microanalyse. - V:* J. P. SUCHET and J. P. SÉNATEUR, *La résonance mossbauer. Application à l'étude de quelques composés semiconducteurs on métalliques de fer. - VI:* F. GAUME, C. LINARES, M. BLANCHARD and J. C. SOUILLAT, *Application de la fluorescence de l'ion* Eu^{3+} *à l'étude de la symétrie des composés minéraux. - VII:* P. CARO and J. DEROUET, *Relations entre les spectres d'absorption optique et de fluorescence et la structure de divers composés des terres rares. - VIII:* M. DRIFFORD and M. T. BOUCRAUT, *Spectroscopie Raman-Laser. Application à l'étude structurale des hexafluorures de sélénium et de tellure. - IX:* A. TRESSAUD and J. PORTIER, *Application des mesures magnétiques aux problèmes structuraux des fluorures des éléments de transition. - X:* P. MOLLARD, A. ROUSSET and J. PÂRIS, *Superparamagnétisme et caractérisation magnétique de la matière. - XI:* A. CASALOT, *Sur la mise en évidence de quelques changements de structure cristalline par l'étude des propriétés de transport. - XII:* M. DUCLOT and C. DEPORTES, *Dilatomètre-Conductivimètre pour l'étude de transformations ordre-désordre dans les systèmes d'oxydes à haute température. - XIII:* O. GOROCHOV and G. KELLER, *Description d'une balance magnétique verticale asservie. Etude de quelques sulfures et séléniures de terres rares.*

TALBOT, C. H., **Medicine in Medieval England,** Elsevier Publishing Company, Amsterdam, 1967, 222 pp., plates, Dfl. 20,00.

Dr. Talbot discusses a wide range of topics—medieval hygiene, public and private; plagues and pestilences; medical ethics and etiquette, and medical education in the Middle Ages; and engrossing and detailed accounts of medieval surgery. This study is a scholastic achievement, for, while it contains much of importance and interest for specialists in the history of medicine, it has been written in a style that will absorb the non-expert layman.

CONTENTS. - *I: Anglo-Saxon Medicine. - II: Arab Medicine. - III: Salerno. - IV: Montpellier. - V: Medical Education. - VI: Gilbertus Anglicus and others. - VII: Surgery. - VIII: John Gaddesden. - IX: Anatomy. - X: The Ordinary Medieval Practitioner. - XI: Medical Ethics and Etiquette. - XII: Hygiene. - XIII: Epidemics. - XIV: Hospitals. - XV: Vernacular Texts. - XVI: The Final Phase.*

TAYLOR, A. W. B., **Superconductivity** (*The Wykeham Science Series*, No. 11), Taylor & Francis Ltd., London, 1970, XII+96 pp., figs., £1.25.

CONTENTS. - *I: The Superconductive State. - II: Magnetic Properties of the Superconductive State. - III: Type 1 Superconductors. - IV: Type 2 Superconductors. - V: Nature of the Superconductive.*

TOMBES, A. S., **An Introduction to Invertebrate Endocrinology,** Academic Press, New York, 1970, XIV+218 pp., figs. and tables, $10.50.

This introductory work presents invertebrate endocrinology as a subject in itself rather than as a subdivision of vertebrate endocrinology or physiology. Of the two major divisions in the book, Part 1, comprised of Chapters 1 and 2, covers the general mechanisms and structure of the endocrine system. The second part, consisting of Chapters 3 through 11, is a discussion of the endocrine system of seven phyla. The first four — Cnidaria, Platyhelminthes, Nemertinea, and Nematoda—can be considered as the minor group with the last three phyla—Mollusca, Annelida, and Arthropoda—constituting the major group. Since the neurosecretory cell is the dominant endocrine component of the invertebrates, a limited morphological discussion of the nervous system for each phylum is necessary. Thus morphology receives initial attention in each chapter, including, when available, ultrastructural evidence to augment the existing information. A discussion of the physiological processes, i.e., reproduction, growth and development, osmoregulation, and others which have been recorded as being under neuroendocrine influence, follows the morphology presentation. It is only after the neuroendocrine elements have been localized and described histologically that a control mechanism for a physiological process can be hypothesized and tested experimentally.

WALLACE, R. H., **Understanding and Measuring Vibration** (*The Wykeham Technological Series*, No. 4), Taylor & Francis Ltd., London, 1970, XII+148 pp., figs. and plates, £1.50.

CONTENTS. - *I: Explanation of Vibration. - II: General Survey of Vibration and Measurement. - III: Typical Modes of Resonant and Self-Excited Vibrations. - IV: Forced Vibrations. - V: Coupled Vibrations. - VI: Typical Measuring Devices. - VII: Specific Transducers and their Applications. - VIII: Application of Transducers and Recording of Signals. - IX: Data Reduction, Analysis and Replay. - X: Sound Room Techniques.*

WEITBRECHT, H. J., **Compendio di psichiatria,** Piccin Editore, Padova, 1970, XX+584 pp., figs., L. 15.000.

CONTENTS. - *I: Schema della psichiatria clinica. - II: Osservazioni sulla condotta dell'esame. - III: Impiego di tests mentali. - IV: Psicopatologia generale e clinica.* - I. Personalità abnormi, reazioni e sviluppi. *I: Personalità psichicamente abnormi (psicopatiche). - II: Tipologia costituzionale. - III: Reazioni abnormi all'avvenimento e sviluppi della personalità (neurosi). - IV: Anomalie sessuali. - V: Tossicomanie. - VI: La debolezza mentale.* - II. Psicosi fondabili su base somatica. *I: Paralisi progressiva. - II: Disturbi psichici nelle affezioni vascolari del cervello. - III: Disturbi psichici nelle atrofie cerebrali. - IV: Disturbi psichici negli esiti di traumi cerebrali. - V: Le affezioni epilettiche. - VI: Le* *psicosi depressive e paranoidi dell'età involutiva.* - III. Le psicosi endogene. *I: Psicosi endogene di tipo depressivo e di tipo maniacale. - II: Psicosi endogene di tipo schizofrenico. - III: Psicosi endogene non comprese nel raggruppamento « classico » (psicosi « atipiche », « psicosi marginali », « psicosi degenerative », « psicosi miste », ecc.). Classificazione delle psicosi secondo Kleist e Leonhard.* - IV. Psicoterapia. - Appendice: Psichiatria infantile. - Problemi di psichiatria forense e di diritto assistenziale.

WENNINGER, M. J., **Polyhedron Models,** Cambridge University Press, London, 1971, XII+208 pp., figs., £5.00.

Men have been fascinated for a long time by models of the regular and semi-regular polyhedral solids. The Greeks knew the simplest of them; since their day the range of figures has extended until seventy-five are now known, called more generally 'uniform' polyhedra. Father Wenninger describes simply and carefully how models of all the known uniform polyhedra and some stellated forms may be constructed. The book is fully illustrated with drawings and photographs. This is the first book to provide practical guidance in the making of all these intricate and beautiful solids.

CONTENTS. - *I: The Convex Uniform Polyhedra: The Platonic and Archimedean Solids. - II: Some Stellations and Compounds. - III: Non-convex Uniform Polyhedra.*

WHITTEN, N. E. Jr. and SZWED, J. F. (eds.), **Afro-American Anthropology** (Contemporary Perspectives), Collier-Macmillan Limited, London, 1970, X+468 pp., figs., tables and plates, £3.00.

Twenty-two original articles present a coherent picture of the problems involved in Afro-American research and examine topics ranging from contemporary urban anthropology, ethnoscience, cultural ecology, and socio-economic evolution to ethnomusicology, patterning in linguistics, ethnic identity, and syncretic religions. To illustrate the diversity among black cultures, the first section expands existing knowledge of Afro-America by the inclusion of material not only from the United States but also from such areas as the tropical rainforests of the Pacific and Atlantic coasts of South America, the Caribbean, Central America, and Eastern Canada. The second section expands the range still further, and analyzes the socio-economic adaptations of black aggregates by providing fresh pespectives on the commonalities of the social systems throughout Afro-America. The final set of papers presents a view of the continuing field research in the urban ghettos of the United States in this time of national crisis. The editors' introduction and notes establish the historical and theoretical contexts of the selections and provide new views of the social evolution of Afro-Americans. Also included is a 32-page photographic essay by George A. Talbot which illustrates the cultural and ecological range of Afro-American societies.

WOLFF, E. (ed.), **La synthèse des ARN et son rôle dans le développement primitif et la differenciation de l'embryon** (Series: *Cours et Documents de Biologie,* No. 2), Gordon and Breach, London, 1971, XIV+152 pp., figs. and tables, No price stated.

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Sede legale - Milano, Via Donizetti 55
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FONCTION ET STRUCTURE EN LINGUISTIQUE

L'étude structurale du langage se fonde sur une analyse, opérée selon le principe de pertinence, qui permet de déterminer ce qui, dans la réalité physique observée, est proprement linguistique et d'établir un classement et une hiérarchie des faits fondés sur leur fonction respective dans la langue étudiée. Si l'on conçoit que, dans une structure, la nature de chaque élément est sous la dépendance de celle des autres, on s'aperçoit que l'évolution d'une langue peut se comprendre comme la dynamique d'une structure soumise à la pression des besoins changeants de ceux qui la parlent.

> Ne pas être raciste, c'est accepter telle qu'elle est une humanité différente, sans se croire obligé, par des " transformations ,, logiques ou autres, de la réduire à la sienne.
>
> JEANNE MARTINET

Dans une science en cours d'élaboration, comme la linguistique générale, où n'existe au départ ni base théorique universellement admise, ni même de tradition à laquelle tout le monde se réfère, les échanges sont souvent rendus difficiles et infructueux du fait de fréquents désaccords terminologiques. Sans doute s'est-on efforcé parfois de bien définir certains termes. Mais chaque école, chaque groupe l'a fait pour satisfaire ses besoins particuliers, d'où des conflits et des chevauchements qui déroutent les débutants et compliquent la tâche de ceux qui s'efforcent de préserver ou de rétablir la communication entre tous les linguistes.

La situation se complique lorsque, sous prétexte d'interdisciplinarité, on emprunte à la logique et aux mathématiques des concepts dont il n'est pas certain qu'ils correspondent aux besoins des linguistes. C'est pourtant parmi les linguistes qu'on trouve les premiers coupables. Mais, depuis que la linguistique s'est établie comme une discipline à la mode, elle voit venir à elle ceux qui ont intérêt, soit à l'intégrer à leur propre domaine, soit à brouiller les frontières qui la séparent d'autres ordres de recherche. L'offensive de la logique que représente la grammaire transformationnelle et générative a naturellement entraîné une débauche terminologique qui n'a pu qu'aggraver la situation. On clarifiera peut-être l'atmosphère en opposant nettement, à une linguistique logico-mathématique, peu soucieuse de fonder ses postulats sur l'observation des faits, la linguistique des langues, qui peut opérer déductivement, mais qui définit son objet sur la base de l'expérience acquise au contact des réalités linguistiques les plus diverses. Cette linguistique des langues, qu'on ne confondra pas avec celle de chacune des langues particulières, ne s'identifie par complètement avec ce qu'on a appelé, en linguistique, le structuralisme. Mais elle en a été et en reste une des formes les plus centrales et celle qui s'oppose le plus catégoriquement à l'offensive logico-mathématique. C'est par examen de la valeur donnée à certains termes qu'on peut, semble-t-il, en mieux cerner les contours, et c'est ce que nous tenterons de faire ici.

Ces termes sont ceux de « structure », « structural », « structuralisme », « fonction », « fonctionnel », « fonctionnalisme » dont il ne s'agit pas de faire l'étymologie, ni de déterminer quelle tendance de la linguistique contemporaine mérite le mieux de se les approprier. Ici, comme partout ailleurs, nous nous gardons d'être prescriptifs. Nous constatons simplement quels sont les usages et nous mettons en garde contre les confusions qu'on pourrait commettre du fait de l'ambiguité des terminologies.

Ce qu'on appelle actuellement, en France et ailleurs, dans les milieux non-linguistiques, le «structuralisme», dérive bien, en dernière analyse, du structuralisme linguistique. Mais le passage s'est fait dans des conditions très spéciales, à partir d'une variété très particulière de ce structuralisme. Il est le fait d'esprits spéculatifs, plutôt que de chercheurs décidés à établir un cadre théorique pour l'observation et l'analyse des faits. Il découle, en dernière analyse, de l'enseignement de Claude Lévi-Strauss qui a, très généralement, procédé par emploi métaphorique de catégories dégagées par l'observation linguistique. Cet enseignement se fondait sur celui de Roman Jakobson qui, de tous les structuralistes, est celui qui est passé le plus vite de l'observation des faits à des formulations générales. Pour opposer le « structuralisme » à la mode au structuralisme linguistique, on ne saurait mieux faire que de rappeler que le premier se pose comme anti-historique, alors que le structuralisme linguistique, celui-là même qui se place dans la tradition saussurienne, distingue soigneusement entre synchronie et diachronie, mais, dépassant Saussure, voit dans la perspective diachronique un moyen de comprendre la dynamique d'une structure soumise à la pression des besoins changeants de l'humanité.

Il n'est pas facile de préciser ce qu'ont en commun les diverses tendances et les différentes écoles qui ont reçu, historiquement, l'épithète de structuraliste. Venus d'horizons de la pensée souvent fort éloignés les uns des autres, les structuralistes pragois, danois et américains, ont en commun certaines pratiques, comme la commutation qui se fonde sur le rapprochement de segments de textes partiellement différents, comme ceux qu'on désigne, sur le plan phonologique, comme des « paires minimales ». De ce fait, le résultat des analyses est, d'une école à une autre, assez semblable pour être généralement utilisable, même si l'on doit se souvenir que les « allophones » des uns sont les « variantes combinatoires » des autres.

Les fondements théoriques sont, au départ, assez différents. Considérons, par exemple, la réalité phonique. Chez les disciples de Bloomfield, elle est considérée comme intégralement valable pour le linguiste: un phonème groupe un certain nombre de sons différents, dits « allophones » qui représentent la réalité linguistique de base. Chez les Danois, rien de la réalité phonique (ni sémantique) n'est, proprement, linguistique: seuls comptent les rapports entre les unités; là où les autres structuralistes voient une unité phonique, le phonème, les glossématiciens identifient un cénème, c'est-à-dire une « unité vide ». Chez les Pragois, seuls comptent certains aspects de la réalité phonique reconnus comme pertinents du fait de leur fonction distinctive dans la langue: un phonème est un faisceau de traits distinctifs dits, de ce fait, pertinents.

En fait, les positions se sont assouplies au cours du temps. Bloomfield lui-même a suggéré l'identification du phonème aux traits distinctifs qu'on y relève, et l'on constate, dans les derniers écrits de Hjelmslev, des concessions à la substance, phonique ou sémantique, à condition qu'elle soit « formée », c'est-à-dire qu'elle participe aux faisceaux de relation, ce qui équivaut à reconnaître le caractère linguistique des traits distinctifs. On aboutit donc, de façon plus ou moins explicite, à reconnaître le principe de pertinence comme le fondement de la linguistique structurale.

Ce principe, dont on doit la formulation à Karl Bühler, se fonde sur l'observation qu'à chaque point d'un énoncé vocal, l'auditeur, s'il connaît la langue employée, fait inconsciemment le tri entre ce qui le renseigne sur l'identité du locuteur, ce qui l'informe de son état d'esprit et de son humeur, ce qui correspond à ce que le locuteur veut communiquer au moyen de la langue dont il se sert. De ceci, on peut conclure que la réalité linguistique ne se confond pas avec la réalité phonique: il y a des éléments de l'énoncé vocal qui correspondent à des habitudes articulatoires acquises, par imitation, au cours de l'apprentissage de la langue; ces éléments caractérisent en propre la langue employée et sont identifiés comme linguistiques; des éléments comme ceux qui permettent d'identifier l'individu comme tel, son timbre de voix par exemple, ne sont pas linguistiques. La réalité physique de l'énoncé ne s'identifie donc pas avec la réalité linguistique correspondante. Sera considéré comme linguistique tout trait qui caractérise en propre une langue déterminée. Parmi les traits retenus sur ces bases, seront pertinents ceux qui contribuent à assurer la communication linguistique. Les traits phoniques pertinents seront ceux qui permettent, à eux seuls, de distinguer entre les mots et les formes. En fait, une analyse un peu poussée aboutit à un classement hiérarchique moins simpliste que celui qui oppose pertinent à non-pertinent: les vibrations de la glotte qui accompagnent la production du [n] de *net* ne sont pas distinctives puisqu'on ne saurait, en français, opposer à *net* un autre mot qui ne se distinguerait de celui-ci que par l'absence de vibrations de la glotte pendant l'articulation du [n]; mais elles ne sont pas linguistiquement indifférentes puisqu'elle donnent plus de relief à ce qui distingue le [n] de *net* du [m] de *mette*.

On voit comment l'application du principe de pertinence aboutit à fonder le classement, non sur la nature physique des faits observés, mais sur leur fonction: des faits, physiquement différents, mais de même fonction, seront classés ensemble; des faits, physiquement identiques, mais de fonctions différentes, seront placés dans des classes différentes. Ceci vaut, non seulement en phonologie, mais également sur les autres plans de la langue. Dans le domaine de l'identification des formes, le *i-* de *ira* a la même valeur que le *all-* de *allons,* mais le *all-* de *allons* n'a rien à voir avec celui de *all-ège.* En ce qui concerne la position respective des éléments de la chaîne parlée, la position de *chat* et de *rat* est pertinente dans *le chat a mangé le rat,* mais celle de *la fleur* et de *s'épanouisse* est non pertinente dans *pour que s'épanouisse la fleur* (*pour que la fleur s'épanouisse*).

Toute science se fonde naturellement sur une pertinence, car aucune ne saurait prétendre épuiser, à elle seule, la description d'un objet quel qu'il soit: un même objet sera soumis à des traitements différents par le physicien, le chimiste ou le géomètre. L'histoire des sciences montre que les pertinences s'établissent assez naturellement tant que n'intervient pas l'activité de l'homme dans la production de l'objet d'étude.

Lorsqu'il s'agit, au contraire, d'examiner scientifiquement l'activité de l'être humain ou l'utilisation par lui de la réalité physique à certaines fins, on ne peut plus s'abstraire de ses intentions, et ce sont ces intentions qui vont fonder la pertinence. Ceci ne veut pas dire que l'étude va consister à s'enquérir auprès du sujet de ses intentions, mais bien à dégager, par l'examen des comportements, les identités fonctionnelles derrière la variété des situations directement observables. Soit, par exemple, tout un lot de clefs dissemblables par le choix de leur substance ou la facture de leur anneau. Un classement fonctionnel aboutira à ranger ensemble ou à accrocher à un même clou, toutes celles qui ouvrent une même porte, qu'elles soient de fer ou de cuivre, d'anneau simple ou orné. Ceci parce qu'on estime que les clefs sont faites pour fermer et ouvrir les portes. Rien n'empêcherait toutefois d'adopter une autre pertinence, une pertinence esthétique par exemple, si, les serrures correspondantes ayant disparu, la fonction la plus probable des clefs en question était d'ornement. Tout ceci vaudra pour un comportement humain, tel le comportement linguistique: la pertinence qui, dans ce cas, s'impose tout d'abord, est la pertinence communicative et c'est elle que les linguistes retiennent. Mais on peut fort bien imaginer un autre type de pertinence, une pertinence esthétique, par exemple, valable pour les chanteurs d'opéra dont il importe peu qu'on comprenne ce qu'ils chantent.

L'application du principe de pertinence permet de déceler, derrière l'apparence des comportements humains, leurs véritables raisons d'être, et de faire, de ces comportements, une analyse qui n'est guidée ni par les *a priori* de l'observateur, ni par la façon dont les faits observés semblent s'organiser à première vue. On retrouve ici la distinction que fait l'anthropologie américaine entre ce qui est *overt*, ce qui tombe directement sous le sens, et ce qui est *covert*, c'est-à-dire non évident, mais qui représente, en fait, la véritable motivation du comportement examiné. C'est pour ne pas s'être avisé d'opérer avec le principe de pertinence que bien des linguistes se sont laissé prendre au piège de l'opposition entre une structure superficielle et une structure profonde qui, surmontée par des « transformations », permettrait de réduire à l'unité la variété des langues. La pertinence permet, elle aussi, de percer au-delà des différences et des analogies de surface, mais pour retrouver, non plus une identité hypothétique, mais le fonctionnement réel et la structure spécifique de chaque langue. C'est là un moyen sûr de ne pas céder à l'impérialisme culturel qui, au nom de l'unité de l'humanité, cherche à faire entrer de force, dans le cadre étroit des habitudes et des préjugés d'un seul groupe, toute la variété des actions et des productions de l'homme.

La pertinence permet de fonder la seule étude véritablement fonctionnelle des comportements humains et, parce qu'on peut et qu'on doit soumettre le même objet à des analyses fondées sur différentes pertinences, le fonctionnalisme se révèle alors, non plus ce qu'on l'accuse d'être, c'est-à-dire un moyen d'éliminer de la vie tous les agréments et tous les raffinements, mais un cadre où l'on peut faire, de tout agrément et de tout raffinement, la source d'une pertinence particulière: si, à la base de tout jugement porté sur l'architecture, on doit poser comme pertinente la fonction de protection de l'homme contre les intempéries, il est facile de constater qu'il s'est vite greffé sur cette pertinence fondamentale une autre pertinence, celle d'une fonction « impressive » qui vise à inspirer, par les dimensions et la magnificence de l'édifice, l'admiration, le respect et la crainte.

C'est l'appartenance à une même pertinence qui établit, entre les traits composants d'un type de comportement ou de produits de l'activité humaine, cette cohésion qu'on constate lorsqu'on déclare qu'ils forment une structure. Dans les langues, comme ailleurs, c'est la communauté de fonction qui fonde la structure.

Etre structuraliste, en linguistique, implique nécessairement, semble-t-il, qu'on considère qu'une langue est une structure. On pourrait arguer qu'il y a dans cet emploi du terme une extension métaphorique du type de celles dont on doit toujours se défier en matière scientifique. Toutefois, si emploi métaphorique il y a, on constate que cet emploi est fort ancien puisqu'en latin même, le mot *structura* avait, à partir du sens premier d'arrangement des pierres dans une maçonnerie, ceux d'arrangement des os dans le corps et même d'arrangement des mots dans le discours: *verborum quasi structura*.

Dans les usages contemporains, on note tout d'abord des emplois du terme en biologie et en psychologie, par exemple, avec une valeur statique pour désigner l'ensemble des éléments de base, indépendamment et par opposition à la dynamique de leur fonctionnement. Cela n'est pas sans rappeler le synchronisme statique et exclusif du « structuralisme » à la mode. Par un raffinement subséquent, l'accent est mis, dans « structure », sur la solidarité des phénomènes constitutifs qui l'oppose au simple conglomérat d'éléments. Dans une telle structure, la nature de chaque élément *dépend* de celle des autres au point qu'il n'*est* ce qu'il est que dans et par sa relation avec eux. Ce sont ces notions qui sont à la base de la théorie de la *Gestalt*.

Il y a deux façons d'interpréter un tel énoncé: une première, plus abstraite, est une interprétation synchronique qui ne retient que le verbe *être* et non le verbe *dépendre*: les choses n'existent et ne peuvent être définies que comme termes de relation. Cette conception débouche, en linguistique, sur la théorie hjelmslévienne sous sa forme originale du rejet de toute substance.

Une seconde interprétation, qui retient le verbe *dépendre*, rétablit des faisceaux de relations causales entre les parties du tout. La dépendance suppose que le statut d'un élément change si change le statut d'un élément dont il dépend. On peut certes concevoir la structure comme immuable: dans un édifice, par exemple, la stabilité du toit dépend de celle des murs portants; si les murs cèdent, le toit cèdera, mais la structure sera *ipso facto* détruite.

Mais si cela est vrai pour la structure d'un édifice donné, cela ne saurait valoir pour des structures qui doivent, sans jamais cesser de fonctionner, s'adapter à l'évolution du monde. En matière d'architecture, ce serait la structure conçue, non comme celle d'un édifice particulier, mais celle d'un type, comme l'immeuble d'appartements, l'adaptation se constatant par la comparaison d'édifices de dates différentes. Une langue se range sans conteste dans le type des structures qui s'adaptent: elle est une structure de faisceaux d'habitudes susceptibles de se modifier sous la pression des besoins changeants de la communication dans la communauté qui l'utilise.

Il est clair que l'évolution des besoins se fait graduellement et n'affecte pas, d'un coup, l'ensemble de la structure, mais certains points seulement de cet ensemble. Si la langue, au lieu d'être une structure, n'était qu'un conglomérat d'éléments autonomes, une modification sur un point n'aurait aucune répercussion sur le reste. Mais, dans une structure où la nature et la valeur de tout élément *dépendent* de celles des autres éléments, toute modification affectant un point de la structure se répercutera à travers l'ensemble.

Si les matériaux que la structure met en oeuvre étaient, par nature, à cent pour cent adaptés aux fonctions qu'ils servent à assurer, s'il s'agissait, en d'autres termes, d'une machine d'une précision absolue, les répercussions seraient immédiates. Mais tel n'est pas le cas pour la plupart des structures et pour les structures linguistiques en particulier. Le fonctionnement même de la communication linguistique dans des conditions qui ne sont jamais idéales et la transmission de la langue à de nouvelles générations réclament, de façon impérative, une grande redondance, c'est-à-dire la présence, sur tous les plans, d'une masse d'éléments qui, le plus souvent, ne servent à rien, mais qui peuvent, le cas échéant, utilement relayer les parties décisives du message, par exemple, en cas de bruit ou de chute d'attention de l'auditeur.

Dans une structure largement redondante de ce type, certaines conséquences d'un ébranlement initial pourront se manifester avec un retard considérable et entrer en conflit avec les conséquences d'une autre atteinte à l'intégrité de la structure provenant également de l'extérieur. Il résulte de ceci que la structure sera, de façon permanente, le lieu du conflit de diverses forces ou pressions, et le fonctionnement de cette structure résultera d'un équilibre entre ces forces et ces pressions, équilibre constamment remis en question, mais toujours retrouvé, puisque la langue ne peut jamais cesser de fonctionner.

Un des premiers devoirs du linguiste est de décrire la langue dans son fonctionnement au moment où il peut l'observer. Cette description synchronique est indispensable avant d'aborder l'étude de l'évolution de la langue à travers le temps. Mais il ne faudrait pas y voir la seule fin de la recherche linguistique. Comprendre le fonctionnement d'une langue, c'est, en fait, comprendre comment elle peut servir à la communication sans jamais cesser d'évoluer.

Une fois mise au point la description de l'état de langue dont on part, on pourrait penser que l'effort du linguiste devrait porter sur l'identification de chacun des ébranlements initiaux dont il a constaté les répercussions en tentant de dégager la structure de la langue. La chose a pu être tentée lorsque l'ébranlement initial consistait en l'introduction dans la langue d'un mot nouveau dont l'apparition a déterminé une modification de la valeur sémantique de mots préexistants. Mais quelle que soit la valeur théorique de telles recherches, elles ne mènent, en général, pas très loin, pour la raison que le lexique d'une langue n'est proprement et étroitement structuré que par places, si bien que les répercussions consécutives à l'introduction d'un nouvel élément lexical s'épuisent vite.

En général, les nouveaux besoins d'une société en matière de communication se manifestent à une plus large échelle. On peut estimer, par exemple, que dans la société romaine, la complexité croissante des rapports humains a rendu insuffisante l'expression des fonctions nominales au moyen des six cas traditionnels, et l'on a tendu à faire, d'anciens adverbes comme *ad*, *in* et *de*, des prépositions indicatives de fonction. Il s'agit là de phénomènes non datables, dont l'origine se perd dans la nuit des temps, et qui n'aboutissent pleinement qu'au moment où disparaissent les dernières traces du système casuel, ceci à des dates variables selon les langues.

Parmi les pressions en conflit que doit identifier le diachronicien, il y en a qui ne sont pas spécifiques et dont on constate l'existence dans toute langue à tous les instants de son évolution. On ne comprendra rien au fonctionnement et au devenir des langues si l'on ne replace les faits particuliers dans un cadre général. Ce cadre est l'antinomie fondamentale entre les besoins de la communication qui réclament plus de précisions et de spécificité et la tendance permanente à réduire l'énergie dépensée pour communiquer ce qui entraîne une moindre précision et une moindre spécificité et une plus grande confiance accordée à l'information que l'auditeur peut tirer de la situation dans laquelle se déroule l'échange linguistique. Il s'agit là, en fait, d'un aspect particulier de ce qu'on a appelé la loi du moindre effort, qui ne suppose pas, comme on feint parfois de le croire, un triomphe de la paresse, mais un équilibre entre l'effort et le résultat.

Cette antinomie est au coeur même de ce que j'ai désigné comme l'économie du langage. Il existe toutefois un troisième facteur, la puissance de la tradition qui interfère à chaque instant dans la résolution de l'antinomie. Lorsqu'on parle de la puissance de la tradition en matière de langue, on pense naturellement à celle qui se manifeste sous la forme de corrections de la part des parents ou des maîtres. Il ne faudrait certes pas minimiser son influence. Mais la tradition s'impose, de façon plus fondamentale et décisive, par l'imitation, par l'enfant, du parler de son entourage avant qu'il sache, dans son propre usage de la langue, opérer par analogie. Ceci ne pourra se faire qu'au fur et à mesure qu'il parviendra à analyser les énoncés en unités significatives minimales, les monèmes. Une fois identifié un monème, il sera tenté de lui donner la même forme chaque fois qu'il l'emploiera; il formera, par exemple, un infinitif *dorer*, au lieu de *dormir* à partir de *il dort*, homophone de *il dore*. Mais, avant que ceci puisse se produire, l'enfant reproduira servilement, dans la mesure de ses moyens, les inconséquences, léguées par la tradition, qui nous valent, par exemple, la bigarrure des formes du présent du verbe *être* (*je suis, il est, nous sommes, vous êtes, ils sont*). Ces formes, l'enfant les a entendues et reproduites si fréquemment et si tôt qu'il est rare qu'il tente, plus tard, de les faire rentrer dans un schéma plus cohérent et plus logique, en remplaçant, par exemple, *je suis* par *j'es*, en accord avec *tu es, il est*, sur le modèle de *je dors, tu dors, il dort*.

S'il est relativement facile de faire comprendre et admettre le principe d'économie sur le plan du lexique et sur celui de la grammaire, on rencontre plus d'incompréhension et de résistances sur le plan de l'aspect vocal du langage. Pendant longtemps, le conditionnement profond de l'évolution phonétique est resté mystérieux. Il a fallu que la phonologie intègre à la langue proprement dite les faits phoniques pour qu'on arrive à concevoir la phonie de chaque langue comme une structure caractérisée, comme toute structure, par l'interdépendance de ses éléments constitutifs. Ceci reconnu, on a constaté que, sur ce plan du langage comme sur tous les autres, la nature et la valeur d'un élément est sous la dépendance des autres éléments de la structure, ce qui implique que l'économie linguistique fonctionne ici comme ailleurs: les sujets sont disposés à une dépense d'énergie pour maintenir une distinction là où elle est utile; ils ne le sont plus, à la longue, si la distinction est pratiquement inutile et de réalisation délicate. Mais là encore, la puissance de la tradition peut jouer dans le sens de la conservation.

Comme on peut s'y attendre, chaque cas particulier implique l'intervention de facteurs divers et les choses ne sont jamais aussi simples que l'exposé qui précède pourrait le faire croire. Pour expliquer, par exemple, la confusion, qui se généralise en français contemporain, de *in* et de *un* (dans *brin* et *brun*, par exemple), on peut, certes, arguer de l'inexistence pratique d'énoncés où seule la distinction entre ces deux phonèmes permettrait la compréhension du message. Il faut, en effet, une bonne dose d'imagination pour trouver des contextes et des situations où ceci serait le cas: soit, peut-être, un bois où des gens cherchent qui des fleurs, qui des champignons, et où quelqu'un dirait *j'en ai trouvé un beau brin* (sous-entendu « de muguet ») énoncé que la non-distinction de *in* et de *un* pourrait faire interpréter comme *j'en ai trouvé un beau brun* (sous-entendu « cèpe »). Mais ceci ne suffit pas, parce qu'il reste à expliquer pourquoi la confusion s'est manifestée à telle époque et dans telle section de la communauté linguistique, et ceci réclame qu'on précise que des phonèmes vocaliques nasals tendent à acquérir une articulation plus ouverte qui permet à une plus grande partie du flux de l'air de passer par le conduit buccal où se caractérisent les articulations, mais, qu'au fur et à mesure que la bouche s'ouvre, il devient de plus en plus difficile de bien distinguer entre une articulation à lèvres arrondies (*un*) et une articulation à lèvres rétractées (*in*). Il faut aussi postuler que tout ce processus a eu lieu plus tôt à Paris et dans les régions avoisinantes, c'est-à-dire dans des zones qui sont, depuis plusieurs siècles et pour des raisons faciles à comprendre, à l'avant-garde de l'évolution de la langue.

Les facteurs particuliers sont tout autres dans le cas d'une autre confusion qui, elle, ne se propage qu'assez lentement parmi les nouvelles générations parisiennes. Il s'agit de la distinction d'un *a* d'avant, celui de *patte*, et d'un *a* d'arrière, celui de *pâte*. Ici la distinction jouait autrefois, à Paris, un rôle non négligeable dans la compréhension des énoncés. Mais lorsque la langue de Paris s'est étendue à l'ensemble du territoire, beaucoup de provinciaux n'ont pas su réaliser la différence entre les deux *a* ou l'ont fait d'une façon telle, par exemple, en allongeant simplement la voyelle de *pâte* sans changer son timbre, que les Parisiens ne percevaient pas la différence. On s'est donc accoutumé, entre Français de toutes provenances, à éviter les mots qui faisaient ambiguïté: *las* en conflit avec *là*, *tâche* avec *tache*, etc... Le résultat est qu'aujourd'hui la distinction ne sert pas à grand chose, et ceux-là même qui savent réaliser la différence ne s'accordent pas sur la répartition des deux *a*: quels sont parmi *âge*, *image*, *cage*, et *page*, ou *sable*, *table*, *râble* et *aimable* ceux qui riment ensemble? Sur cinq

Parisiens, il y a toute chance pour qu'il y ait autant de réponses différentes que de sujets. Ici donc, il n'existait, au départ, ni absence de fonction, ni difficultés articulatoires particulières. C'est la nécessité de la communication à l'échelle nationale qui a déterminé une confusion qui gagne, lentement, mais sûrement, du terrain.

Toute explication fonctionnelle et structurale donne toujours, si elle est valable, une impression de grande complexité, sans qu'on ait pourtant l'impression que tout a été dit. Ceci, tout simplement, parce que les faits eux-mêmes sont complexes, que les facteurs, même si l'on parvient à les grouper en faisceaux, sont nombreux et variés et que, si loin qu'on remonte dans le passé, il ne peut être question de découvrîr une cause première. Ici, comme partout ailleurs, la recherche est toujours en devenir et l'on ne devra jamais se lasser de poursuivre les analyses et de rechercher les interdépendances entre les éléments qu'elles isolent, que ce soit en synchronie ou en diachronie.

Le structuralisme ne doit jamais être un prétexte pour pratiquer le globalisme qui consiste, en fait, à démissionner devant la complexité des relations que révèle l'analyse des structures. Il est simplement la garantie que le processus d'analyse ne débouchera jamais sur une multitude de détails dont on traite isolément sans qu'il soit jamais question de les reconsidérer dans leur ensemble.

L'analyse des structures ne doit jamais aboutir à leur dislocation: l'analyse « anatomique » doit toujours être complétée par une « physiologie », c'est-à-dire une étude du fonctionnement. Pour reprendre une illustration qui a servi, il ne s'agit pas de démonter la structure qu'est une montre pour en étaler, sur un feutre vert, tous les rouages et tous les ressorts, mais d'indiquer comment les traits pertinents de ces éléments contribuent à assurer le fonctionnement d'un outil d'information.

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TRANSLATION: I. MCGILVRAY - ROME

FUNCTION AND STRUCTURE IN LINGUISTICS

The structural study of language is based on an analysis carried out according to the principle of relevance. Such an analysis must enable us to determine what is specifically linguistic in the physical reality observed and also allow us to classify the facts and arrange them into a hierarchy on the basis of their respective functions in the language studied. If it is admitted that in a given structure the nature of each element is dependent on that of the others, it can be seen that the evolution of a language may be conceived as the dynamics of a structure under pressure from the changing needs of those who speak it.

> Ne pas être raciste, c'est
> accepter telle qu'elle est une
> humanité différente, sans se croire
> obligé, par des «transformations»
> logiques ou autres, de la réduire
> à la sienne.
>
> JEANNE MARTINET

In a developing science such as general linguistics, where there is initially no universally accepted theoretical basis, nor even a tradition that everyone can draw upon, exchanges are often made difficult and unproductive by frequent disagreement over terminology. Of course, an effort is made periodically to define certain terms more accurately, but each school or group only does so to satisfy its own particular requirements. The consequent conflicts and overlapping lead beginners astray and complicate the task of those who endeavour to maintain or re-establish communications between all linguists.

The situation becomes further complicated when interdisciplinary considerations are used as pretexts to borrow from logic and mathematics concepts which do not necessarily meet the linguist's requirements. The greatest offenders in this respect are in fact to be found among linguists themselves. On the other hand, ever since linguistics has become fashionable, it has attracted those who are interested either in incorporating it in their own field or in breaking down the boundaries between it and the other branches of research. The onslaught stemming from logic in the form of transformational and generative grammar has naturally brought in its wake a welter of terminology that has only worsened the situation. One way of clearing the air may be to set up the linguistics of languages in flat opposition to logico-mathematical linguistics. Whereas the latter is unwilling to base its postulates on factual observation, the former, although capable of operating deductively, nevertheless defines its object in terms of the experience acquired through contact with widely varying linguistic realities. The linguistics of languages, which is not to be confused with the linguistics of individual languages, does not coincide exactly with what is known in linguistics as structuralism. However, it has been, and still is, one of the most basic forms of structuralism and the one which has withstood the logico-mathematical onslaught most decisively. The best way to delineate its domain seems to be to examine the values attributed to certain terms, and this is what we shall try to do here.

These terms are « structure », « structural », « structuralism », « function », « functional » and « functionalism ». Our purpose here is not to go into their etymology, nor to decide which contemporary linguistic trend can lay the best claim to them. In this case, as in all others, let us beware of being prescriptive. Let us simply observe what the usage is and take our precautions against possible confusion arising out of terminological ambiguities.

In France and elsewhere, what is currently known as « structuralism » in non-linguistic circles has been derived, in the final analysis, from linguistic structuralism. But the derivation was carried out under very special conditions and using a very peculiar variety of structuralism as its starting point. It is the product of speculative minds rather than of research workers bent on working out a theoretical framework for factual observation and analysis. In the final analysis, it stems from the teaching of Claude Lévi-Strauss who mostly proceeded by the metaphoric use of categories singled out by linguistic observation. This teaching is based on that of Roman Jakobson who, of all the structuralists, was the first to leave factual observation for general formulations. The best way to distinguish between fashionable « structuralism » and linguistic structuralism is to bear in mind that the former claims to be anti-historical whereas linguistic structuralism, in the Saussurian tradition, makes a clear distinction between synchrony and diachrony but, going beyond de Saussure, sees diachronic perspective as a means for understanding the dynamics of a structure under pressure from the changing needs of mankind.

It is no easy task to establish what there is in common between the various schools and tendencies to which the term structuralist has been applied historically. Although often coming from widely differing backgrounds, the Prague, Danish and American structuralists have certain methods in common. One example is commutation, which is based on the comparison of partially differing text segments as is done with minimal pairs on the phonological level. Consequently, the results of the analyses of the various schools are sufficiently analogous to be of general utility, even bearing in mind that the « allophones » of the one school are the « combinatory variants » of the other.

Their basic theoretical tenets are, however, quite different. Let us take as an example phonic phenomena, which are considered by Bloomfield's followers as being totally significant for the linguist. A phoneme groups together a certain number of different sounds, called « allophones », which represent basic linguistic reality. For the Danes, nothing in phonic (or semantic) phenomena is of true linguistic relevance; only the relationships between units are significant. Where the other structuralists see a phonic unit, the phoneme, the glossematicians identify a ceneme or « empty unit ». For the Prague school only those aspects of phonic phenomena are significant which have been recognized as relevant by virtue of their distinctive function in the language. A phoneme is a complex of distinguishing features which are, for this reason, relevant.

In actual fact, the various positions have become more flexible in the course of time. Bloomfield himself proposed that a phoneme be identified by the distinguishing features observed in it and, in his later writings, Hjelmslev makes some concession to phonic or semantic substance, provided that it is « formed », i.e., that it takes part in the relational complexes; this is tantamount to a recognition of the linguistic nature of distinguishing features.

This leads to a more or less explicit recognition of the relevance principle as the basis of structural linguistics.

This principle, which was formulated by Karl Bühler, is based on the observation that, at each point of an utterance, a hearer knowing the language used will unconsciously sift the information received for what it tells him about the speaker's identity, what sort of state of mind and mood he is in and what it is that the speaker is trying to tell him by means of the language used. This leads to the conclusion that linguistic reality does not coincide with phonic reality. Those elements of the utterance which correspond to articulatory habits acquired by imitation in the process of learning the language characterize the language as such and can thus be defined as linguistic. On the other hand, those elements which allow the individual to be identified, the timbre of his voice, for instance, are not of a linguistic nature. The physical reality of the utterance does not therefore coincide with the corresponding linguistic reality. All features specifically characterizing a given language can be considered as linguistic in nature. Among the features defined on this basis, the relevant ones will be those which contribute to maintaining linguistic communication. The relevant phonic features will be exclusively those which allow a distinction to be made between words and forms. In actual fact, deeper analysis will give a more complex hierarchical classification than that obtained simply by using the criterion of relevance or non-relevance. The glottal vibrations accompanying the articulation of the [n] in *net* are not distinctive, as there is no other word in English which differs from *net* only by virtue of the absence of glottal vibrations during the articulation of the [n]. They are not, however, linguistically insignificant as they give greater emphasis to what distinguishes the [n] of *net* from the [m] of *met*.

The application of the relevance principle clearly leads to a classification based, not on the physical nature of the facts observed, but rather on their function; physically different facts having the same function will be classified together, while physically identical facts having different functions will be placed in separate categories. This will be true not only in phonology but also at the other language levels. In the case of form identification, the *i-* of *ira* has the same value as the *all-* of *allons*, but the *all-* of *allons* has nothing to do with that of *all-ège*. As for the relative position of constituents of the vocal stretch, the positions of *chat* and *rat* are relevant in *le chat a mangé le rat*, but those of *la fleur* and *s'épanouisse* are not in *pour que s'épanouisse la fleur* (= *pour que la fleur s'épanouisse*).

All sciences are naturally based on relevance; no single science could ever claim alone to give an exhaustive description of any object whatever. The same object will receive different treatment at the hands of the physicist, the chemist or the geometrician. The history of science reveals that relevance is established quite naturally as long as there is no interference by man in the production of the object being studied.

When, however, it is a question of scientifically examining the activities of human beings or their utilisation of physical reality for specific purposes, it is no longer possible to neglect their intentions. Indeed, it is these intentions themselves which will point to the relevance. This does not mean that the examination will consist of enquiring into the subject's intentions. What must be done is to lay bare the functional identities underlying the variety of directly observable situations by means of an examination of behaviour. For instance, suppose a number of keys all different in the material of which they were made or in the workmanship of their bows. A functional classification would lead to all keys opening the same door being grouped together or hung on the same nail, whether they were made of iron or brass, or had a plain or embellished bow. This would be an application of the criterion that keys are made for locking and opening doors. Nothing would prevent us, though, from adopting another kind of relevance, for instance, an aesthetic one, if all the corresponding locks had disappeared and the most likely function of the keys was one of decoration. The same is true of human behaviour, including linguistic behaviour. In the latter case, the primary relevance is that of communication and it is this which falls within the linguist's province. But it is equally possible to conceive of another kind of relevance, an aesthetic one for instance, which would suit the purpose of opera singers where it is relatively unimportant to understand what they are singing.

The application of the relevance principle enables the true *raisons d'être* hidden below the surface of human behaviour to be detected, and allows an analysis of such behaviour to be made which is influenced neither by the observer's preconceived ideas nor by the way observed facts seem to be organized at first sight. This recalls the distinction American anthropologists make between what is overt, or immediately obvious, and what is covert, or hidden, but in actual fact represents the true motivation of the behaviour under examination. It is by failing to use the principle of relevance that many linguists have fallen into the trap of opposing a surface structure to a deep one, with « transformations » for passing from the former to the latter, ostensibly allowing the variety of languages to be reduced to unity. Relevance also enables us to penetrate beneath the superficial differences and analogies, not, however, to seek a hypothetical identity, but to find the true funtioning and specific structure of each language. This is the best way of opposing cultural imperialism which, in the name of the unity of mankind, tries to force the whole range of human actions and productions into the straitjacket of a single group's habits and preconceived ideas.

Relevance provides the basis for the only truly functional study of human behaviour. Because one can and must subject the same object to analyses based on different relevances, functionalism no longer appears as what it has often been accused of being, i.e., a way of doing away with all amenities and refinements in life, but rather the framework within which one can detect the particular relevance corresponding to any one of the amenities or refinements. Although in all judgements expressed on architecture the function of protecting man against the elements must be considered of basic relevance, it is nevertheless obvious at a glance that another relevance has become grafted on to this fundamental one: that of the « impressive » function which, by means of the size and magnificence of the building, aims at inspiring awe and admiration.

It is the fact that they belong to the same relevance that establishes this cohesion between the component features of a type of behaviour or the products of human activity which is observed when they are said to form a structure. Also in the case of languages it is common function which gives rise to structure.

In linguistics, to be a structuralist seems necessarily to imply that one considers a language as a structure. One could argue that this use of the term consists of a metaphorical extension of a kind that one should always be wary of making in scientific subjects. However, whether it be metaphorical or not, this use is of very ancient origin as, even in Latin, the word *structura*, besides its primary meaning of the arrangement of stones in masonry, also meant the arrangement of bones in the skeleton and even the arrangement of words in the sentence: *verborum quasi structura*.

In contemporary usage, one is first of all struck by the use of the term, for instance, in biology and psychology, where it has a static value and denotes the complex of fundamental elements, without regard to and in contrast with their functional dynamics. This is somewhat reminiscent of the static and exclusive synchronism of fashionable « structuralism ». In « structure », by a later refinement, the accent is placed on the solidarity of the constituent phenomena which distinguishes it from a simple aggregate of elements. In such a structure, the nature of each element *depends* on that of the others to the extent that it *is* what it is only by virtue of its relationship with the others. These are the notions which form the basis of the *Gestalt* theory.

There are two ways of interpreting such a statement. The first, and more abstract one, is a synchronic interpretation centred on the verb *to be* and not on the verb *to depend*. Things exist and can be defined only in terms of relationships. In linguistics, this conception leads to Hjelmslev's theory in its original form of rejection of all substance.

A second interpretation based on the verb *to depend*, re-establishes the network of causal relationships between the parts of the whole. Dependence presupposes that the status of an element changes if the status of an element on which it depends changes. It is, of course, possible to conceive of structure as being unalterable. The stability of the roof of a building, for instance, depends on that of the bearing walls; if the walls give way, the roof will collapse but, *ipso facto*, the structure is destroyed.

But while this is true for the structure of any given building, it is not true of those structures which must continually adapt themselves to the evolution of the world without ever ceasing to function. In the field of architecture the parallel would be structure conceived not as that of a particular building but of a type of building such as a block of flats. The adaptation becomes apparent on comparing buildings of different dates. A language certainly belongs to this type of self adapting structure. It is a structure composed of a complex of habits capable of being modified under pressure from the changing communicative needs of the community in which it is used.

It is clear that this evolution in needs takes place gradually and, at any given moment, only affects certain parts of the structure and not the entire structure. If, instead of being a structure, a language were simply an aggregate of autonomous elements, a modification at one point would have no effect on the rest. But, in a structure where the nature and value of every element depend on those of the other elements, any modification at one point will have repercussions throughout the whole structure.

If the materials used in the structure were, by nature, perfectly suited to the functions required of them, that is, if the structure were a machine of absolute precision, the repercussions would be immediate. But this is not true for most structures, and for linguistic structures in particular. The very fact that linguistic communication takes place in conditions which are always far from ideal and the necessity to transmit the language to new generations require a considerable amount of redundance, i.e., the presence at all levels of a mass of elements which, more often than not, serve no useful purpose but can, when required, satisfactorily relay the vital parts of the message, for instance, in the event of noise or a decrease in the hearer's attention. In an extensively redundant structure of this type, the appearance of certain consequences of an initial disturbance may be considerably delayed and thus come into conflict with the consequences of another threat to the structure's stability, also coming from the exterior. As a result the structure will become an area of permanent conflict between various forces or pressures and its resultant functioning will consist of an equilibrium between these forces and pressures which is constantly being upset but is always re-established, as language can never cease to function.

One of the first duties of a linguist is to describe the language as it functions at the moment when he is able to observe it. It is indispensable to carry out this synchronic description before setting out on a study of the language's evolution in time. However, it must not be considered as the only purpose of linguistic research. An understanding of how a language functions is, in actual fact, tantamount to understanding how it can be used for the purposes of communication without ever ceasing to evolve.

Once the prerequisite of a description of the *état de langue* has been carried out, it might well seem that the linguist should then endeavour to identify each of the initial disturbances which gave rise to the repercussions he has observed when laying bare the structure of the language. It has, in fact, been possible to do this when the initial disturbance consisted of the introduction of a new word into the language as a result of which there was a modification of the semantic value of pre-existing words. But whatever the theoretical value of such research, it usually does not get us very far for the simple reason that the lexis of a language is only truly and closely structured in places. Consequently, the repercussions arising out of the introduction of a new lexical element are very quickly exhausted.

The new communicative needs of a society generally manifest themselves on a larger scale. For instance, it is highly probable that in Roman society, because of the increasing complexity of human relationships, the expression of nominal functions by means of the six traditional cases became inadequate and a tendency arose to use former adverbs such as *ad*, *in* and *de* as prepositions indicating function. It is impossible to tell when these phenomena began, as their origins are shrouded in the mists of time. They only come into full play when the last traces of the case system disappear, the date of which varies according to the language.

Among the various conflicting pressures that the diachronic analyst must identify, there are those which are non-specific and which can be found in all languages at any stage of their evolution. Nothing can be understood of the functioning or the evolution of languages if the particular facts are not placed in a general framework. This framework consists of the fundamental antinomy between communication needs, which demand greater precision and specificity, and the permanent tendency to reduce the amount of energy required for communication, which engenders less precision and specificness and induces the hearer to abstract more information from the situation in which the linguistic exchange is taking place. This is, in fact, a particular aspect of what has been called the law of least effort. This does not mean, as some people would seem to assume, the triumph of laziness; it is simply a state of equilibrium between effort and the result obtained.

This antinomy is the very essence of what I have called the economy of language. However, there is a third factor which interferes with the resolution of the antinomy at each instant. This is the weight of tradition. When speaking of the weight of tradition in language, one automatically thinks of it in terms of correction by parents or school teachers. While this, of course, has an importance that must not be minimised, tradition makes itself felt in a more basic and decisive way when the child imitates the language of the people around it before it reaches the stage in its own use of the language of working by analogy. This it can only do as it gradually becomes able to analyse utterances into minimal significant units or monemes. Once it has identified a moneme, the child will be tempted to give it the same form each time it uses it. For instance, starting from *il dort*, a homophone of *il dore*, it will form an infinitive *dorer*, instead of *dormir*. But before this can happen, the child will have, to the best of its ability, simply repeated parrot-fashion the inconsistencies handed down to it by tradition, such as the motley forms of the present of the verb *être* (*je suis, il est, nous sommes, vous êtes, ils sont*). The child will have heard and repeated these forms so often and from such an early age that very rarely will it later try to force them into a more coherent and logical pattern by replacing, for example, *je suis* by *j'es* in agreement with *tu es, il est*, taking as model *je dors, tu dors, il dort*.

While it is fairly easy to explain and admit the principle of economy on the lexical and grammatical levels, more misunderstanding and resistance are met with at the level of the vocal aspects of language. For a long time the deep conditioning of phonetic evolution remained a mystery. Only when phonology succeeded in integrating phonetic features into language proper was it possible to conceive the phonetics of each language as being characterized, like all other structures, by the interdependence of its constituent elements. Once this was recognized, it was seen that at this language level, as at all the others, the nature and the value of an element are dependent on the other elements of the structure, which implies that linguistic economy operates here as elsewhere. Speakers are inclined to spend energy to keep a distinction where it is useful. They are much less inclined to do so in the long run where the distinction is practically useless and hard to make, although, even here, the weight of tradition may play a conservative rôle.

As might be expected, each particular case implies that there are different factors at work and things are never as simple as the preceding discussion may lead one to believe. In order to explain, for example, the confusion which is spreading in contemporary French between *in* and *un* (e.g. in *brin* and *brun*), one could easily argue that practically no utterances exist where understanding of the message depends on a distinction made between these two phonemes alone. Indeed, a lot of imagination would be required to find contexts and situations where this would be so. A possible case could be in a wood where some people are gathering flowers and others are looking for mushrooms. If, at a certain point, someone says *j'en ai trouvé un beau brin* (with *« de muguet »* understood) and no distinction is made in the utterance between *in* and *un*, it could be interpreted as *j'en ai trouvé un beau brun* (with *« cèpe »* understood). But this is not enough, as it still has to be explained why the confusion took place at a particular time and in a particular section of the language community. To do this, it must be explained that nasal vocalic phonemes tend to acquire a more open articulation allowing a greater flow of air to pass through the buccal passage where articulation takes place. The wider the mouth opens, however, the more difficult it becomes to make a clear distinction between an articulation with rounded lips (*un*) and one with spread lips (*in*). It must also be said that the whole process took place first in Paris and neighbouring regions, that is, in those areas which, for obvious reasons, have been in the vanguard of the language's evolution for several centuries.

Quite different factors are at work in another confusion which is only very slowly spreading among the new Parisian generations. This is the dropping of the distinction between a front *a*, as in *patte*, and a back *a*, as in *pâte*. In Paris, this distinction used to have a considerable importance in the understanding of utterances. When the Paris dialect spread throughout the country, many provincial speakers were either unable to make the distinction between the two *a* sounds, or made it in such a way, e.g., by simply lengthening the vowel of *pâte* without changing its quality, that Parisians were unable to tell the difference. Wherever they came from, French speakers thus got into the habit of avoiding words that gave rise to ambiguity such as *las* in conflict with *là*, *tâche* with *tache*, etc.... The result is that, today, the distinction serves no useful purpose, and even those who are able to make the distinction do not agree on how the two *a* sounds are distributed; out of *âge*, *image*, *cage* and *page*, or *sable*, *table*, *râble* and *aimable*, which are those that rhyme together? If the question were put to five Parisians it is quite likely that you would get five different answers. In this case, therefore, it was not initially a matter of absence of function nor of particular articulatory difficulties. The confusion arose out of the necessity to communicate at a national level and is slowly but surely gaining ground.

All satisfactory functional and structural explanations give an impression of great complexity and yet one is left with the feeling that they do not tell the whole story. The reason for this is simply that the facts themselves are complex, the factors, even if they can be grouped and ordered, are numerous and varied, and no matter how far one goes back into the past, there is no hope of ever finding a prime cause. As in all other fields, research in this direction is in continuous development and both diachronic and synchronic analyses must be pursued unremittingly in order to ferret out the interrelations between the elements they bring to light.

Structuralism must never become a pretext for the practice of globalism which is tantamount to surrender in the face of the complexities revealed by structural analysis. It is simply a guarantee that the process of analysis will never give rise to a multitude of details that are treated singly instead of being considered as a whole.

Structural analysis must never lead to dismemberment; « anatomical » analysis must always be followed up by a « physiological » one, i.e., a survey of function. To use a well-known illustration, it is not a matter of pulling a structure like a watch to pieces in order to display the various movements and springs on a piece of green felt. What must be done is to demonstrate the part played by the relevant features of these elements in guaranteeing the functioning of an instrument of information.

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FISICA QUANTISTICA
E TRADIZIONE FILOSOFICA

L'idea di complementarità può essere intesa come espressione dell'esigenza semantica, di dare cioè significato, sul piano dell'esperienza, al formalismo della teoria dei quanti. Nel soddisfare tale esigenza l'esperimento (in particolare quantico) va visto come un tutto « indivisibile » e « chiuso » come ha notato insistentemente N. Bohr: da qui discende un carattere di primitività dell'esperimento, che va però assegnato a tutto l'operare dei fisici, e non soltanto nel campo interessante la quantistica. La differenza tra la fisica, che si dice classica, e quella del secolo XX è soltanto nelle conseguenze dell'arbitrario taglio (dicotomia), in osservatore ed osservato, che per comodità di descrizione viene operato: nella prima le conseguenze non sono gravi e possono persino passare inavvertite, nella seconda appaiono invece conturbanti, in quanto ci pongono di fronte a difficoltà che possono anche sembrare non superabili. La relatività speciale, e più decisamente la quantistica, hanno richiamato l'attenzione sull'arbitrarietà di quella dicotomia, incrinando così fortemente la tradizione filosofica su cui si è sviluppata la costruzione della fisica classica.

1. - COMPLEMENTARITÀ ED ESIGENZA SEMANTICA. — Il principio di complementarità, proposto e difeso in più scritti da Niels Bohr, si presenta con una certa chiarezza quale criterio metodologico; e ciò in contrasto con gli altri principi considerati in fisica, pur se questi ultimi, quando siano esaminati criticamente, si presentino anch'essi appunto come criteri di ordinamento delle nostre esperienze; così, ad esempio, è per il principio di causalità, nonostante le proiezioni che di esso sono state fatte, ed ancora si fanno, nel piano metafisico. Il suddetto principio di Bohr, come è ben noto, si riferisce alla descrizione di processi ed esperimenti, la cui spiegazione e sistematizzazione teorica siano ottenute soltanto col ricorso alle posizioni fondamentali della quantistica, le quali negano la possibilità di coordinare coerentemente tali processi mediante i concetti (o costrutti) messi a punto dalla fisica classica: distribuzione nello spazio-tempo con connessioni di tipo causale. Tuttavia tali concetti, pur escludendosi mutuamente, risultano complementari per un'esauriente descrizione della situazione sperimentale. L'esclusione suddetta scaturisce notoriamente dal fatto che se, nella coordinazione mutua delle componenti di un esperimento quantico, ci serviamo dello schema spazio-temporale, non possiamo definire delle connessioni strettamente causali (tra quelle componenti) bensì soltanto delle relazioni di tipo statistico. Viceversa se stabiliamo connessioni causali, ad esempio invocando la conservazione dell'energia ovvero della quantità di moto, dobbiamo rinunciare a distribuire rigorosamente le diverse componenti dell'esperimento in un quadro spazio-temporale.

Può dunque dirsi che l'idea di complementarità scaturisca dall'osservazione che, per una completa esauriente descrizione e coordinazione dell'esperimento quantico, sia necessario riferirsi ai suddetti due modi di descrivere; e qui « descrivere » va preso in senso *lato*, e cioè di « discorrere », sia ai fini di comunicare ad altri (preparati opportunamente) quanto si sta facendo concretamente, sia a quello di farsi una ragione, di spiegare cioè (nel senso scientifico) il nostro operare. Da ciò consegue infine che, se nella descrizione si ricorre a modelli, ci si trova di fronte alla necessità di considerarne due, l'onda ed il corpuscolo - meglio, diremo, nella necessità di invocare un *modello gemino*: onda e corpuscolo costituendo le due facce di una stessa figura, che permette di riassumere il discorso esplicativo degli esperimenti in esame.

Orbene il suddetto carattere del principio ci porta ad esaminarlo nella luce della moderna concezione delle teorie fisiche. Una tale teoria è pensata costituita da un sistema relazionale sviluppato deduttivamente partendo da appropriate posizioni primitive (aventi dunque carattere di postulati o di ipotesi), che dev'essere interpretato sul piano dell'esperienza. Ciò si ottiene tramite la messa in corrispondenza di alcune proposizioni particolari conclusive (chiudenti le catene deduttive) con altre che siano corrette descrizioni, in termini osservazionali, di particolari situazioni sperimentali. La suddetta concezione - vale la pena notarlo - ha ricevuto proprio dalla quantistica un notevole appoggio; in fisica dei quanti si parla esplicitamente appunto di « formalismo » (quantistico) e di « interpretazione ».

Un esperimento quantico è preparato e condotto, brevemente controllato, mediante la scelta di concreti corpi sui quali sia possibile eseguire concrete operazioni, in particolare quelle che sono significative per i fisici; corpi dunque atti a fungere - diremo - da supporto di dette operazioni, e che infine vengono distribuiti, nelle descrizioni, in un appropriato ordine spazio-temporale. Dunque la descrizione del sistema concreto operativo (in cui si opera) può essere fatta *soltanto* in termini del linguaggio fisico che si usa dire classico.

Condotto a termine un qualunque esperimento si presenta la questione di spiegarlo, nel senso scientifico, e cioè di inserire gli elementi che compiutamente lo descrivono (condizioni preliminari, eventuali fasi intermedie, risultati) in un contesto di pure relazioni. Ebbene nel caso degli esperimenti quantici non abbiamo che una sola possibilità per una tale spiegazione: ricorrere al formalismo della meccanica dei quanti, basato sulle matrici o sugli operatori hermitiani, accettandone tutte le conseguenze, tra cui quelle, apparentemente conturbanti, connesse con le indeterminazioni e le distribuzioni di probabilità.

Sembra a chi scrive che il contrasto tra i due modi di parlare del complesso degli esperimenti quantici, accettato - quasi come una necessità - in base all'idea di complementarità, possa essere del tutto superato se:

a) si veda la descrizione, in termini che usiamo dire classici, come discorrere sul processo di interpretazione operativo - concreta del formalismo [processo questo che - per definizione - chiama in causa concreti supporti delle componenti dell'operare (come fisici si intende), i quali sono - *per la riproducibilità* - distribuiti in un quadro spazio-temporale];

b) si assuma la meccanica dei quanti come il sistema formale da interpretare sperimentalmente.

In conclusione, il principio di Bohr può così essere visto come riconoscimento dell'esigenza semantica, della necessità cioè di assegnare significato empirico al formalismo della teoria quantistica; tale significato dovendo essere inteso come sommariamente detto nel capoverso a). [Maggiori notizie sulla questione della significanza empirica il lettore può trovare in una precedente nota dello scrivente pubblicata su questa Rivista[1]].

È qui da avvertire che nella concezione epistemologica di N. Bohr, ampiamente sviluppata nei suoi due volumi più importanti[2], il principio di complementarità svolge un ruolo più articolato. È ben noto che Bohr considera fondamentale, nel problema della conoscenza umana, la distinzione tra soggetto ed oggetto (conoscente e conosciuto), anche se tale distinzione non possa essere sotenuta in modo netto, come mostrano le difficoltà che si presentano in quantistica; in particolare l'indeterminazione nel precisare il confine tra il complesso degli strumenti di osservazione ed il sistema osservato [occorrerebbe dire piuttosto: il sistema che si immagina, nella spiegazione modellistica, di osservare]. È per questa convinzione che Bohr ha esteso le considerazioni sulla complementarità a campi diversi da quelli fisici, suscitando così non poche critiche, quasi tutte ben fondate. In questa nota, rimanendo però strettamente nell'ambito della fisica, sarà sviluppata una più incisiva concezione dei rapporti tra quantistica e tradizione filosofica; concezione che, tuttavia, tiene conto di alcune osservazioni del fisico danese che, a chi scrive, sembrano tra le più significative.

2. - Indivisibilità e primitività dell'esperimento.

— Niels Bohr nei suoi scritti metodologici ha molto insistito su due caratteri dell'esperimento quantico: la indivisibilità e la chiusura; al primo essendo affidato un ruolo più importante e significativo. Qui va subito notato che per esperimento quantico s'intende un processo operativo portante ad una situazione sperimentale, che viene spiegata mediante la meccanica quantistica, in genere invocando quelle che si dicono particelle elementari. Così: se consideriamo un esperimento su un fascio elettronico possiamo descriverne in modo del tutto riproducibile soltanto le fasi estreme: quella iniziale corrispondente all'eccitazione del campo elettrico che impartisce energia cinetica a ciascuno degli elettroni *immaginati*, nella descrizione esplicativa modellistica, costituire quel fascio (visto dunque come una collettività), e quella finale: l'effetto dell'impatto, ad esempio su uno schermo, in particolare fluorescente. Ebbene, nota Bohr, questo processo non può essere analizzato in fasi intermedie perfettamente definite in termini di « eventi » quali quelli considerati in fisica classica. [Qui una nota sarà opportuna. Dicendo « *elettroni immaginati....* » non si è voluto mettere in dubbio una loro « esistenza », ma soltanto precisare che essa è limitata all'immagine che noi, a buon diritto, ci formiamo della costituzione di un fascio elettronico. Questo può dirsi *portatore* di quantità significative per la fisica, e cioè precisate mediante concrete operazioni definite nelle loro modalità così da assicurare una soddisfacente ripetibilità, e consequentemente riproducibilità dei risultati; così, ad esempio, il fascio può «portare» una traiettoria visualizzabile per punti mediante lo spostamento appropriato di uno schermo fluorescente. Il singolo costituente della collettività (qui l'elettrone) si dovrà dire *pseudo-portatore*].

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[1] Serie VII, anno LXV, p. 245 (fasc. III-IV/1971).

[2] N. Bohr, *Atomic Physics and the Description of Nature*, Cambridge 1934 (1961): *Atomic Physics and Human Knowledge*, N.Y. 1958.

Dal carattere di indivisibilità suddetto conseguono quegli aspetti degli esperimenti quantici che tanto hanno meravigliato, ed ancora meravigliano, alcuni. Così: le interazioni tra gli strumenti di osservazione ed i singoli presunti componenti del sistema su cui si opera e che, nella descrizione esplicativa, vengono dunque immaginati osservati. Ma l'osservato è sempre una collettività, e di qui la possibilità di considerare nelle previsioni (sulla posizione e quantità di moto, ad esempio, di quelle particelle elementari) soltanto delle probabilità, le quali possono essere collegate agli effetti di disturbo che l'atto fisico di osservazione esercita, indipendentemente da eventuali imperfezioni degli strumenti impiegati. Conseguono infine le indeterminazioni le quali - è da tener presente - sono contenute e precisate già nel formalismo della quantistica, e costituiscono lo scotto che dobbiamo pagare per aver preso in considerazione quelli che prima abbiamo detto pseudo-portatori.

Recentemente in questa Rivista[1] J. L. Park ha esaminato attentamente quale relazione possa essere assunta come primitiva, nella quantistica, in sostituzione di quella accettata in fisica classica, al riguardo di una generica operazione di misura e del suo risultato. In fisica classica la misura è intesa quale procedimento idoneo a valutare quantitativamente una proprietà dell'osservato; così che la relazione primitiva, nota appunto J. L. Park, è qui quella del possesso. In quantistica questa relazione non è accettabile, e va quindi sostituita, secondo l'A. citato e col ruolo di idea primitiva, da quel complesso ordinato di operazioni (ripetibili), che preparano il sistema di corpi effettivamente osservato (costituito dunque da portatori) e permettono inoltre la definizione di valori numerici; complesso che J. L. Park chiama *preparation- measurement format.* Pertanto quando si afferma che, avendo preparato in un certo modo un concreto sistema, dalle misure scaturiscono numeri con relative probabilità, il discorso è senz'altro chiuso; e non ha senso (data dunque la primitività ammessa) domandarsi quale significato più profondo debba vedersi in tale affermazione. Ecco allora che le previsioni probabilistiche vanno intese riguardare soltanto le operazioni di misura (susseguenti a date specifiche preparazioni), e le quantità che a tali operazioni associamo; e non eventi intesi del tutto indipendenti dal nostro operare.

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[1] J. L. Park, « Scientia » CV (1970), 269, 370.

Orbene, se per poco si riflette, si conclude che la suddetta posizione di primitività, dell'esperimento dunque, è del tutto idonea a far superare le difficoltà metodologiche con cui si presenta la fisica quantistica. Come per tutte le idee, anche per questa può trovarsi il germe in precedenti osservazioni critiche, che qui chi scrive intende mettere in luce, senza con ciò voler negare completamente originalità a J. L. Park.

Un primo germe dell'idea in esame può vedersi nell'analisi einsteiniana della simultaneità, che notoriamente conclude ponendo in piena evidenza come un'affermazione di simultaneità abbia senso fisico (operativo) soltanto se è precisato lo stato di moto relativo tra l'osservatore - operatore (con i suoi strumenti) e la sede in cui si immagina abbiano luogo gli eventi; così risultando una stretta e determinante connessione tra i due poli, diciamo, dell'esperimento: osservatore ed osservato; poli collegati da una successione di eventi, che possono essere compiutamente definiti soltanto se si accetta una precisa posizione sulla velocità di propagazione del segnale realizzante la suddetta connessione; ad esempio: quella einsteiniana di invarianza. Un'analisi critica dunque che ha dimostrato come, più che di simultaneità (che dovrebbe caratterizzare degli eventi) si possa, e quindi si debba, parlare soltanto di operazioni permettenti di definire, o no, una simultaneità. Analogamente per l'operazione di lunghezza e per quella di massa: una lunghezza ed una massa sono attribuiti (all'osservato) da un certo osservatore-operatore. In definitiva una proprietà intrinseca (all'osservato) la possiamo attribuire - se ci piace - soltanto in seguito ad una particolare convenzione (operatore in quiete coll'osservato). In conclusione può dirsi che, già per la relatività speciale, una « proprietà » non è più un *quid* posseduto da un corpo (o da un sistema di corpi) bensì l'*indice di una relazione*.

Secondo chi scrive, già il carattere di indivisibilità dell'esperimento, in particolare quantico, contiene - se pur confusamente forse - l'idea di primitività; e ciò specialmente se quel carattere lo si veda nel suo aspetto generale. Invero dichiarare una indivisibilità significa affermare essere arbitraria ogni scomposizione, dato che essa porta ad un risultato che non è pienamente soddisfacente (V. in seguito). Con ciò non siamo ancora giunti, è indubbio, all'idea di assumere come concetto primitivo l'esperimento; occorre compiere un salto è vero, ma a chi scrive sembra che l'indivisibilità notata da Bohr offra la migliore pedana di lancio.

A questo punto si presenta una domanda, che a rigore ha già la risposta nel richiamo prima fatto al riguardo della relatività speciale; e precisamente: la indivisibilità, e quindi primitività, dell'esperimento va intesa caratterizzare soltanto quello che suol dirsi quantico? Per J. L. Park sembra di si, per chi scrive la risposta è invece decisamente negativa.

Innanzi tutto si osserva che stendere la descrizione di un esperimento, classico o quantico, scomponendolo in componenti ha conseguenze che potranno risultare inavvertibili in alcuni casi e gravi in altri, e tanto gravi da togliere persino efficacia alla descrizione stessa, ovvero sollevare difficoltà di interpretazione. Nel primo caso dette conseguenze non lasciano traccia, nel senso di non impedire che l'operatore riesca a controllare lo sviluppo dell'esperimento nei limiti che lo soddisfano; nel secondo il controllo può risultare fortemente compromesso e finanche impedito. Orbene il maggiore o minore peso di queste conseguenze non esclude, per sè solo, il carattere di primitività; questo invero lo si ammette, o no, all'inizio del discorso metodologico.

Chi scrive ha da tempo assunto la posizione di primitività, e l'ha assunta significante indipendentemente dalle considerazioni quantistiche. Vuole però qui notare che è stata proprio l'insistenza di N. Bohr, sul carattere di indivisibilità dell'esperimento (quantico) ad esercitare su di lui grande suggestione verso la suddetta concezione. Sia qui permesso riportare alcuni brani da uno studio metodologico sulla fisica[1] articolato sulla posizione fondamentale così formulata: « il termine *operazione concreta* è per noi primitivo ». Le operazioni di misura accettate in fisica (anche quella classica) sono appunto particolari operazioni concrete, la cui ripetibilità è accuratamente messa a punto dai fisici. Per una chiara comprensione del passo è bene notare che nel citato studio i termini fondamentali, e caratteristici, del discorso sviluppato dai fisici sono le quantità (fisiche) le quali (op. cit. p. 8): « non preesistono alle nostre operazioni ma le rappresentano; con un'operazione fisica noi non misuriamo qualcosa avente esistenza indipendente dal nostro operare, bensì costruiamo un termine quantificato del discorso ». L'esistenza indipendente invocata per il *quid* nel precedente passo corrisponde al possesso nel linguaggio di J. L. Park; ebbene, tale esistenza, nel passo suddetto, è esplicitamente negata. [Ciò che può dirsi esista prima di un'operazione di misura, non è una quantità bensì un presunto portatore di quantità].

È senz'altro vero che in fisica classica si riteneva che un'operazione di misura permettesse di cogliere, diciamo, e precisare numericamente in una scala convenuta, una proprietà intrinseca all'osservato; questa convinzione non porta tuttavia a concludere che a ciò la fisica classica effettivamente giungesse. Il suddetto atteggiamento ha esercitato indubbiamente un fortissimo peso, dando agli scienziati la convinzione di toccare così il reale assoluto (in senso ontologico); e ad essa è in gran parte dovuto l'entusiasmo dei ricercatori che ha portato a tanti successi. Era però una convinzione acritica; e che così fosse lo dimostrano le difficoltà a cui essa ha portato alla fine del secolo passato;

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[1] D. Faggiani, *La struttura logica della fisica*, Iª Ediz. Torino, Einaudi, 1957, p.4.

difficoltà superate soltanto dopo che si iniziò l'analisi operazionale del significato dei termini del discorso fisico. Tale convinzione infine va scacciata se si vuole avere una più chiara idea dello *status* metodologico del procedimento scientifico in tutte le sue articolazioni, dunque anche non quantistiche. La relatività speciale e la meccanica dei quanti, questa in modo più conturbante e pertanto più efficace, hanno messo in luce l'assurdità di tale convinzione.

Prima di chiudere questo paragrafo è forse opportuna una precisazione. Chi scrive ha dunque parlato di primitività dell'operazione, J. L. Park dell'esperimento. Ebbene quale la mutua relazione?

Una operazione concreta, fondamentale per la fisica, è l'organo di definizione di un termine primitivo del discorso dei fisici: di qui la sua primitività. La sua analisi, scomposizione in fasi o momenti dell'operare, dunque in componenti, fa perdere tutto il significato (fisico).

Un esperimento conclusivo - « chiuso » dice Bohr - è costituito sempre da una serie operativa ordinata, e cioè da una combinazione, di operazioni fondamentali, avente un suo fine specifico - ecco l'ordine! Essa combinazione risponde ad una nuova domanda, che la caratterizza come esperimento chiuso appunto: ha dunque un significato (fisico), che non è dato dalla semplice somma dei significati delle sue componenti, ma che contiene qualcosa di più. Di qui l'indivisibilità, e quindi il carattere di primitività. La scomposizione di una tale serie di operazioni fondamentali fa sempre perdere quella parte del significato che la caratterizza proprio come esperimento chiuso.

Ecco infine del tutto chiaro perchè la scomposizione dell'operazione, o dell'esperimento, ha sempre un carattere di arbitrarietà, come si è notato nei precedenti capoversi.

3. - La quantistica e la dicotomia osservatore-osservato. — Il semplice riconoscimento di una primitività dell'esperimento, intesa cioè indipendentemente dalle eventuali conseguenze di un'analisi - e quindi significativa, sia per la fisica classica, sia per quella del presente secolo - getta un ponte tra le due fisiche; ponte che d'altronde è ben segnato dal fatto che il formalismo quantistico può notoriamente essere visto come una generalizzazione della meccanica classica, tenendo però presente che alle solite quantità numeriche vanno sostituiti nuovi algoritmi, quali le matrici ovvero gli operatori hermitiani. Una osservazione analoga può essere fatta anche al riguardo della relazione tra meccanica newtoniana e quella einsteiniana, la prima risultando un caso particolare della seconda; con che viene messo chiaramente in luce a quali condizioni quell'indice relazionale, a cui si è accennato (§ 2), possa essere assunto - senza gravi consequenze - rappresentazione di una proprietà (dell'osservato).

Ritorniamo dopo ciò alla quantistica, che ci interessa qui maggiormente. Aage Petersen[1] basandosi sulla precedente osservazione, che porta a porre una corrispondenza tra i formalismi della meccanica classica e di quella quantistica, ritiene che la teoria dei quanti non operi in definitiva una vera rottura con la tradizione filosofica. È una questione questa assai interessante che va approfondita; questo paragrafo è appunto dedicato ad un più vasto esame.

Una osservazione scientifica - un esperimento appropriatamente controllato in vista della riproducibilità della sua descrizione (anche quantitativa) - può essere vista come una ordinata sequenza di segnali (rilevabili con mezzi fisici appropriati) nella quale possono individuarsi - per comodità di descrizione - due poli, una domanda ed una risposta, ai quali possono farsi corrispondere due termini: osservatore, osservato. I due poli debbono però essere intesi come quelli di un magnete: la coppia suddetta va dunque vista come un tutto solidale, e non come una contrapposizione, quale quella che risulta dalla tradizionale dicotomia soggetto-oggetto che, nel linguaggio della fisica, ha trovato corpo nella ipostatizzazione prima, e nei tentativi di costruzione poi, di uno spazio e di un tempo assoluti, intesi cioè del tutto indipendenti dal nostro operare, in particolare come fisici; concezione che la fisica del novecento ha rifiutato perchè portava in secca la nave della ricerca. In questa concezione infine che, pur essendo criticabile ed ormai abbandonata in fisica, ha tuttavia profondamente operato per secoli, rientra quell'idea, già discussa, di poter definire delle proprietà intrinseche agli oggetti o agli eventi, quasi che questi le possedessero.

Si è visto che un esperimento, quantico o classico, è sempre un tutto indivisibile: la domanda - se è ben formulata - contiene già le caratteristiche distintive della risposta, anche se non i particolari valori numerici che potranno essere associati alle serie operative che costituiscono l'esperimento. [Un'operazione, o un complesso di operazioni, di misura ha sempre un preciso scopo fisicamente significativo: definire una o più quantità (nel senso matematico) svolgenti un ruolo coordinato nelle descrizioni che i fisici danno del loro operare specifico]. Un esperimento dunque, se ordinato, non è soltanto indivisibile ma anche, come dice Bohr, *chiuso*: ed è tale sempre almeno potenzialmente. Un processo operativo che non sia concluso con risultati, che rispondano ad una domanda in modo soddisfacente, non può dirsi ancora un esperimento ovvero una osservazione scientifica. [Quando ciò avvenisse, quel processo, con modificazioni appropriate suggerite in parte dai precedenti insuccessi, sarebbe ripetuto più e più volte fino a risultare chiuso nel senso prima detto].

Un'abitudine, radicata profondamente nella nostra attività mentale, ci porta a polarizzare l'attenzione sulle cose e sui loro mutamenti; inoltre a porre associazioni in termini ancora di cose; infine a memorizzare, distribuendo in ordine di successione ritmato su eventi esterni

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[1] A. PETERSEN, *Quantum Physics and the Phylosophical Tradition*, M.I.T. Press, Cambridge and London, 1968.

- ed esterni onde poter procedere ad intercomunicazione. E tutto ciò tenendo in ombra, quando non trascurando del tutto, i momenti del nostro operare. Un'abitudine che si è imposta anche nell'operare come ricercatori scientifici, perchè per secoli il metodo ha avuto successo, ed ha trovato in sistemi filosofici una certa giustificazione. Ebbene chi scrive pensa che a questa tradizione la quantistica abbia inferto un grosso colpo: l'immagine fisica della « cosa » già con la relatività viene strettamente legata all'osservatore, ma è la quantistica in definitiva a rifiutarla, detta immagine, e con *animus* che è sembrato, e ancora sembra a molti, iconoclasta: i tradizionali connotati fisici dell'oggetto (corpuscolo), posizione e quantità di moto, sfumano invero in aloni di probabilità strettamente connesse - si ricordi - alla struttura del processo di osservazione, e soltanto a questa. Si è sentito dire, ed ancora può sentirsi, che l'elettrone ed il fotone « si presentano » o « si comportano » alle volte come curpuscoli, altre come onde; ed in questa frase è chiaro il permanere della vecchia concezione. Ebbene non sono l'elettrone ovvero il fotone i responsabili; lo sono i nostri metodi di osservazione e di coordinazione dei risultati osservazionali: è l'esperimento (ordinato) che pone la domanda nella luce di schemi (in genere formulati in relazioni matematizzate) a cui si fanno corrispondere particolari modelli (corpuscolo, onda).

Così va inteso il nucleo profondo dell'atteggiamento del ricercatore che inquadri il suo operare nello schema formale della teoria dei quanti. Anche se non possa dirsi che nel pensiero di N. Bohr la rottura con la tradizione, nel senso qui visto, sia del tutto esplicita, tuttavia la insistenza sulla indivisibilità e chiusura dell'esperimento quantico ad essa conduce. Invero l'estrema conseguenza dei suddetti caratteri è appunto la negazione della dicotomia osservatore-osservato. [Che faccia comodo conservarla, in un discorso esplicativo delle situazioni sperimentali, è altra questione].

Si è detto prima che la domanda chiama il corpuscolo, ovvero l'onda, se non entrambi. L'affermazione va delucidata. Un esperimento attentamente preparato e condotto, così da interessare la scienza, è dunque sempre chiuso. Alcuni lo sono con la precisazione degli elementi caratterizzanti un particolare modello visualizzabile, il « corpuscolo » ad esempio; altri con quelli associati al modello (anch'esso visualizzabile) « onda ». Il modello, questo o quello, è contenuto nella struttura stessa dell'esperimento, non di ciò che si dice, per abitudine, l'osservato. [La « struttura » suddetta va intesa precisata dal carattere degli strumenti utilizzati (di preparazione del sistema su cui si opera, di osservazione e misura) e della loro distribuzione spaziale; inoltre dal carattere delle operazioni (eseguite su quel sistema) e dalla loro distribuzione temporale; infine dal carattere delle mutue connessioni che, come notato alla fine del precedente paragrafo, aggiungono significato]. Quando dunque si vedono in questa luce gli esperimenti della quantistica e si sente, ad esempio, parlare di onda associata, non si ha più ragione di chiedere cosa oscilli in quell'onda; e non ci si meraviglierà più se si conclude che sono in gioco delle probabilità nella definizione dei valori numerici che chiudono l'esperimento.

I due modelli semplici suddetti, corpuscolo ed onda, sono stati messi a punto dalla fisica classica, e si sono dimostrati idonei a descrivere le diverse situazioni sperimentali, ed a controllarle entro ampi limiti, finchè l'isolamento di alcune componenti della situazione (che doveva operarsi per semplificare la descrizione) non ha avuto conseguenze gravi, tali cioè da togliere efficacia alla descrizione. E si noti che tale isolamento è stato operato anche al semplice livello percettuale per definire i due modelli semplici suddetti. Esso però è sempre del tutto arbitrario; non deve meravigliare quindi che possa portare in un vicolo cieco. Questo carattere di arbitrio la quantistica ha messo in piena evidenza - se non ha rotto con la tradizione, ha almeno operato una profonda incrinatura.

Si è prima accennato alla possibilità di sviluppare un discorso esplicativo in termini di modelli visualizzabili. Ebbene in un tale discorso la differenza tra fisica classica e quantistica può essere caratterizzata come segue.

Nella prima si pone un'alternativa, tra i due modelli semplici, che è esclusiva: l'uno *aut* l'altro, non entrambi presenti dunque; ed è chiaro che da una cotale posizione, sottogiacente magari inconsapevolmente, scaturisca l'idea di proprietà intrinseche al *quid* che si dichiara osservato, per cui questo «si presenta» all'osservatore in un ben definito modo, che appunto quelle proprietà rispecchia. Di qui infine il passo è breve per passare ad assegnare carattere di permanenza alle proprietà, ed affermare pertanto la possibilità del loro riconoscimento. In definitiva l'osservato risulta *essere* un corpuscolo, ovvero un'onda; e cioè *possedere* (dice J. L. Park) i caratteri del corpuscolo o dell'onda.

Nella fisica quantistica l'alternativa, sempre tra i due modelli, non è esclusiva, corrispondendo piuttosto alla disgiunzione (tra proposizioni) della logica formale: l'uno *vel* l'altro modello; non è cioè escluso che entrambi siano invocati in esaurienti spiegazioni (modellistiche) di alcune complesse situazioni create dagli esperimenti, quelle situazioni a cui ci riferiamo - per abitudine - invocando i termini « particella » ovvero « fotone ». La nuova posizione logica, più articolata della precedente (il primo, ovvero il secondo, ovvero ancora entrambi i modelli), oltre ad offrire maggiori possibilità di spiegazione, toglie la base alla ipostatizzazione della proprietà (intrinseca all'osservato): per cui non ha più senso invocare un carattere distintivo posseduto in permanenza, una essenza; e di qui infine il volgersi dell'attenzione sugli altri aspetti della situazione sperimentale, quali il modo di operare dello sperimentatore.

Si tenga però ben presente che una spiegazione in termini di modelli del genere contiene sempre il pericolo di ingenerare equivoci, proprio perchè dà corpo a quell'abitudine mentale di cui si è detto. La domanda che pone un completo esperimento è precisata in modo sicuro, inequivocabile e riproducibile, soltanto se formulata in base a criteri di ordine contenuti, suol dirsi oggi, nel formalismo della teoria, e cioè nel sistema ipotetico-deduttivo che è alla sua base; criteri di ordine che sono formulati in termini matematici e costituiscono le proposizioni primitive (accettate senza dimostrazione, come sono le ipotesi) di quel sistema formale. Ebbene è a questo che occorre far riferimento se si vogliono evitare equivoci.

Ciò che è essenziale per l'interpretazione operativo-concreta di un formalismo è la messa in corrispondenza di due sequenze: una, di numeri, *scelta* tra quelle che si possono dedurre nel sistema formale precisando alcune variabili; l'altra, di quantità fisicamente significative (costrutti operativi quantificati), *scelta* tra quelle che si riferiscono a particolari riproducibili situazioni sperimentali. [Qui si presenta la questione: con quali criteri si operano le due suddette scelte? La prima è in genere piuttosto semplice, in quanto può essere fatta tra i valori numerici che soddisfano una certa relazione matematica conclusiva del sistema ipotetico-deduttivo; la seconda presenta sovente delle difficoltà superate, alle volte, operando diversi tentativi, altre ricorrendo a quella che suol dirsi intuizione fisica, quando cioè non può stendersi una razionale giustificazione]. Le suddette descrizioni di situazioni sperimentali vanno stese in termini puramente osservazionali, precisando ovviamente tutte le operazioni, di preparazione del sistema operativo, e di misura vere e proprie. A ciò infine può associarsi una spiegazione modellistica che svolge la interessante funzione di connettere i due momenti del processo, cioè le due componenti della teoria, quella formale e quella interpretativa sul piano concreto. Così è soddisfatta anche l'esigenza connessa alle nostre abitudini, ma la spiegazione va presa qual'è: modellistica appunto. Libero però chiunque di porre una corrispondenza col piano ontologico, a condizione che si renda conto che ad essa non giunge attraverso un processo logico (della logica che regge il discorso scientifico) bensì per un atto di fede.

Ritornando al nostro problema fondamentale, concluderemo notando che la concezione articolata sulla contrapposizione osservatore-osservato, se vista rappresentare l'unico metodo di analisi del nostro operare concreto, si è dimostrata una pretesa del tutto arbitraria. [Se la dicotomia suddetta è vista in tal senso, la quantistica ci pone dinnanzi a gravi difficoltà; e si comprendono le decise opposizioni, quali quella di Einstein. Se essa è invece vista rappresentare *uno* dei metodi (con le sue limitazioni), quelle difficoltà possono essere superate; e lo sono nelle linee indicate da N. Bohr]. La fisica quantistica, dopo il primo richiamo della relatività speciale, ha fortemente contribuito al chiarimento; ed è da pensare che, senza di essa, quel primo richiamo non avrebbe lasciato gran traccia.

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TRANSLATION: R. CAPECCHI HOLZER - GENOVA

QUANTUM PHYSICS AND THE PHILOSOPHICAL TRADITION

The idea of complementary can be considered to be an expression of the semantic requirements of quantum theory, whose formalism requires to be given a significance on the plane of experience. In satisfying such exigency, physicists must regard each of their experiments (particularly in quantum physics) to be an « indivisible » and « closed » whole, as it was pointed out by Niels Bohr insistently. Experiments derive a kind of primitivity from this, but it is to be noted that such a characteristic must be assigned to physical operations not only in quantum mechanics but in all fields. The difference between the so-called classical physics and the 20th century physics lies in the consequences of the arbitrary division (dichotomy) drawn between the observer and the observed for the sake of convenience in the descriptions of experiments. The consequences are not so very serious for classical physics, since they can even pass unnoticed in that field, whereas so troubling are the consequences for quantum physics that physicists are compelled to face difficulties that sometimes may seem to be insurmontable. Having emphasized the arbitrariness of the above dichotomy, special relativity and, more decidedly, quantum physics have deeply shaken the philosophical tradition on which the structure of classical physics developed.

1. - COMPLEMENTARITY AND SEMANTIC EXIGENCIES. — The principle of complementarity, proposed and upheld by Niels Bohr in many writings, reveals itself rather clearly to be a methodological criterion; and this contrary to any other principles considered in physics, even though, when examined critically, these also turn out to act as ordering criteria of our experience; so it is, for instance, for the principle of causality, in spite of the projections of it which were and are still made on the plane of metaphysics. As it is well known, Bohr's principle concerns the descriptions of the physical procedures and experiments whose theoretical explanation and systematization are obtained by resorting only to the fundamental premises of quantum physics, which deny the above procedures the possibility of being co-ordinated consistently by means of the concepts (or constructs) formulated in classical physics: space-time distribution and causal connections. Though such concepts exclude from each other, they turn out to be complementary for an exhaustive description of the experimental situations. It is known that the above exclusion ensues, as a matter of fact, from this remark: in co-ordinating the components of quantum experiments with one another, if we use the space-time scheme, we shall be able to define only statistical relations and not strictly causal connections (between the components); whereas, if we establish causal connections (for instance: on the ground either of the conservation of energy or of the momentum) we shall have to give up the possibility of distributing the different components of the experiment in question strictly within a space-time frame.

Therefore, we can say that the idea of complementarity originates in that, for a complete and exhaustive description and co-ordination of quantum experiments, reference must be made to the above two ways of describing; where the word « describing » must be understood in a broad sense and considered to be nearly equivalent in meaning to the word « talking ». And this with a view either to communicating what we are doing in the concrete to other people (suitable prepared) or to knowing the reasons for our operating, that is to explaining it (in the scientific sense).

It follows that, finally, if models are to be used in descriptions, two of them must be considered: one referring to the wave, the other to the particle; to be more exact, we ought to apply to a *geminate model*, as the wave and the particle constitute the two faces of the same picture, on whose ground we can sum up our explanatory talk about the experiments in question.

Well, such a peculiarity leads us to examine Bohr's principle in the light of the modern thought on physical theories. Nowadays a theory is regarded to be constituted by a relational system developed by deduction from proper primitive positions (having the character, therefore, of postulates or hypotheses) which must be interpreted on the plane of experience. This is obtained by correlating some particular conclusive propositions (closing the deductive chains) with others properly describing some particular experimental situations in observational terms. The above conception - this is worth remarking - was strongly backed up by quantum theory, where (quantum) « formalism » and « interpretation » are explicitly talked about indeed.

A quantum experiment is planned and carried out, in one word controlled, by selecting some concrete bodies on which concrete operations can be done, such as the significant ones for physicists, in particular; bodies, therefore, fit to act - let us say - as operations-bearers and finally arranged in a suitable space-time order in the descriptions. In conclusion, the concrete-operational system (within which physicists operate) can be described *only* in the terms of the language of physics which is usually called classical.

As soon as an experiment concludes, the question arises of explaining it, in the scientific sense, namely of inserting the elements describing it completely (such as: preliminary conditions, possible intermediate stages and results) in a relational context. Well, as far as quantum experiments are concerned, we have only one possibility left for giving an explanation: to have resort to the formalism of quantum mechanics based on hermitiam matrices or operators; yet taking all the consequences, even those, which ensue from the indetermination and distribution of probability, puzzling us apparently.

It seems to the writer that complete overcoming can be attained of the contrast between the two ways of speaking about quantum experiments taken in their complex, contrast which is accepted - almost out of necessity - by physicists on the ground of the idea of complementarity, if:

a) any description made in the terms we usually call classical is looked upon as a way of talking about the process of concrete-operational interpretation of quantum formalisms [by definition, such a process involves some concrete bearers of the components of our operating (as physicists, of course) which are distributed within a space-time frame for the sake of reproducibility];

b) quantum mechanics is taken as the formal system to be interpreted experimentally.

Bohr's principle, therefore, can be regarded to be an acknowledgement of the semantic requirement of an empirical meaning, in the sense summarized in a), which must be given to the formalism of quantum theory. [The reader may found further informations about the empirical significance in the writer's article published by this Review in a preceding issue[1]].

It is to be pointed out here, however, that in Bohr's epistemologic thought, widely developed in his two most important volumes[1], the principle of complementarity plays a more complex rôle. It is well known that Bohr considered the distinction between the subject and object (the observer and the observed) to be fundamental for the problems of human knowledge, even though such a distinction can not be traced out and supported, as the difficulties arising in quantum physics show; let us mention in particular, the indetermination in stating the boundary between the observing set and the system observed. [We should rather say: the system we imagine to be observing when we are explaining our experiments by means of models]. Being convinced of this, Bohr extended his considerations on complementarity to fields different from physics, thus exposing himself to many criticisms, almost all well-grounded, besides. In this paper a sharper conception of the relationships between quantum physics and the philosophical tradition will be developed, yet rigorously restricted within the limits of physics; the conception, however, takes the most significant, in writer's opinion, of the danish physicist's observations into account.

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[1] Series VII, year LXV, p. 245 (issue III-IV, 1971).

2. - The Indivisibility and Primitivity of Experiments. — Many a time in his methodological works Niels Bohr insisted upon two characters of quantum experiments: indivisibility and closure; the former playing the leading rôle, more important and meaningful. It is to be noted first of all that by quantum experiment we mean an operational process leading to an experimental situation we explain through quantum mechanics, generally by resorting to the so-called elementary particles. So, as an example, among the stages of an experiment on electron beams, only the extremes can be described with whole reproducibility: the initial stage consisting of the excitation of the electric field supplying kinetic energy to each of the electrons which, in our explanatory description by models, are *imagined* to form a beam (whom we regard to be a collectivity, therefore); and the final stage consisting of the effect produced by the impact of the electron beam, for instance on a screen, a fluorescent screen in particular. Well, Bohr observed that such a process can not be analyzed in intermediate stages, thoroughly defined in terms of « events » such as those considered in classical physics. [Now, let us make an opportune remark. By saying « *electrons imagined......* », we had not intention of calling in question their « existence »; we only intended to point out that such an existence reduces itself to the image of the constitution of an electron beam we rightfully form in our mind. The beam can be considered to be a *bearer* of quantities which are meaningful for physics because they are specified by means of concrete operations, whose procedures are defined so that a satisfying repeatability and, consequently, reproducibility of the results can be attained; so, as an example, the beam can « bear » a path visualizable point by point by suitable shifting of a fluorescent screen. Every single component of the collectivity (the electron, in this case) has to be called a *pseudo-bearer*].

It is from the above character of indivisibility that quantum experiments derive those aspects even now so amazing for some physicists. So it is, as an example, for the interactions between the observing instruments and the single elements supposed to be some component parts of the system on which we operate; components, therefore, that, in the explanatory description, are imagined to have been observed. But since the observed thing is a collectivity, it follows that in predictions (of the position and momentum of the elementary particles, for instance) there is only the possibility of taking probabilities into account, which can be connected with the disturbing effects that the only physical action of observing induces by itself, apart from possible faults of the measuring instruments used. Finally, other consequences of the character of indivisibility are the indeterminacies which, it must be kept in mind, are already included and specified in the formalism of quantum theory; but this reckoning has to be paid because the elements taken into consideration are only pseudo-bearers, as it was said above.

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[1] N. Bohr, *Atomic Physics and the Description of Nature*, Cambridge 1934 (1962): *Atomic Physics and Human Knowledge*, N. Y. 1958.

In a recent issue of this Review[1], J. L. Park carefully examined what relation could be assumed to be the primitive one for quantum physics, as regards the measuring operations and their results, and to act as a substitute for the relation accepted in classical physics. In classical physics, the term measurement means a procedure suitable for evaluating a property of the observed quantitatively; it follows that the relation of possession is the primitive one in that case, as it was pointed out by J. L. Park. Such a relation is unacceptable to quantum physics where, in Park's opinion, the rôle of primitive idea must be taken by the ordered set of (repeatable) operations, which are carried out to prepare the system of bodies actually observed (therefore constituted by beares) and enable us to obtain some numerical values relative to the system; this set is called *preparation-measurement format* by J. L. Park. Hence it follows that, when one states that some numbers with their respective probabilities were obtained from measurements carried out on a concrete system prepared in a certain way, there is no doubt that his talk closes; (owing to the assumption of primitivity) it does not make sense, therefore, wondering what deeper meaning may be hidden in that statement. That is why the probability predictions must be envisaged to concern only the measuring operations (following some given specific preparations) and the quantities we associate with such operations; but not some events considered to be quite independent of our operating.

Well, on thinking it over a little, we will come to the conclusion that the foregoing position of primitivity, of the experiment of course, is quite suitable for enabling us to overcome the methodological difficulties presented by quantum physics. As it is for all ideas, so the germ of this idea can be found in previous critical remarks on which the writer means to throw light; having no intention, however, of denying any originality of J. L. Park's opinion.

From among the very first germs of the idea in question, one can be found out in Einstein's analysis of simultaneity, which, as it is well-known, concludes by clearly pointing out that a simultaneity statement is physically (operatively) meaningful only if full details have been given about the state of relative motion between the observer-operator (together with his instruments) and the platform where the events considered are imagined to occur; and this is the result: a strict and determining connection between the two poles, let us say so, of the experiment: the observer and the observed; the poles are linked by a succession of events, which can be defined completely only if a suitable assumption about the propagation velocity of the signal carrying out the aforesaid connection is agreed upon; as an example: Einstein's hypothesis of invariance. Such a critical analysis, therefore, showed that, rather than of simultaneity (which ought to be peculiar to the events) it is of the operations enabling us to define, or not, the simultaneity that it can, nay it must, be talked about. Likewise it is for the operations concerning length or mass, through which length and mass are assigned by an observer-operator (to the observed). After all, an intrinsic property (of the observed) can be assigned by us - if we like so - only in consequence of a particular convention (the observer and the observed are in the state of relative rest each other). In conclusion we can say that special relativity already considered a « property » to be no longer *a quid* possessed by a body (or by a system of bodies) but a *relational index*.

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[1] J. L. Park, « Scientia » CV (1970), 269, 370.

In writer's opinion, the idea of primitivity is already contained - though indistinctly, perhaps - in the character of indivisibility of the experiments, particularly in quantum physics; and this especially if the character is examined from a general point of view. As a matter of fact, to asseverate an indivisibility means to state the arbitrariness of any divisions, because of their leading to results not quite satisfying (please, see below). With all that, however, there is no doubt we have not yet got the idea of assuming the experiment to be a primitive concept; to tell the truth, we have to take a jump, but it seems to the writer that the indivisibility noticed by Bohr gives us the best launching-platform.

At this point a question arises which, strictly speaking, has been answered in the above-mentioned remark on special relativity; here it is: have the indivisibility and, therefore, the primitivity of experiments to be considered peculiar only to the so-called quantum topics? It seems so to J. L. Park but, according to the writer, the answer must be decidedly in the negative.

First of all, let us point out that the description of an experiment, either on classical or on quantum physics, made by division into its component parts, gives rise to consequences that may result to be unnoticeable in some cases but serious in other cases, and so serious as to deprive the description itself of efficacy or to raise difficulties of interpretation. In the former case, the consequences do not leave trace, namely they do not prevent the operator from controlling the course of his experiment within satisfactory limits; in the latter, the control can turn out to be seriously compromised or even hindered. Well, whether the consequences are of great or of little importance does not, in itself, exclude the character of primitivity, which is acknowledged, or not, from the beginning of the methodological talk.

It is quite some time since the writer adopted the position of primitivity, and assumed it to be significant independently, apart from quantum considerations. He, however, thinks it right to point out here that it was just N. Bohr's insistence on the character of indivisibility of (quantum) experiments that influenced him towards that conception. He takes the liberty of quoting some passages from a methodological study on physics[1], hinging on the fundamental position which is so formulated: « the term *concrete operation* is a primitive one for us »[2]. Well, measuring operations accepted in physics (classical physics included) are just concrete operations, whose repeatability is *carefully* improved by physicists. For a clearer understanding of that passage it is worth remarking that in the above-cited study the fundamental, and characteristic, terms of the talk developed by physicists are the (physical) quantities, which (op. cit., p. 8): «do not exist before our operations but represent them; with a physical operation we do not measure anything existing independently of our operating, but we build up a quantified term of our talk »[1]. The independent existence, claimed for the *quid* in the foregoing passage, corresponds to the word possession in J. L. Park's language; well, such an existence is explicitly denied in the above passage. [It is not a quantity but a quantity-bearer that can be assumed to exist before a measuring operation].

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[1] D. Faggiani, *La struttura logica della fisica*, Iª Ediz., Torino, Einaudi, 1957, p. 4.

[2] [*Translator's note.* In the original language: « il termine *operazione concreta* è per noi primitivo »].

It is quite true that in classical physics a measuring operation was considered fit to enable an intrinsic property of the observed to be picked up, let us say, and specified numerically according to a scale agreed upon; from this conviction, however, one is not allowed to infer that classical physics could really attain that object. Such an attitude undoubtedly exerted a great deal of influence upon scientists, making them convinced of having touched the absolute real (in the ontological sense); and it is largely due to this attitude the experimenters' enthusiasm which led to so many successes. It was, however, an acritical attitude, as it is shown by the difficulties which ensued from it at the end of the past century, and were overcome only after the operational analysis of the meaning of the terms of the physical talk began. Such a conviction must be driven away if we want to get a clear idea about the methodological status of the scientific procedure in all branches, not only in quantum physics. Special relativity and quantum mechanics, the latter in a more exciting thus more efficacious way, displayed the absurdity of such a convinction.

Before closing this section, we deem it would be better to explain a question. Here it is: the writer's remarks concerned the primitivity of operations, whereas J. L. Park wrote about the primitivity of experiments. Well, which is the connection between these two ideas?

A concrete operation, fundamental for physics, is the means to the definition of a primitive term of the language of physicists: hence its primitivity. Analyzing it, that is dividing it into operational stages or moments, therefore into component parts, makes the whole (physical) meaning vanish.

A conclusive-« closed » said Bohr- experiment is always constituted by an orderly operational series, that is by an ordinate set, of fundamental operations, having a precise object of its own - so order is involved! Such an arrangement answers a new question which just characterizes it as a closed experiment indeed: the orderly operational set, therefore, has a (physical) meaning, which can not be obtained by merely summing up the meanings of its components, because it contains something more. Hence the indivisibility, therefore the character of primitivity. Well, dividing such a series of fundamental operations into parts makes it always lose just the part of its meaning which characterizes it as a closed experiment.

Now it is all clear the reason why the division of an operation, or of an experiment, into its component parts has always a character of arbitrariness, as it was pointed out in the foregoing paragraphs.

3. - Quantum Mechanics and the Dichotomy between the Observer and the Observed.

— With the mere acknowledgement of the primitivity of the experiments the two physics link up; that character being understood independently, apart from the eventual consequences of an analysis - and thus meaningful to both classical and present century physics; the connecting line, besides, is already well indicated by the well-known fact that the formalism of quantum physics can be thought to be a generalization of classical mechanics, provided that one bears in mind that the usual numerical quantities have to be replaced by new algorithms, such as hermitian matrices or operators. An analogous observation can also be made on the relation between Newton's and Einstein's mechanics, in that the former turns out to be a particular case of the latter; which clearly shows to what conditions the relational index, mentioned above (§ 2), can be assumed - without serious consequences - to represent a property (of the observed).

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[1] [*Translator's note.* In the original language: « non preesistono alle nostre operazioni ma le rappresentano; con una operazione fisica noi non misuriamo qualcosa avente esistenza indipendente dal nostro operare, bensì costruiamo un termine quantificato del discorso »].

After all this, let us come back to the argument of greater interest to us now, that is quantum physics. On the ground of the foregoing observation, which brings forth a correspondence between the formalisms of classical and quantum mechanics, Aage Petersen[1] deemed that, after all, quantum theory does not really break off the philosophical tradition. So interesting is the question that we must go deep into it; which will be done in this section.

A scientific observation - that is an experiment carefully controlled in view of the reproducibility of its description (from the quantitative viewpoint too) - can be seen to be an orderly sequence of signals (detectable with suitable physical means) in which two poles, question and answer, may be individuated - for the sake of description; the last ones, in their turn, may be put into correspondence with two terms: the observer and the observed. The two poles, however, must be considered to be like magnetic poles: the aforesaid couple, therefore, has to be taken as a single whole, not as a contraposition of opposing members like that one which ensues from the traditional dichotomy between subject and object. In the language of physics, that dichotomy assumed the concrete form of hypostatization first, then of the tentative constructions of the absolute space and time, which were thought to be completely independent of our operating, as physicists in particular. As it is well known, indeed, the twentieth-century physics rejected such a conception, which was leading scientific research to a dead-end. Finally, it is in the same conception (which is criticizable and obsolete nowadays but had been working profitably for centuries) that the idea, already discussed above, is included of the possibility of defining the intrinsic properties of things, or events, as if things and events really possessed them.

It was shown hereinbefore that an experiment, both in quantum and in classical physics, is always an indivisible whole: provided the question is well formulated - it already contains all the distinctive characters of the answer, excepting the particular numerical values which will be associated with the operational set constituting the experiment. [A measuring operation, or a set of measuring operations, has always a precise purpose, physically meaningful: to define one or more quantities (in the mathematical sense), which play a co-ordinate rôle in the descriptions given by physicists relating their specific activity]. An orderly experiment, however, is not only indivisible but also *closed*, as Bohr said: and so it is always, at least potentially. Should an operational process not yield such results that the starting - question is answered satisfactorily, it can not yet be called an experiment, or a scientific observation. [In that case, the same process, suitably modified as partly suggested by previous failures, ought to be repeated over and over again until it will result to be closed, in the sense explained above].

Now we have got used to polarizing our attention on things and their changes, and this is already a deep-rooted habit; moreover, we are accustomed to putting associations, once again in terms of things, and, finally, to memorizing all that, by distributing it successively, according to an order based on external events - and reference is made to external events in order that intercommunicating might be possible. And all this is done by keeping the moments of our activity in the shade, if not quite neglecting them. This habit has taken root also in the human activity in the field of scientific research, because the method was a success for centuries and found a certain justification in some philosophical doctrines. Well, the writer deems that quantum theory struck such a tradition heavily: the physical image of the « thing » was already closely linked with the observer according to the theory of relativity; but, after all, it was quantum physics that rejected such an image, with an *animus* which seemed, and still seems, iconoclastic: to tell the truth, the traditional physical features of the object (particle), that is its position and momentum, shade into haloes of probability which are closely connected - let us recall this - only with the structure of the observing process.

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[1] A. PETERSEN, *Quantum Physics and the Philosophical Tradition*, M.I.T. Press, Cambridge and London, 1968.

It could be heard in times past, and still it can be, that electrons and photons « look » or « behave » sometimes like particles, sometimes like waves; and from these expressions it is evident how persisting the old conception has been. Well, we can not hold electrons or photons responsible for that, but it is our observing procedures and the methods for co-ordinating the results observed that are responsible for: as a matter of fact, it is the (orderly) experiment itself which asks the question, on the ground of schemes (generally formulated by means of relations in mathematical form) to which some particular models (as particles or waves) are made correspond.

Thus it is to be understood the deepest nucleus of the attitude assumed by the researchers whose activity is framed into the formal scheme of quantum theory. Well, even if it can not be affirmed that the break with the philosophical tradition was quite explicitly stated in N. Bohr's thought, at least in the sense explained hereinbefore, it can be said, however, that it was his insisting on the indivisibility and closure of quantum experiments that led to such a break. The denial of the dichotomy between the observer and the observed is the ultimate consequence of those characters, indeed. [As for the fact that retaining the use of it might be convenient to our explanatory talks about experimental situations, well, this is quite another question].

It was said above that it is the question put by the experiment itself which calls the particle, or the wave, if not both of them. Well, this statement has to be clarified. An experiment, so carefully prepared and carried out as to be of some interest to science, is always closed, therefore. For some experiments, closure is obtained when the elements are specified which characterize a particular visualizable model, as the « particle », for instance; for other experiments, the elements to be specified are those associated with the « wave » model, which is visualizable too. Either of the two models, indeed, is included in the structure itself of the particular experiment involving it, not in the structure of what, out of habit, is called the observed. [The above-cited « structure » is considered to be defined by the characteristics of the instruments used (to prepare the system on which operations are done, and to carry out observations and measurements) and of their spatial arrangement; by the characteristics of the operations (carried out on the system) and of their temporal distribution; and, finally, by the characteristics of the mutual relations which add meaning, as it was pointed out at the end of the preceding section]. If quantum experiments are seen from this point of view, therefore, when we hear, for instance, of an associated wave, there is no reason to wonder what oscillates in that wave; then we shall be surprised no more at hearing that, in conclusion, probabilities are involved in the definition of the numerical values closing those experiments.

The aforementioned simple models, particle and wave, were set up by classical physics and proved suitable for describing and controlling, within wide limits, all the different experimental situations met with, until some components of certain situations were isolated (in order to make the description easier); which led to such serious consequences, that some descriptions were deprived of all their effectiveness. Moreover, it is to be noted that, on defining the two simple models mentioned above, classical physics performed already an isolating operation, even if at a mere perceptual level. Such an isolating operation, however, is always quite arbitrary; no wonder, therefore, it could lead to a dead-end. It was quantum physics that, displaying such a character of arbitrariness, at least strongly shook, if not quite broke with, the philosophical tradition.

It was already mentioned above the possibility of developing an explanatory talk in terms of visualizable models. Well, in such a talk the difference between classical and quantum physics may be distinguished as follows.

In the former, an alternative choice is put between the two simple models, which implies an exclusion: one *aut* the other, not both of them, can be present, therefore; it is clear that the idea concerning the intrinsic properties of the « *quid* », which is declared to be the observed, resulted from this position underlying the theory in question maybe unconsciously; the observed, therefore, « appears » to the observer in a well-definite way reflecting just such properties. From all that, it was easy for classical physics to go farther, assign the character of permanency to the properties, and state, therefore, the possibility of recognizing them. In conclusion, the observed results *to be* a particle or a wave, that is to *possess* the characters of particle or of wave (as J. L. Park said).

In quantum physics, on the contrary, the choice, between the above models, is not an alternative one and does not imply an exclusion; it rather corresponds to the disjunction (between propositions) of formal logic: one *vel* the other model; this does not exclude we may have resort to both models in our exhaustive explanations (by means of models) of some complex situations arising from experiments; situations which we refer to - out of habit - appealing to the terms « particle » or « photon ». This new logical position is more flexible than the preceding one (one, or the other, or even both of the models are admitted); besides offering us some more possibilities of explanation, it undermines the hypostatization of properties (as intrinsic of the observed): so, applying to the distinctive characters, of which the observed would be permanently in possession, that is to the essence of the observed, does not make sense any longer; from all that, finally, we are led to turn our attention to other aspects of the experimental situations, as the experimenters' way of operating.

It must be kept in mind, however, that an explanation in terms of such a kind of models makes us run the risk of giving rise to a misunderstanding, just because it puts the above-mentioned mental habit into concrete form. The question put by a complete experiment is specified certainly, unambiguously and in a reproducible way, only if it is formulated on the ground of the ordering criteria which, as we say to-day, are included within the formalism of the theory considered, that is within the hypothetico-deductive system upon which the theory is founded. Such ordering criteria are formulated in mathematical terms, and constitute the primitive propositions of that formal system (propositions which have to be accepted with no demonstration, as it is for hypotheses). Well, we must refer to all that, in order to avoid any misunderstanding.

The essential thing to the concrete-operational interpretation of a formalism is that these two sequences are put into correspondence: one, of numbers, *selected* among the numerical sequences which can be obtained by specifying the values of some variables of the formal system; the other, of physically meaningful quantities (that is: quantified operational constructs), *selected* among the sequences referring to particular reproducible experimental situations. [Now this question arises: by what criterion do the foregoing choices be made? The former choice is generally rather simple, because it can be made among the numerical values satisfying a certain mathematical relation which closes the hypothetico-deductive system; the latter often involves some difficulties, which may be overcome sometimes by making several attempts, sometimes by having resort to the so-called physical intuition; and this when a rational justification can not be given]. The above-mentioned descriptions of experimental situations have to be developed only in observational terms; and it is obvious that both the operations carried out to prepare the operational system and the measuring operations must be specified precisely. Finally, an explanation given in terms of models may be associated with all that; explanation which performs the interesting function of linking the two moments of the process, that is the two components of the theory: the formal one and the interpretative one on the concrete plane. According to this, the exigencies ensuing from our mental habit also turn out to be satisfied; but the explanation must be taken just as it is: a model-made explanation, let us say. It is understood that anyone is quite free to put a correspondence on the ontologic plane, but on condition that he realizes to have attained to such a correspondence through no logical process (conformed to the logic which holds the scientific talk), but on the ground of an act of faith.

Coming back to our main problem, we conclude by pointing out that it is an arbitrary pretense to regard the conception hinging upon the contraposition between the observer and the observed as the only method for analysing our concrete activity. [If the foregoing dichotomy is understood in that sense, quantum theory make us meet with serious difficulties; and we can realize now the point of view of the decided opponents to that theory, as Einstein for instance. On the contrary, if such a dichotomy is considered to represent *one* of the methods (together with its restrictions), the above-mentioned difficulties can be overcome; and they are, indeed, along the lines indicated by N. Bohr].

After the first recall from special relativity, it was quantum physics that strongly helped in clarifying the question; and we are led to think that, without quantum physics, such a first recall would hardly have left any trace.

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A CONTRIBUTION TO THE THEORY OF ECONOMIC DYNAMICS

The present paper discusses the nature of economics as a social science and concentrates on the major contributions made by Michal Kalecki which have left a significant imprint on the history of economic analysis. While Kalecki left a clear mark on almost every field in economics, his seminal contributions were 1) he anticipated and independently discovered the system which has been called the Keynesian Revolution; 2) while the Keynesian system was primarily static, Kalecki's was explicitly dynamic and took income distribution into account, and Kalecki paid much attention to price theory and market structures; 3) he predicted the regime of the political business cycle; 4) he made significant contributions to the analysis of the problems of developing nations and modern capitalism in motion; and 5) he developed a theory of growth of the socialist economy and made great advances in planning methodology. The present paper concentrates primarily on 1, 2, and 4. The subject matter of 5 will be treated in a subsequent paper.

1. - Economics and Aims of Society. — Economics was once defined by a critic as common sense made difficult. Economics necessarily encompasses problems vital to all economic subjects as it is a study of mankind in the ordinary business of living, it studies that part of individual and social action which is most closely connected with the attainment and the usage of material requisites of well-being. But as the great Cambridge economist, Alfred Marshall, emphasized economics is on the one side a study of wealth (income) of nations, and on the other, more important side an integral part of the study of men as they live and move and think in the ordinary business of life. For man's character has been molded by his everyday work, by his behavior as producer and consumer, and the material resources he thereby produces and procures generally more than by any other influence Economics concerns itself predominantly with those motives which affect most powerfully and steadily man's conduct in the business part of his activity. It should be noted that the economist does not claim to measure any affection of the mind in itself directly but only in an indirect manner through its outward demonstration or its effect. Economics is a social science and cannot be compared with the exact physical sciences, as it deals with the continuously changing and subtle forces of human nature. Economics cannot achieve the precision, exactitude or power of prediction of the physical sciences[1].

Everybody has a personal stake in economics; the affairs of virtually every citizen and of governments (in domestic and international ramifications) in considerable and frequently increasing measure are economic affairs. But with all its limitations and with the proverbial idiosyncrasies of the members of the economic fraternity (Bernard

Alfred Marshall, *Principles of Economics*, (London: Macmillan & Co., 1920).

Shaw observed that if economists of this world were laid end to end they would not reach a conclusion), economics, can claim a number of major feats, discoveries and contributions to our understanding of the working of the economic fabric of society (prediction of likely consequences of adopting a particular measure or mismanagement of scarce resources). Economics attempts to answer questions such as: What to do and abstain from doing to improve the performance of the economic system and to administer (at all levels of economic activity) scarce resources better (make more effective choices in employment of scarce resources which have alternative uses) in achieving whatever ends the society aims at[1].

The ends themselves, the kind of society, culture, the system of values, etc., society must choose itself. Economic science can do no more than indicate the means, or alternative working arrangements for resource allocations and most effective instruments of attaining the postulated ends[2]. But the question as to which working arrangements and instruments of economic policy are employed is not a matter of indifference to the welfare of economic subjects. Their application entails certain costs, both material and human. « Material costs are all sacrifices to be made in terms of resources, that is the use, by the policy-maker, of manpower, capital ... and nature ... Immaterial costs include the aversion of the citizens to certain restrictions on their freedom, or the frictions arising out of them; these may indeed, also lead to material costs »[3].

Institutional arrangements - economic, administrative, or interaction of economic administrative arrangements - should not be assumed a priori. Institutions should be treated as unknown factors in the solution of the problem of optimal allocation of resources. They represent merely alternative production techniques to attain the postulated ends at the least cost (most efficiently). More concretely, whether market or extra-market devices (techniques) should be universally employed cannot be solved a priori for all cases and states of the economy. The only laws we must obey are the laws of nature (production constraints). No condition of institutional character are « given » beforehand but, from the array of alternative arrangements, those must be chosen that would perform the task most effectively.

In the perennial controversy of « planning versus market » planning and the market are often treated by the contestants in the dispute as ends in themselves (rather than as instruments in achieving whatever goals are aimed at). Neither plan nor market can be treated independently of the country's stage of economic development, tasks to be performed and rapidity of the required change, the country's endowment with natural and human resources, its participation in and reliance on foreign trade, general state of the economy (overheating, excess demand) and a host of other factors. To reiterate, planning and market (or blending of plan and the market), are best viewed as just alternative techniques of production. The economic problem is to maximize the product (of desired composition) in comparison to the productive inputs used (available). While the primer teaches us that the economic problem can be conceived alternatively as maximization of output from employment of given resources or minimization of inputs to produce given output, the alternative formulations are hardly equivalent as, for instance, in case of widespread unemployment the focal problem appears to be the anomaly of underutilization of resources (sizable unemployment of labor and idle equipment). Economic policies which lead to fuller employment of available resources (labor, capital, land, entrepreneurship), promotion of economic growth and greater economic stability, arresting of the rate of increase of inflation, promotion of high rate of technical dynamism and more efficient allocation of resources between alternative, uses, are gaining importance as governments are increasingly committed to pursue growth-oriented, full employment (stability) policies and a host of new objectives are added and rank high as desiderata of economic policy (equity, distributional, social and environmental aspects of economic life).

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[1] Cf. Paul A. Samuelson, *Economics*, (New York: McGraw Hill, 1970) and Oskar Lange, *Political Economy*, (New York: Pergamon Press, 1963).

[2] J. A. Schumpeter, *History of Economic Analysis*, (New York: Oxford University Press, 1954) p. 1145.

[3] Jan Tinbergen, *Economic Policy*, (Chicago: Rand McNally, 1967) p. 24.

This is not to say that there is no conflict between the widening gamut of objectives that the economic policy makers impose or are compelled to introduce, in articulation of economic policies.

2. - KEYNESIAN REVOLUTION. — We can only skim here the surface of the « Keynesian Revolution » in economics and of Kalecki's unique role in it. Prior to the breakthrough, the classical theory that dominated economic thought and policy viewed the capitalist economy as a natural, self-propelled, and self-regulating mechanism (« supply creates its own demand »), where a tendency to establish full employment prevails[1]. Such an economy tends to maintain full employment, and unemployment may be considered merely as an accidental, temporary displacement of and deviation from full utilization of economic resources - a rather unimportant frictional occurrence which may largely be disregarded. The flexibility of wages and prices ensures the mechanism. Unemployment may be cured by reducing money wages, since employment will rise with a decline of real wages. Unemployed workers, competing for employment, will be willing to lower the price for their labor. The wage reduction, by making labor relatively cheaper, should induce substitution of labor for other factors, thereby increasing employment. Rigid prices and institutional factors, such as trade unions, may obstruct the equilibrating process by artificially maintain-

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[1] MICHAL KALECKI, *Selected Essays on the Dynamics of the Capitalist Economy* Cambridge: Cambridge University Press 1971, Part I.

ing wages. *Ceteris paribus*, should wages be appropriately reduced, the unemployed, willing to accept a cut in wages, would find employment. Wages would fall more rapidly than prices. Profit margins would be improved. But wages have both cost and income effects. The reduction of money wages, *inter alia*, reduces money income and effective demand.

The classicists considered the rate of interest as another device for equilibrating savings and investment. Since the alternative to consumption is savings (income not consumed), a decline in consumers' expenditures increases savings. With a larger volume of savings, the savers lower their price. Because of the lower rewards, some savers are discouraged and choose to consume. At the same time, investors are encouraged to invest more and to employ a larger volume of savings, since, at the lower terms for borrowing, the previously unprofitable investment ventures may become profitable. Thus employment, in producing both consumption and investment goods, is expanded.

As modern macroeconomic theory has shown, an *ex ante* increase of non-speding on consumption (savings) does not guarantee additional investments. Equally, an *ex ante* rise in savings is not a prerequisite for additional investment. The decision to invest is independent of the decision to save. While the former is, *inter alia*, governed by the businessman's anticipation of profit, the latter is mainly a function of income. There is no mechanism to ensure that investment will increase so as to fill the gap for the deficiency of effective demand. Moreover, a decline of consumption expenditures reduces derived demand for additional equipment required for the production of goods. As a result of a decrease of consumption, the businessman's revenue drops. Part of the product produced will remain unsold and, thus, inventories of goods will pile up. Losses permeate the system. Output will be cut down and businessmen will reduce their labor force. With growing unemployment, income declines. With a fall in income, savings are reduced. Should the volume of investment remain stationary, income will decline to the point where, at the lower income, the reduced savings will equal investment. The equilibrating mechanism takes place through the variation of income rather than through the rate of interest.

The «Keynesian Revolution» refers[1] to the impact of the theory of determination and fluctuations of the level of aggregate output and employment, underscoring the dependence and impact of the level and structure of effective demand on the level of economic activity and on the degree of utilization of labor and productive capacity, emphasizing that fluctuactions in the volume of investment will cause and be accompanied by corresponding fluctuations in economic activity, expenditures, output, incomes, and employment. The policy, implicit in the theory, assigns a crucial role to deliberate government policy as an instrument for regulating or influencing effective demand; in case of deficiency of demand, by filling the gap between total income generated and private expenditures on consumption, capital formation, and net exports through compensatory fiscal policy, government expenditures to increase (or to reduce, in case of deficiency of productive capacities) the volume of expenditures.

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[1] Cf. JOAN ROBINSON, « Introduction to Michal Kalecki », *Studies in the Theory of Business Cycles* (Oxford: 1966), pp. VIII-IX.

The role of investment in establishing a certain level of national product is not associated with the final destination or the uses to which the investment will eventually be put. It is its income-generating effect that matters at the time when it is carried out. Whether the investment will prove to be useful or useless in the future is inconsequential from the standpoint of the equilibrating process of current (short-run) macroeconomic equilibrium. What matters is its role from the standpoint of the equilibrating process: that it generates the same effective demand when it is executed. It follows that from the standpoint of creating demand, it is inconsequential, ruling out offsetting effects, whether private investments are replaced by government expenditures, financed by loans-to mention here only one policy alternative - so that no reduction of any purchasing powere by taxation is involved. By the means of what Kalecki calls « financial tricks » (fiscal weapons, functional finance), the government expenditures, financed by budget deficits, even solve the insufficiency of effective demand; and, if they are appropriately large, they can ensure full employment. If the aim is to restore full employment, as far as the generation of effective demand is concerned, the direction of government expenditures is largely immaterial (perhaps given some appropriate assumption as to the propensity to consume). When investments fall short of savings, or of that level which is necessary to maintain effective demand, it is the basis of government policies in modern capitalism that the gap is covered by governmental expenditures (or taxes are reduced so as to increase disposable income and expenditures on consumption, and measures are taken to induce investments).

3. - KALECKI AND KEYNES. — In 1933, outside the mainstream of traditional economic theory, an unknown Polish economist, without formal training in economics, brought up almost exclusively on Marx and influenced by the writings of Rosa Luxemburg, without any contact with Keynes,

> created a system that contains everything of importance in the Keynesian system, in addition to other contributions ... (Kalecki) has a theory of employment that is the equal of Keynes' ... he certainly lacked Keynes' reputation or ability to draw world-wide attention; hence his achievement is relatively unnoticed[1].

[1] LAWRENCE R. KLEIN, « The Life of John Maynard Keynes », *Journal of Political Economy*, October 1951, pp. 447-448.

When Kalecki was visiting Sweden in 1936, he began dictating to his wife what would have constituted a comprehensive treatise of the main ideas that he had developed. He then learned of the publication of Keynes' *General Theory*. Upon reading the book, he discovered that it contained most of what he wanted to say, even to the jokes illustrating the proposition[1]. He immediately interrupted his work and left for England where he contacted the Keynesians and was considered an exponent of Keynes' theories. With his usual modesty, Kalecki never told Keynes that he had a prior claim on « his » theory. While Keynes was expounding to Kalecki that hardly anyone over the age of forty understood Newton's theory, thus drawing a parallel between that situation and the lack of undestanding with which Keynes' own theories were meeting, Kalecki was far too modest to even assume such a comparison. Kalecki's anticipation of the *General Theory* has only been publicly recognized after Keynes' death. However, he never regretted his retiring attitude during those years and recently commented that he would not have acted differently with hindsight.

> All the same, as Michal Kalecki is the first to admit, the 'Keynesian Revolution' in Western academic economics is rightly so called. For without Keynes' wide sweep, his brilliant polemic, and, above all, his position within the orthodox citadel, in which he was brought up, the walls of obscuratism would have taken much longer to break[2].

The record was finally set straight by one of Keynes' most distinguished students, Joan Robinson, who, herself with proper scholarly dignity, attested that:

> Michal Kalecki's claim to priority of publication is indisputable. With proper scholarly dignity (which, however, is unfortunately rather rare among scholars) he never mentioned this fact. And, indeed, except for the authors concerned, it is not particularly interesting to know who first got into print. The interesting thing is that two thinkers, from completely different political and intellectual starting points, should come to the same conclusion. For us in Cambridge it was a great comfort. Surrounded by blank misunderstanding, there were moments when we almost began to wonder if it was we who were mad or the others. In the serious sciences, original work is *discovery* - finding connections that were always there, waiting to be seen. That this could happen in economics was a reassurance that what we had discovered was really there[3].

Kalecki's original (1933) essay on the business cycle theory withstood the test of time and great transformation in economics[4]. In an introduction to an English translation, published for the first time in

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[1] JOAN ROBINSON, in *Problems of Economic Dynamics and Planning: Essays in Honour of Michal Kalecki* (Warsaw: 1964), p. 337.

[2] JOAN ROBINSON, in *Problems of Economic Dynamics and Planning*, p. 339.

[3] *Ibid.*, p. 337.

[4] KALECKI, « Essay on Business Cycle Theory », in *Studies in the Theory of Business Cycles*, 1933-1939 (Oxford: 1966).

1966, Joan Robinson wrote: « Its sharp and concentrated statement provides a better introduction to the general theory of employment, interest, and money than any that has yet been produced »[1].

4. - Economic Dynamics and Distribution. — It is noteworthy that in some respect Kalecki's model is superior to that of Keynes. Namely, « it is explicitly dynamic; it takes income distribution as well as level into account; and it makes the important distinction between investment orders and investment outlays »[2]. On the one hand, Kalecki did not approach the theory of macroeconomic equilibrium and employment through the multiplier effect - a major contribution to economics by a brilliant student of Keynes, now Professor Lord Kahn, « which makes his version in a way less rich than Keynes', though no less forceful. On the other hand, he went straight to the theory of the trade cycle, on which Keynes was very weak »[3] Whereas Keynes' theory was couched in terms of a closed system, Kalecki extended his analysis to the crucial repercussions of the balance of payments. Another aspect that can only be mentioned *en passant* is the political divergency between Kalecki and Keynes. Whereas the latter was primarily a reformer of capitalism, the former was an advocate of socialism. It should also be noted that in a later work[4], Kalecki modified principally the analysis of the determinants of investment decisions and allowed for alternations in stockbuilding during the course of the business cycle[5].

While Keynes did not pay much attention to price theory (theory of relative prices, market structures and micro-processes of adjustments), Kalecki incorporated into his macro-analysis the theory of individual prices (distribution of produce, determinants of the relative share of wages in income, long-run and short-run changes in the distribution of income) and effective demand (finding markets for national product at full utilization of resources at the economy's disposal) by demonstrating that the determination of gross profit margin is cardinal to the distribution of the value added (i.e. the value of products less the cost of materials procured equal to the sum of wages, overheads and profits) in industry between wage earners and capitalists, and therefore, conditions effective demand and employment. Kalecki propounded that degree of monopoly was one of the key determinants of the distribution of national income between capitalistic and labor shares, and, in particular, of the share of manual labor (the relative share of wages in the value added of manufacturing industry is determined apart from the industrial composition of the value added by the degree of monopoly and by the ratio of raw material prices to unit wage costs. According to Kalecki, a rise in the degree of monopoly or in raw material prices in relation to unit wage costs causes a fall of the relative share of wage income in the value added)[1].

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[1] Joan Robinson, Introduction to *Studies in the Theory of Business Cycles*, p. xii.

[2] Klein, *Journal of Political Economy, loc. cit.*, p. 448.

[3] Cf. Michal Kalecki, *Theory of Economic Dynamics* (London: 1965).

[4] Cf. Joan Robinson, Introduction to Studies in the *Theory of Business Cycles*, p. xi, and Paulo Sylos-Labini, *Oligopoly and Technical Progress* (Cambridge, Massachusetts: 1962, 1969).

[5] Kalecki, *Studies in the Theory of Business Cycles*, p. 1.

Kalecki made significant progress towards integrating of macroeconomic and microeconomic analysis. Kalecki avoided the more customary assumption of perfectly competitive market form of behavior and concentrated on major factors underlying changes in the degree of monopoly in modern capitalist economics. The process of concentration in industry leads to the formation of giant corporations. The emergence of firms representing a substantial share of the output of an industry appreciably influences the mode of operation of the economy and the pattern of resource allocation. Such large producers know that the price they charge influences appreciably the weighted average price of all firms producing the product in question and that, moreover, the other firms will be pushed in the same direction because their price formation depends on the industry average price. Thus, the monopolistic firm can fix prices for its product at the level higher had competitive conditions prevail.

The same behavior is displayed by other large producers and thus the degree of monopoly increases substantially. This state of affairs can be reinforced by tacit agreements (for example, price fixing by one large firm, the industrial leader, while other firms simply follow suit). In turn, tacit agreements may develop into various types of cartel agreements i. e. equivalent to full scale monopoly restrained only by the fear of new entrants to the industry. The second major influence Kalecki sees in the development of modern techniques of marketing, sales promotion through advertising, salesmen pressures, etc.. Price competition is replaced by competition in advertising campaigns, marketing drives, etc., leading to rise in degree of monopoly. Moreover, if the level of overheads (costs that generally do not vary with variation of the volume of production) should rise considerably in relation to variable costs, there will necessarily follow a « squeeze of profits » unless the ratio of sale revenue to variable costs is permitted to rise. Consequently, there may arise a tacit agreement among the producers to protect profits, i.e., to increase prices in relation to unit variable costs (for example, the increase in the cost of capital employed per unit of output as a consequence of introduction of more capital-intensive techniques tends to increase the degree of monopoly in this way). The protection of profit moves are especially apt to appear during periods of depression in the rates of economic activity. Under the circumstances of depressed business conditions, aggregate proceeds (sales revenue) would fall in the same proportion as total variable costs if the degree of monopoly remained unaltered. Simultaneously, aggregate overheads (fixed costs) fall in depression less than variable costs. This tends to prompt tacit agreements not to reduce prices in the proportion as variable costs. As a result there is a general tendency for the degree of monopoly to rise in the slump (but in some instances the opposite process of cut-throat competition may develop in a depression), a tendency which is generally reversed during prosperous times. Overheads may affect price formation, but the influence of fixed costs upon prices is much less clear-cut than that of prime costs. The degree of monopoly may, but need not necessarily, increase as a result of a rise in overheads in relation to variable cost.

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[1] Selected Essays Part II and Paolo Sylos-Labini, *Oligopoly and Technical Progress* (Cambridge, Mass., Harvard University Press, 1969) pp. 96-100 and *passim.*

In addition, Kalecki emphasizes the significance and strength of the power of trade unions upon the degree of monopoly. The existence of powerful trade unions may tend to reduce profit margins. A high ratio of profits to wages strengthens the bargaining position of trade unions in their demands, for sizable wage increases are followed by price increases (cost push inflation); this in turn would call forth new demands for wage increases. A high ratio of profits to wages cannot be maintained without prompting a tendency towards rising costs. This adverse effect upon the competitive position of a firm or an industry encourages the adoption of a policy of lower profit margins. Thus, the degree of monopoly is checked to some extent by the pressures of organized labor, and this is likely to be stronger and more effective the more powerful the trade unions are.

The changes in the degree of monopoly are not merely of crucial importance for the distribution of income between labor and capitalists, but in some instances for the distribution (redistribution) of income among the proprietors themselves. A rise in the degree of monopoly caused by growth of giant corporations results in a relative shift of income to industries dominated by big corporate interests from other industries, resulting in redistribution of income from small to big corporations in modern capitalist economies.

In Kalecki's view, long-term growth of the national product, involving satisfactory utilization of productive capacity in a modern capitalist economy is far from obvious, inter alia, because the monopolistic factors involved in price fixing are deeply rooted in the capitalist mode of production, conditioning the relationships between wages and prices both during the course of business cycles and in the long-run, and cannot be treated merely as temporary short-run price rigidities and maladjustments.

Kalecki argued that contemporary theory of growth of capitalist economies tends to consider the problem of long-run trend (relating the problem of investment decisions to the process of economic growth) in terms of a moving equilibrium rather than adopting an approach similar to that applied in the theory of business cycles (fluctuations). The approach to business cycles roughly consists of establishing two relations: one based on the impact of the forces of effective demand generated by investment expenditures upon profits and the national income, and the other concentrating on the determinants of investment decisions predominantly by the level and the rate of change of economic activity. Kalecki argued that while the first relation does not involve now particularly intricate questions, the second remains the *central piece de resistance* of modern economics.

Kalecki related the problem of determination of investment decisions to the process of long-run economic growth. He argued that in fact, the long-run trend is but a slowly changing component of a chain of short-period (short-run) situations. It has no independent entity on its own. The basic approach applied in the business cycle theory should not be abandoned in confrontation with the problems of long-run growth of the economy. The two basic relations applied in the theory of business-cycles should be formulated in such manner as to produce the long-run trend cum business cycle phenomenon. In his own analysis Kalecki started from developing a theory of the pure business cycle in a stationary economy, and at a later stage the analyses were modified to get the trend into the picture. Kalecki admits that by this separation of short-period and long-run determinants he missed certain repercussions of technical progress which condition the dynamic process as a whole[1].

5. - CAPITALIST AND DEVELOPING ECONOMIES. — In the examination of the problem of the market for (realization of) the national product in terms of the Marxian schemes of reproduction (Capital Vol. II), the position taken by Tugan - Baranovski and Rosa Luxemburg are diametrically opposite. While Tugan denies altogether that the problem of realization may constitute an obstacle to the development of capitalist economy (i.e. development depends entirely on the growth of productive capacity) Rosa Luxemburg attributes all development of the capitalist economy to the possibility of selling its wares (effective demand).

Marx's follower Rosa Luxemburg rejected altogether the possibility of a long-term expanded reproduction in « a closed capitalist system ». She maintained that the development of capitalism relied entirely on the existance of possible realization outlets outside the world capitalist system - the « external markets ». The latter were composed not only of underdeveloped economies but also included the non-capitalist sectors of developed capitalist economies such as peasant agriculture and the governmental sector. « Export » outside the sphere of capitalist sectors was considered by Luxemburg as the *spiritus movens* of development.

If Rosa Luxemburg unduly stressed the impossibility of development of capitalist economies without « external markets » and disregarded some expansion due to certain aspects of technical progress, the significance of « external markets » for the development of capitalist economies cannot be dismissed. Kalecki argues that one of the most interesting elements of Luxemburg's theory relevant to present day capitalism is the treatment of government purchases, especially of armanents, as a form of « external market », a market that plays a crucial role in the functioning of modern capitalism. True, Rosa Luxemburg did not treat the government expenditures as being of primary importance. Nor did she envisage the possibility of averting crises by the income effect that such expenditures generate. Moreover, as in the case of other « external markets », she committed the error of treating the total government expenditures as a factor of realizing surplus production, i.e., independently of the means of financing. Government expenditures, to the extent that they are financed (not offset) by budgetary deficits or by taxation of capitalists, contribute to solving the problem of effective demand because their effect - in contrast to financing government expenditures by indirect or direct taxation of workers *who largely spend what they earn* - contributes to solving the cardinal problem of effective demand, as their effect is not offset by the decline of investment and consumption expenditures.

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[1] Selected Essays, pp. 166 ff.

While this is not the place to analyze Rosa Luxemburg's argument about « external markets », it may be noted in passing that one of the flaws in her reasoning was identification of investment decisions not with individual decisions of investors but with the capitalist class investing as a whole. This class, frustrated by the knowledge that finally it will not be possible to realize the economic surplus, will see no reason in undertaking such investments. In Kalecki's views, her skepticism as to the possibility of longterm expanded reproduction is valuable since the self-propelled growth of capitalist economies cannot be taken for granted. Expanded reproduction does not necessarily ensure long-trend satisfactory utilization of productive capacities. Kalecki maintains that despite Luxemburg's erroneous claim that the development of capitalism depends entirely on « external markets », the latter are essential factors in dynamics.

Whereas the crucial problem of a developed capitalist economy is the adequacy of effective demand, as such an economy possesses a stock of productive capital which more or less matches the existing labor force and, therefore, is capable of generating a rather high per capita income, provided that its resources are fully employed, the cardinal problem of the underdeveloped (or less developed) economies is the deficiency of productive capacity rather than the question of its variable utilization (under-utilization).

In the standard, essentially « short-run » and static Keynesian system, investment is merely treated in its income-generating capacity, and the essential and fundamental properties of investment creating the additional productive capacity of the economy are not taken into account, no doubt in view of the problems that the author of the towering *General Theory* was tackling and the uses at which his analytical construct was aimed.

In underdeveloped economies, even if the meager productive capital is fully utilized, it is not of sufficient size to be capable of absorbing all available labor resources. Consequently, the standard of living persists at a very low level. This is not to deny that an underdeveloped country may suffer from a deficiency of effective demand and underutilization of even the meager capacities. But the core problem cannot be remedied by «financial tricks». Here, the central issue is deficiency of productive capacity rather than the anomaly of its under-employment. The spotlight shifts from the problem of full utilization to how capacity could be considerably and rapidly enlarged, not for the sake of creating sufficient effective demand, but in order to accelerate the expansion of this capacity as a *conditio sine qua non* for a rapid rise in national income.

The increase of investment meets with three essential obstacles: First, it is possible that private investment will not be forthcoming at a sufficiently high rate. Second, the economy may face a shortage of natural resources and human skills necessary for increasing the production of investment goods. Third, even if the first two obstacles would be overcome, there still remains the problem of adequate supply of necessities to satisfy the demand resulting from increased employment. With reference to the first obstacle, it is obvious that especially in underdeveloped countries it is difficult to reach a high level of private investment. But there is a possibility of supplementing private investments by state investments in order to reach the required level. In the case of the second obstacle, it can be assumed that certain reserves exist, especially in construction. Furthermore, a significant share of investment goods is obtained through indirect production. In such a situation, as soon as investment is increased, inflationary pressures for necessities will be generated, since their supply is limited. This difficulty be overcome by financing additional investments through taxation of profits, for it would lead to a situation where profit taxes will not rise and the capitalist consumption will not tend to increase. Because of higher employment, there will be presures on the limited supply of necessities, and their prices will rise to a level where real wages of the enlarged wage bill will be equal to the unchanged supply of necessities. Profits before taxes will increase by the amount of additional investment, but this increment will be taxed away. Thus, both profits after taxes and the real wage bill will remain unchanged. However, with increased employment, more work will be executed for the same real wages; i.e., real wages per worker will fall. Consequently, a higher level of investment will be possible. This would be a highly inequitable way of increasing investment for accelerating economic growth, argued Kalecki.

The intensity of the described basic difficulties of underdeveloped economies can be significantly attenuated if they are analyzed within the framework of a perspective plan, rather than as an abrupt acceleration of the expansion of productive capacities (take-off). In constructing such a plan, both a gradual growth of investment in relation to national income and a gradual increase of the production of necessities can be taken into account. Inflationary pressures can be prevented by planning the increase of the supply of necessities to match the growth of demand resulting from the planned increase of national income.

Parallel to it, a second basic problem of the planned development of an underdeveloped economy consists of a gradual increase in investment in relation to national income. An increase in the share of investment is synonymous with a lowering of the share of consumption in national product. In order to achieve this end equitably - Kalecki maintained - luxuries should be taxed to restrict their consumption. Thus, the gradual acceleration of the growth of national product will be accompanied by an increase in the supply of necessities, adequate to prevent potent inflationary pressures. At the same time, a larger share of investment in national income will be compensated for by a reduction of the share of consumption of luxuries in national income, achieved through direct and indirect taxation of the wealthier segments of the population.

This theoretical solution to the problem of non-inflationary growth of developing economies meets in practice with serious difficulties of a political nature. The first obstacle arises at the point of introducing the basic elements of a planned economy. Not only the size, but also the structure, of investment need to be planned since investment has to be properly allocated between production of necessities, non-essentials, and investment goods. This requires greater state intervention than that which aims at achieving full use of production capacities in the developed capitalist economies. On the one hand, the state must invest in all areas where private investment proves insufficient or unprofitable (external returns to investment), and, on the other, in activities where it would contravene desiderata.

The most difficult task is to ensure adequate growth of necessities. Let us assume that investments are earmarked for agriculture and for industry producing fertilizers in order to ensure the output of planned growth of foodstuffs. In an under developed economy, agricultural production meets with a series of constraints hampering its growth, even if agriculture were to dispose of all indispensable resources. These obstacles have their source in feudal, or semi-feudal, property relationships, in the system of land-lease, and in the domination of poor peasants by traders and money lenders. Thus, agricultural development cannot be accelerated without adequate institutional changes. An agrarian reform is by itself insufficient. The experience of many developing nations shows that, abstracting from the circumstances and manipulations of the reform, it does not solve the problem of the poor peasants' dependence on traders and money lenders. Obviously, it is much more difficult to overcome the opposition of privileged groups than, as in the case of the developed capitalist state, to introduce the state financial intervention to solve the problems of deficiency of effective demand.

Finally, there remains the problem of taxation of privileged groups in order to finance increased investment. Taxation in underdeveloped economies is itself difficult and tax evasion is a common practice. This argument is often used by the privileged groups in combating tax regulations. As a result, no essential progress can be obtained in this area.

The three above - mentioned issues - state intervention in investments to ensure its planned size and structure; the overcoming of institutional barriers for a rapid development of agriculture; and an adequate taxation of the wealthy and better-to-do segments of the population - present clearly essentially political problems. In theory, most ruling groups will agree and approve that the undertaking of such means is an economic necessity. But when it comes to its implementation, undermining a whole gamut of privileges and bested interests, the situation changes radically, and various forms of decisive opposition develop to undermine the measures.

Kalecki made seminal contributions to the theory of economic dynamics and political economy of the capitalist, developing and socialist economies.

As one of the leading econometricians of our time, Lawrence Klein attested, it is customary to attribute the recent upsurge of macroeconomic model building in econometric research to neo-Keynesian development. In fact most macroeconomic models in existence could be traced to and decomposed into ideas first found in the dynamic models of Kalecki, Kaldor Metzler and Goodwin. The latter three could have been developed as the natural extension of Kalecki's contribution to economic dynamics. Kalecki's « Theories of the 1930's are seen to be intellectual *tours de force* in the light of modern developments »[1]. But the range of Kalecki's contribution was not confined to the dynamics of the capitalist and developing economies.

Kalecki has also made monumental contributions to the theory of growth of centrally planned (socialist) economy, which shall be reviewed upon the publication of Vo. II of his ESSAYS on the theory of economic dynamics.

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G. R. FEIWEL

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[1] L. R. KLEIN, « The Role of Econometrics in Socialist Economics » in *Problems of Economic Dynamics* (Warsaw: 1964) p. 189.

EINSTEIN'S SIMPLICISM

This paper attempts to examine critically certain remarks of Einstein's concerning the simplicity of scientific theories. It distinguishes two basic types of simplicity which may be found in physical theories: simplicity of mathematical form, and poverty of the fundamental postulates or concepts. These notions are examined within the context of Einstein's own work in physics. Attempts are made to provide objective criteria for measuring both types of simplicity, and to evaluate their role in scientific methodology.

In his Herbert Spencer lecture delivered in Oxford in 1933 Einstein made the rather surprising statement:

> 'Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas'[1].

This I take to be a strong statement of a doctrine known as simplicism. I don't want to beg any questions by starting off with a rigorous definition of simplicism but some sort of preliminary characterization is desirable. Simplicism is the doctrine that some sort of simplicity is of some sort of value in a scientific theory. Two questions arise: What sort of simplicity? and What sort of value? It is these questions that I shall attempt to answer with special reference to the sort of simplicism to which Einstein adhered.

Philosophers of science, particularly those philosophers in the empiricist tradition, have been sceptical, even dismissive, of simplicism. On the other hand many scientists (e.g., Kepler, Newton, Dalton, Maupertuis, even Galileo) have supported some variant of simplicism. Schlick noted the scientists' adherence to the doctrine: '... we must yet recognize the fact that any scientist who has succeeded in representing a series of observations by means of a very simple formula (e.g., by a linear, quadratic, or exponential function) is immediately convinced that he has discovered a law'. But he, himself, dismisses the notion on the grounds that 'simplicity is obviously a wholly relative and vague concept'[2]. As it tends to be just those philosophers, who, like Schlick, treat science as a paradigm, who are most sceptical of simplicism, the issue of simplicism might contain a major difficulty for their philosophical position. They would be holding up as a paradigm a discipline which contained an element with which they profoundly disagreed. This will, of course, depend upon the strength of the grounds on which they reject simplicism; and on the significance of the role played by simplicism in science.

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[1] 'On the Method of Theoretical Physics', in ALBERT EINSTEIN, *Ideas and Opinions*, (Redman, 1956), p. 274. Hereafter cited as *IAO*.

[2] *Naturwissenschaften*, vol. 19, 1931, p. 148.

I.

Let us begin by looking at some elementary ways in which simplicity is used in science. Suppose that we are investigating the stretching of a spring under a given load and we apply, successively, loads of ½kg., 1kg., 1½kg., 2kg., measuring the extension for each load. Suppose we obtain the following results for the extension: 4.9cm., 8.12cm., 10.88cm., and 14.0cm. respectively. If these results were plotted on a graph, the line we would draw between the points obtained would be a straight one, notwithstanding the fact that the majority of the points would lie just outside it. This would give the simple relation between the extension and the load known as Hooke's law:

$$\text{extension} \propto \text{load.}$$

The deviation of the actual data-points from the line drawn for them would be accounted for by a number of suppositions: the presence of extraneous factors, the inaccuracy of the measurements. Yet to some extent this is wishful thinking, for the sole justification for Hooke's law is a number of graphs of data-points just like the one we have obtained. The possibility that more than a handful of experiments gave data-points that exactly fitted the straight line is remote; certainly more experiments must have given data-points that lie off the line, than have given data-points that lie precisely on it. Thus, by the orthodox canons of induction, Hooke's law might seem to be without precise support. (We may also add that, for any given collection of data-points, it would be possible to construct infinitely many, more complex curves that fitted the data-points as well, if not better, than the straight line, i.e., that were sufficiently straight within the region of the data-points actually obtained)[1].

A more powerful example concerns Newton's law of gravitation which states that the gravitational force between two bodies varies inversely as the square of the distance between them. It had been known since 1850 that the predictions of this law were inaccurate for the orbit of Mercury. Various hypotheses had been put forward to explain the discrepancy between theory and observation (an unobserved planet between Mercury and the sun was a favourite suggestion). Hypotheses of this nature were variously considered and rejected. It was also known that agreement between theory and observation could be achieved if Newton's law were so ammended that the force varied inversely as 2.000000016 power of the distance, instead of as the square. It might be thought that, in these circumstances, a law which agreed more accurately with observation would be preferred, but this was not the case and the ammendment of Newton's law was never widely accepted. Of course the *ad hoc* nature of the ammendment was partly responsible for this. Physicists were not content with a hypothesis for which there was no test other than the effect it had been devised to explain. But the hypothesis of an inter-Mercurial planet was equally *ad hoc* and yet had wider acceptance than the ammendment of Newton's law. This fact would seem to have its explanation in the extremely ungainly complexity of the proposed ammendment.

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[1] For an excellent statement of the process of curve-fitting, see QUINE, *Word and Object*, (M.I.T. Press, 1960), pp. 19-20.

Of course, physicists have got used to using much more complex equations that Newton's law with the proposed ammendment, and such more complex equations were known during the time of the controversy about Mercury's orbit. But as far as physicists have been concerned each increase in the complexity of the relations has been contested and very good reasons for it have had to be given. A famous example that has received much discussion in the historical and philosophical literature is that of Kepler's discovery of the laws of planetary motion. Kepler saw his task as trying to find the simplest planetary orbit that would fit Tycho Brahe's observations of Mars. Kepler first tried a circular orbit, which failed to fit the observations, then an ovoid and finally an ellipse. At each stage the requirement of simplicity gave way before the demands of accuracy[1].

A further way in which simplicity plays a role in science is in simplifying assumptions. Few natural phenomena are simple enough to be dealt with as they stand. In experimental science they are placed in artificially simple surroundings (laboratory conditions) so that certain aspects of the phenomenon are exhibited, whilst others are excluded. In theoretical science the same problem is overcome by the use of simplifying assumptions. For example, classical hydrodynamics is based on the concept of an ideal fluid (i.e., an incompressible and inviscid fluid). In nature no such fluid exists but in theoretical science the concept has proved immensely powerful. Similarly, the simplifying assumptions of homogeneity and isotropy have been of great importance in the development of theoretical cosmology, in spite of the fact that such assumptions cannot actually hold in detail.

II.

I think that the examples mentioned in the previous section indicate that simplicity, in various guises, plays an important enough role in science to be worth investigation by philosophers of science. The problem has been made more difficult by the fact that simplicity has often been conflated with other notions (e.g., aesthetic notions such as elegance and harmony, and physical concepts such as 'economy' of the type found in the so-called minimum principles). There are also a large number of different concepts of simplicity[1]. Lack of space prevents me from dealing systematically either with those concepts with which simplicity is often confused, or with more than two of the different concepts of simplicity. Moreover, the two concepts of simplicity with which I shall be concerned will only be considered within the framework of a very restricted part of science, namely Einstein's work on relativity. There seems to me to be nothing which would prevent their utilization outside this limited area, but this would need to be shown in detail[2].

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[1] For a more detailed treatment of this theme, see N. R. HANSON, *Patterns of Discovery*, (Cambridge University Press, 1965), pp. 73-84; and for a more general treatment of Kepler's work, ARTHUR KOESTLER, *The Sleepwalkers*, (Hutchinson, 1959), Part IV.

The two types of simplicity that I have singled out for attention I have called 'mathematical simplicity' and 'theoretical simplicity'. They are listed by Bunge (*op. cit.*) as 'the logical simplicity of sentences' and 'the logical simplicity of theories', respectively. As far as my present purpose is concerned 'the logical simplicity of sentences' would be a misleading description because there is no need for us to embark upon the daunting task of measuring the simplicity of *all* sentences we only need concern ourselves with those sentences which express physical laws: As for 'the logical simplicity of theories', 'theoretical simplicity' has the advantage of being shorter.

The most useful suggestion for measuring the simplicity of those sentences used to express physical laws comes from Dorothy Wrinch and Harold Jeffreys[3]. To make the problem more manageable they introduce the assumption:

> 'Every law of physics is expressible as a differential equation of finite order and degree, with rational coefficients'[4].

This is not the place to comment in detail on this assumption. Certainly it has great plausibility. Ordinary and partial differential equations are the natural mathematical expression of laws involving continuous change. When fundamentally discontinuous changes were introduced by the quantum theory, differential-difference equations, involving finite differences, were employed for their mathematical representation. In wave mechanics partial differential equations are adequate. I am not aware of any physical law which *cannot* be expressed as a differential equation. Wrinch and Jeffreys' assumption means that we only have to consider sentences of the same type, for which it should not be impossible to suggest a criterion of simplicity. Wrinch and Jeffreys suggest that those differential equations in which the laws of physics can be expressed be ordered 'such that those of low order and degree, and involving no numerical constants other than small integers and fractions with small numerators and denominators, come earliest in the sequence'[1]. This suggests the following formula for measuring the complexity, C, of a differential equation:

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[1] The most complete taxonomy of simplicity of which I'm aware has been given by MARIO BUNGE, *The Myth of Simplicity*, (Prentice-Hall, 1963), pp. 51-80, together with some destructive criticism.

[2] Their use outside physics, however, especially in biology, psychology and the social sciences, requires great case - unless one happens to believe in the positivist unified science programme.

[3] 'On Certain Fundamental Principles of Scientific Inquiry', *The Philosophical Magazine*, vol. 42, 1921, pp. 369-390.

[4] *Op. cit.*, p. 386.

$$C = O + D + N \tag{1}$$

where O is the order of the equation, D its degree and N the absolute value of its numerical coefficients. The simplicity, S, of the equation we take to be the reciprocal of its complexity[2].

$$S = \frac{1}{C} = \frac{1}{O + D + N} \tag{2}$$

However, certain difficulties arise with (1) and (2) which make them counter-intuitive. In the first place it seems most unlikely that the numerical co-efficients should count as highly as the order and degree of the equation, especially in view of that fact that the coefficients in many of the differential equations used in physics (e.g., the velocity of light c in Maxwell's equations) can be normalized to zero. According to (1) the second order equation:

$$7362 \frac{d^2y}{dx^2} + 234 \frac{dy}{dx} + 23 = 0 \tag{3}$$

has the complexity index:

$$C = 2 + 1 + 7619 = 7622.$$

On the other hand, the third order, sixth degree equation:

$$\left(\frac{d^3y}{dx^3}\right)^6 + \frac{d^2y}{dx^2} + \frac{dy}{dx} = 0 \tag{4}$$

will have a complexity index of:

$$C = 3 + 6 + 3 = 12.$$

We thus come to the conclusion that (3) is *very much* more complex than (4), which is far from the case.

An obvious solution to this problem would be to add some smoothing function of N in (1) instead of N itself. Log N would be admirable for this purpose. Thus (1) becomes:

$$C = O + D + \log N \tag{5}$$

and (2) is altered accordingly. Equation (5) now gives for the complexity index of (3):

$$C = 2 + 1 + 3.8819 = 6.8819,$$

whilst that of (4) is now:

$$C = 3 + 6 + 0.4771 = 9.4771.$$

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[1] *Op. cit.*, p. 390. This, of course, does not prevent a group of equations occupying the same position in the sequence. Elsewhere Jeffreys suggests the adoption of 'some convention (such as order or degree) for the ordering of equations in the same group'. (HAROLD JEFFREYS, *Scientific Inference*, (1st edn., Cambridge University Press, 1931), p. 45).

[2] Hereafter, C and S will be called 'the complexity index' and 'the simplicity index' of an equation, respectively.

This comparison agrees better with our intuitions. The drawback to using log N is that the logarithm of a coefficient between 0 and 1 would have a negative characteristic and this would have the result of making the equation more simple. Indeed, an equation with a very small coefficient (e.g., the reciprocal of the square of the velocity of light) would be quite likely to end up with a negative complexity index, which is absurd. Moreover, we would expect an equation with a very small coefficient to be just as complex as one with a very large one. For example, we would expect the equation:

$$10^{-5} \frac{dy}{dx} = 0 \tag{6}$$

to be roughly as complex as:

$$10^{5} \frac{dy}{dx} = 0. \tag{7}$$

In view of the fact that we wish (6) and (7) to have the same complexity index, it would seem best to retain log N but with the convention that the sign of the characteristic be reversed whenever it is negative. I will mark this convention by adding '+' as an upper suffix, thus: '$\log^{+}N$'.

However, criterion (5) is still not satisfactory, because it fails to take account of the difference between partial and ordinary differential equations. Partial differential equations are, of course, more complex, other things being equal, than ordinary. Their most general solutions are usually difficult; and in physics it is often the case that a partial differential equation can be replaced by a sufficiently large number of ordinary differential equations. The simplest way of taking this into account would be to include the number of independent variables, V, in the criterion for C, thus:

$$C = O + D + V + \log^{+}N. \tag{8}$$

Even with this addition (8) is still too crude, because it takes insufficient account of the difficulty or ease of solving a certain equation. Admittedly, the degree, order and number of variables involved make a difference to an equation's ease of solution, but other factors are involved as well. For example, non-linear differential equations are generally more difficult to solve than linear ones, and this fact has not been taken into account so far. Of course, it would be an easy matter to add a constant to (8) to take account of an equation's non-linearity, but this simple expedient would mask a multitude of special cases, and ignore a number of similar problems. Some homogeneous non-linear equations are more simple to solve than some linear ones. There are other similar problems: such a simple operation as changing the sign of a term in certain differential equations may change them from ones with simple solutions into ones with no solution in terms of known functions. Of the two types of Abel equation, the first is easier to solve than the second, of which it is a special case. Indeed, the general solution of any type of differential equation will, as a rule, be more difficult than a special solution (if any) to which it is amenable. Consequently one feels that a criterion of simplicity should take into account those equations for which special solutions are available, and that these should be rated as more simple than those for which no special solution is available, other things being equal.

To cope with problems such as these criterion (8) might be ammended by the addition of a new factor, K, which is a constant for each type of equation, and which I will call 'the solution constant':

$$C = O + D + V + \log^{+} N + K. \tag{9}$$

It is proposed that the different types differential equations of given order and degree be arranged in order of the difficulty of their simplest solutions (this being done separately for ordinary and partial differential equations)[1]. This will give a number of sequences of types of differential equation, each sequence containing only equations of the same order and degree, with separate sequences for ordinary and partial differential equations. A value for K can be assigned to each type of equation by numbering off a sequence from one, the type of equation with the easiest solution coming first. In case the simplest solution of any type of equation is very much more difficult than that of its predecessor it would be permissible to give it a higher solution constant than that of its predecessor plus one. On this scheme more than one type of equation will have any given solution constant. For example, the value $K = 1$ will be assigned to the initial type of equation in each sequence, the value $K = 2$ to the second type of equation in each sequence, and so on. Indeed, as differential equations may, in principle, be of any order and degree, there will be an infinite number of sequences and thus an infinite number of types of equation with a given solution constant. The differences in the complexity of these different types of equation all with the same solution constant will be taken care of by the occurrence of their order, degree and number of variables in criterion (9). Distinctions between the complexities of different equations which fall within the same type of equation will be made by adding the logarithm of their numerical coefficients.

It might be thought that, having embarked upon such an elaborate procedure for assigning a value, K, to each type of equation, K will be sufficient on its own to determine an equation's complexity. This is undesirable on two grounds: Firstly, we may legitimately question whether what is meant by 'the simplicity of an equation' is the same thing as is meant by its 'ease of solution'. It seems essential that the structure of an equation, as well as its ease of solution, should play some part in the determination of its simplicity. Secondly, if the programme of determining the complexity of an equation solely on the basis of K is to be carried through it would mean placing *all* types of differential equations (irrespective of their order, degree, etc.) into *one* sequence according to the difficulty of their easiest solution. Although, in general, second order differential equations are more difficult to solve than first order ones, this does not imply that every second order differential equation is more difficult to solve than every first order one. The same consideration applies to equations of different degrees and to partial *vis-a-vis* ordinary differential equations. The task of trying to amalgamate these different types of equation into one sequence would be an immense one. Moreover, it can be shown that it could not, in principle, be completed. The existence of equations of arbitrarily high order and degree ensures that the sequence would be infinitely long. Admittedly, the method of assigning values to K that I have adopted implies an infinite number of finitely long sequences. But using my method K can be given a value by considering only one sequence, whereas, by the method here rejected, no value could be assigned with any certainty to K (especially if the equation under consideration has only a difficult solution) until the whole sequence had been worked out and this, as we have seen, could not be achieved in a finite number of steps.

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[1] Some idea of what such a sequence of ordinary differential differential equations would look like can be seen from G. M. MURPHY's excellent and comprehensive taxonomy, *Ordinary Differential Equations and their Solutions*, (van Nostrand, 1960), where ordinary differential equations are classed according to their order and degree and then ordered within each class according, approximately, to the degree of difficulty of their easiest solution. So far as I know, no similar treatment of partial differential equations exists.

The valid objection that can be raised against (9) is, indeed, the exact opposite of the one considered above. It is that (9) gives too much weight to the ease of solution of an equation *vis-a-vis* its structure. For example, in the sequence of first order, first degree equations listed by Murphy (*op. cit.*) there are 25 types of equation. Assuming that they are listed in exact order of difficulty of solution, and that it is not the case that one of them is very much more difficult than its predecessor, the solution constant of the 25th type of equation will be 25. An example of this type of equation is:[1]

$$x^2 - y^2 - \frac{dy}{dx} = 0. \tag{10}$$

The complexity index for (10), according to criterion (9), is:

$$C = 1 + 1 + 1 + \log^{+}3 + 25 = 28.4771.$$

Clearly the solution constant K has an entirely disproportionate influence on the complexity index of equations with small coefficients. For this reason it is desirable to ammend (9) by adding the logarithm of K instead of K itself, so that (9) becomes:

$$C = O + D + V + \log^{+} N + \log K. \tag{11}$$

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[1] Cf. MURPHY, *op. cit.*, p. 28, equation 40.

Although (11) gives so far as I am aware, results which accord well with our intuitions for ordinary and partial differential equations, it is still not a complete specification of the complexity of any type of differential equation because it leaves out the special complexities of integro-differential equations and differential-difference equations. Both of these types of equations have important applications in physics, the first in areas such as the theory of elasticity which involve what Volterra called 'hereditary laws', and the second in the early quantum theory. There is not space here to deal with these problems, although such equations ought to be covered by a fully adequate criterion of mathematical complexity. What I hope to have done in this section is to show that a rigorous criterion for the simplicity of an equation is attainable, and that the result will not differ too much from our intuitions of mathematical simplicity.

III.

Given that a precise measure of mathematical simplicity is possible, it remains to be seen of what use it might be in physics. As has been suggested in section I, given any set of data what can be satisfied by two functions, one much simpler than the other, then the simpler function is preferred by virtually all scientists. Ironically, this fact has been accepted most strongly by those empiricist philosophers who deny any maxim of the sort that asserts the simplicity of nature. They have done this usually on the grounds that the simpler function is the easiest to work with or the easiest to comprehend. Bertrand Russell, for example, noted this:

> 'What science does, in fact, is to select the *simplest* formula that will fit the facts. But this, quite obviously, is merely a methodological precept, not a law of Nature. If the simplest formula ceases, after a time, to be applicable, the simplest formula that remains applicable is selected, and science has no sense that an axiom has been falsified. We are thus left with the brute fact that, in many departments of science, quite simple laws have hitherto been found to hold. This fact cannot be regarded as having any *a priori* ground'[1].

Similarly, the early Wittgenstein expressed the view that:

> 'The procedure of induction consists in accepting as true the *simplest* law that can be reconciled with our experiences. This procedure, however, has no logical justification, but only a psychological one'[2].

These views most probably stem from those of the early positivists. Mach's view of a scientific theory was that it was the simplest way of stating the facts, a way that had advantages in ease of manipulation and economy of work. The simpler a theory is the greater will be these advantages[1]. Mach's doctrine applies equally well to the mathematical simplicity of laws just discussed, and to the simplicity of theories which we will consider in section IV below. Mach's views were echoed by scientifically minded philosophers and philosophically minded scientists in the same positivist tradition[2].

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[1] 'On the Notion of a Cause', *Mysticism and Logic,* (Allen & Unwin, 1963), p. 148.

[2] *Tractatus Logico-Philosophicus,* (Routledge & Kegan-Paul, 1961), 6.363 - 6.3631. It is interesting to note that the first sentence of this quotation is very similar to several of Einstein's simplicist statements. The qualification expressed in the second sentence, however, was one Einstein rarely made in his philosophical writings. In his popular scientific writings, however, he was occasionally more cautious. Cf., for example, 'On the Generalized Theory, of Gravitation,' *IAO.*, pp. 342, 349.

In his youth Einstein was much influenced by Machian positivism. In his autobiography he says of Mach's *Science of Mechanics* 'this book excercised a profound influence upon me ... while I was a student. I see Mach's greatness in his incorruptible skepticism and independence; in my younger years, however, Mach's epistemological position also influenced me very greatly, a position which today appears to me to be essentially untenable'[3]. We also know that this influence extended beyond Einstein's student years, through the period of work on special relativity until his work on general relativity. In an obituary of Mach, Einstein wrote that Mach's ideas had been 'directly and indirectly a great help in my work'[4]. And he even went so far as to say:

> 'Mach recognized the weak spots of classical mechanics and was not very far from demanding a general theory of relativity half a century ago.... It is not improbable that Mach would have discovered the theory of relativity if the problem of the constancy of the velocity of light had been discussed among physicists when his mind was still young and flexible'[5].

Nonetheless, between 1905 and 1921 Mach's epistemology lost its hold on Einstein. Two elements in Einstein's own scientific work may have been responsible for this[6]: The first was Einstein's early work on Brownian motion and molecular dimensions. This work is generally accepted as providing the first definite proof of the existence of atoms. Mach, on the other hand, had always regarded atoms as convenient theoretical fictions without real existence. This difference of opinion may have led Einstein to feel that Mach's epistemology did not do justice to science[7]. Secondly, while working on general relativity Einstein used criteria which fell far outside the orthodox Machian canon, and the resulting theory was further removed from empirical test than Mach would have considered desirable[1]. The reasoning that led to the general theory of relativity also led to Einstein's simplicism. In this Einstein may have felt that he was reacting against Mach's epistemology - certainly his later simplicism offers a significant contrast with what is often termed his 'early positivism'. But to see the development of his philosophy of science as an abrupt transition from an early (fruitful) positivism to a later (sterile) simplicism is to ignore the subtleties of both positions. Einstein's simplicism, in some ways, just went further than Mach's. Einstein incorporated Mach's 'economy of work' doctrine into a wider and more general simplicism[2]. Because the general theory of relativity was most probably responsible for this shift in his philosophical position, it is a good starting point for a study of the precise nature of Einstein's simplicism.

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[1] See, ERNST MACH, *The Science of Mechanics*, (Open Court, 1942), pp. 481-94; and *Popular Scientific Lectures*, (Open Court, 1907), Ch. XIII.

[2] For example, KARL PEARSON, *The Grammar of Science*, (2nd edn., London, 1899), p. 96; also KIRCHHOFF: 'Mechanics is the science of motion; we define as its object the *complete* description in the *simplest* possible manner of such motions as occur in nature'. (*Mechanik*, (Leipzig, 1874), p. 1).

[3] 'Autobiographical Notes', in P. A. SCHILPP (ed.), *Albert Einstein: Philosopher - Scientist*, (Harper & Row, 1959), vol. i, p. 21.

[4] 'ERNST MACH', *Physikalische Zeitschrift*, vol. 17, 1916, pp. 101-102.

[5] *Ibid.*, p. 103. This attribution is by a long way too generous.

[6] A personal meeting between Einstein and Mach in Vienna in 1913 may also have helped to change Einstein's mind. This is not the place for a detailed consideration of the interrelation between Mach and Einstein. Such a study has been provided by Gerald Holton, 'Mach, Einstein, and the Search for Reality', in R. S. COHEN and R. J. SEEGER (eds.), *Ernst Mach: Physicist and Philosopher* (*Boston Studies in the Philosophy of Science*, vol. VI, Reidel, 1970), pp. 165-199.

[7] HOLTON (*op. cit.*, p. 195) assigns 'relatively little weight' to this suggestion.

Space does not permit a detailed exposition of general relativity so a somewhat potted version of some of the relevant points will have to suffice. Basically, the mathematical problem that Einstein faced between 1912 and 1915 was how to find a geometrical representation of gravitation. The basic mathematical tools for this undertaking were Riemannian geometry and the absolute differential calculus of Ricci and Levi-Civita. The first steps in the right direction had been taken by Minkowski in 1908, when he provided a geometrical representation of the special theory of relativity by using a four-dimensional, pseudo-Euclidean, space-time continuum, described by the following metric:

$$ds^2 = dx^2 + dy^2 + dz^3 - c^2dt^2 \qquad (12)$$

where ds is the interval or line element between two events, c is the velocity of light, and dx, dy, dz and dt are the co-ordinate differentials of the events. Equations, such as (12), which specify the line element of a given space are known as the metric of the space. In a Minkowski world reference systems which are in uniform motion relative to each other may be represented by rotations of the t-axis of the co-ordinate system. And the equivalence of all such rotated co-ordinate systems is the geometrical representation of Einstein's principle of special relativity, which states that all natural laws are equivalent for all uniformly moving reference systems.

The train of thought that led Einstein to the general theory of relativity started with the attempt to generalize the special principle of relativity for all motions, so that natural laws would be equivalent for arbitrarily moving reference systems. The principle of equivalence, stating the equality of inertial and gravitational forces, provided the connection between this programme and the development of a new theory of gravitation. If uniform motions could be represented geometrically by the rotation of the t-axis of a Cartesian co-ordinate system employed by Minkowski, then it seemed probable that arbitrary motions could be represented by arbitrary (Gaussian) co-ordinate systems. The mathematics of such systems had been considered by Riemann. The Riemannian line element is given by:

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[1] Ironically, it was in the general theory that Einstein's scientific debt to Mach is most obvious. Einstein, himself, was responsible for coining the term 'Mach's principle' for the doctrine that rotation is not absolute but takes place relative to the fixed stars, which played a considerable part in the formulation of the general theory of relativity.

[2] Cf. EINSTEIN's remark: 'There is another, more subtle, motive [for constructing new theories] of no less importance. This is the striving toward unification and simplification of the premisses of the theory as a whole (i.e., Mach's principle of economy, interpreted as a logical principle).' ('On the Generalized Theory of Gravitation', *IAO.*, p. 342).

$$ds^2 = \sum_{i,k=1}^{n} g_{ik}\, dx_i\, dx_k \tag{13}$$

where dx_i, dx_k are the co-ordinate differentials, and where the g_{ik} are continuous and twice differentiable functions of the co-ordinates, and satisfy the symmetry: $g_{ik} = g_{ki}$, and n is the number of dimensions. Gauss investigated the two-dimensional case where he found that the geometries satisfying equation (13) could each by characterized by specifying a scalar known sa the Gaussian curvature, R. Riemann generalized this procedure for space of n-dimensions and found that such spaces could be characterized by a fourth rank tensor, R_{ijkm}, known as the Riemann curvature tensor. The symmetry properties of this tensor are such that it has only one independent component for $n = 2$ (hence Gauss could characterise a two-dimensional space with the scalar R); 6 for $n = 3$; and 20 for $n = 4$, the case in which we are interested.

Einstein, in about 1912, saw his task as the specification of the physical geometry of the universe by formulating field equations for the g_{ik}. The mathematically most simple field equations which we could devise for the g_{ik} would be:

$$R_{ijkm} = 0. \tag{14}$$

As field equations of general relativity (14) had two important defects. In the first place they yeild a flat space with a Minkowskian metric which, as we have seen, is inadequate for the general principle of relativity. Secondly, the metrical tensor g_{ik} has, on account of its symmetry properties, only ten independent components, whilst there are twenty equations (14). Our system is thus over-determined. What is clearly required is not a fourth rank tensor but a second rank one, which would have ten components in four-dimensions. Such a tensor can easily be constructed from the Riemann curvature tensor by contracting over the indices j and m, to give the contracted curvature tensor, $R_{ik} = R_{ki}$, which has ten independent components. The simplest field equations we can construct from this tensor are:

$$R_{ik} = 0. \tag{15}$$

These are in fact the field equations of general relativity for empty space and were proposed by Einstein, and his friend Marcel Grossmann, as early as 1913[1].

However, in the same paper in which Einstein proposed equations (15) he also rejected them. He had two reasons for this: firstly, he thought that he could show that a law of gravitation which was invariant under arbitrary transformations was inconsistent with the principle of causality. This turned out to be an error. Secondly, he could find no physical interpretation of (15). In his earlier work Einstein had always adhered to the view that the mathematics should be amenable to a fairly direct physical interpretation; this dictum had indeed contributed considerably to his early success (for example, the physical interpretation he gave to the co-ordinates in special relativity was both breath-takingly direct and also the foundation for the whole theory). This doctrine of physical immediacy Einstein almost certainly got from Mach and Hume. It is clear that he attempted to carry it over into the general theory of relativity (as can be seen from his early papers on the subject) but that there it was not as helpful as he had expected. Although he always regarded physical immediacy as desirable, after general relativity he no longer thought it as desirable as he once had. In general relativity, therefore, we may have a decisive factor in his break with positivism and in the development of his simplicism.

Einstein's failure to find a physical interpretation of equations (15) cost him, in his own words, 'two years of excessively hard work, until I finally recognized [it as a mistake] at the end of 1915 and ... succeeded in linking the theory with the facts of astronomical experience'[2]. The problem was eventually solved by the realization that for infinitesimal co-ordinate transformations Minkowski's metric was adequate. This enabled Einstein to rid equations (15) of those terms for which he was unable to find a physical interpretation. In other words, equations (15) were the results he had been looking for, and his intuitions of mathematical simplicity had proved more trustworthy than those of physical immediacy.

Nonetheless, equations (15) give no support to Einstein's remark, quoted at the beginning of this paper, that 'nature is the realization of the simplest conceivable mathematical ideas'. The apparent simplicity of equations (15) results from their tensor notation. In fact the contracted curvature tensor is an enormously complex non-linear, differential operator, far more complex than anything hitherto found in mathematical physics. In Newton's theory the gravitational potential was a single function; in Einstein's its place was taken by the ten independent g_{ik}. Indeed, the physical interpretation Einstein sought was only achieved by the use of a simplifying assumption, namely, that because gravitational forces were so weak the geometry of space-time would be Minkowskian over infinitely small regions.

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[1] EINSTEIN and GROSSMANN, 'Entwurf einer Verallgemeinerten Relativitätstheorie und eine Theorie der Gravitation', *Zeitschrift für Mathematik und Physik*, vol. 62, 1913, pp. 225-261. For our purposes in this article it is not necessary to consider the more general field equations in the presence of matter.

[2] 'Notes on the Origin of the General Theory of Relativity', *IAO.*, p. 289.

Consequently, the claim that equations (15) are simple can only be taken seriously on certain conditions. Given the following conditions it does, however, seem possible to claim that (15) are the simplest field equations that it is possible to construct that satisfy all the conditions:

(1) Physical space is specified by a Riemannian metric of the type (13).
(2) Physical space is four-dimensional.
(3) The field equations must be generally covariant. That is they must hold under arbitrary curvilinear transformation.
(4) The g_{ik} must be neither over- nor under-determined by the field equations.
(5) The field equations must be of second order.

It can be said that equations (15) are the simplest that can satisfy all these conditions. Indeed, there is no sense in talking about simplicity *simpliciter*. The statement 'Equation *X* is the simplest' is meaningless, we want to ask 'For what?' It is only when we get an answer to this question, in the form of certain conditions such as have been specified above, that we can make sense of any simplicity claim concerning an equation. Failure to realize this has been the main cause of the very genuine bewilderment of philosophers of science when confronted with simplicism. For this reason, they have tended to dismiss simplicity, as Schlick did, as 'a wholly vague and relative concept'[1]. It is only when the conditions which specify the purpose of an equation are given that we can attach a precise and objective meaning to simplicity claims.

Another example might be taken from Einstein's last unified field theory, the theory of the non-symmetric field. It would take too long to develop this example in detail but Einstein, himself, was particularly explicit in assessing its simplicity. He wrote of his final set of field equations:

> 'In my opinion the theory presented here is the logically simplest relativistic field theory which is at all possible. But this does not mean that nature might not obey a more complex field theory.
>
> More complex field theories have frequently been proposed. They may be classified according to the following characteristic features:
>
> (a) Increase of the number of dimensions of the continuum...
> (b) Introduction of fields of a different kind....
> (c) Introduction of field equations of a higher order (of differentiation).
>
> In my view, such more complicated systems and their combinations should be considered only if there exist physical-empirical reasons to do so'[2].

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[1] *op. cit.*, p. 148.

[2] ALBERT EINSTEIN, *The Meaning of Relativity* (6th edn., Methuen, 1956), Appendix II, 'Relativistic Theory of the Non-Symmetric Field', p. 156. Interested readers may consult this appendix for details of the derivation of the unified field equations about which Einstein is talking.

In this passage Einstein is more explicit about the use he makes of simplicity than in any other passage of which I am aware. In the first place, he makes the point already made in this paper that an equation can only be called the simplest after a number of prior conditions have been satisfied. In fact, in Appendix II of *The Meaning of Relativity* and in his popular article, 'On the Generalized Theory of Gravitation'[1] on a slightly earlier version of the unified field equations, he explicity lists the conditions that the equations have to satisfy. Secondly, he indicates that he uses simplicity as an important factor in choosing hypotheses. In an earlier paper[2] I indicated that Einstein's later philosophy of science was hypothetico-deductive rather than inductive. In many passages in his writings on the philosophy of science he claimed that the scientist's choice of hypothesis depended upon intuition. When formulating mathematical relations as abstract as those of general relativity, however, intuition - even the intuition of an Einstein - is not going to be a very satisfactory or a very convincing guide. What Einstein, in fact, did was to adopt (perhaps sometimes with no better guide than intuition) certain postulates and then to find the simplest mathematical equation which satisfied them. In this he sought to overcome the 'obstacle [of] arbitrary choice (*embarras de richesse*)'[3] that bedevils hypothetico-deductivism.

The position our analysis suggests is the following: Claims regarding the simplicity of an equation are only intelligible within the context of a body of prior conditions satisfied by the equation. When a scientist claims that his equations are the simplest that can be formulated, he means that granted certain conditions no more simple equations could be formulated that satisfy all the conditions. (Simplicity, in this context, can then be measured by some such criterion as that suggested in section II). Can we make any further stipulation about the nature of the prior conditions? One desideratum would be that at least one of the conditions must be empirical. This would be desirable on the ground that if the equations are to have empirical content this empirical content must be introduced in the conditions that the equations are required to satisfy. Without at least one empirical precondition the equations would be empirically-empty mathematical constructions. It is, however, easier to make the stipulation, than to meet it. We have, in the philosophy of science, no really clear distinction between an empirical and a non-empirical statement. For example, should we treat the principle of general covariance, in the way that Einstein treated it, as a logical principle; or as an empirical statement, based on results such as that of the Michelson-Morley experiment; or does it have some intermediate status? Is the principle of causality empirical or logical? Einstein says it's logical, Planck says it's empirical[1]. The whole question of the status of a scientific principle (as opposed to a scientific law or a logical principle) is one to which philosophers of science are far from giving a unanimous answer. There is, moreover, a further reason why it is difficult to stick too precisely to the requirement that at least one of the preconditions should be empirical. In very abstract branches of mathematical physics there would seem to be no objection to a set of purely mathematical preconditions giving rise to an equation which only afterwards received a physical interpretation. Of course, we might want to say that until it receives its interpretation such an equation remains part of mathematics, but such demarcation disputes musn't be taken too seriously. One further point concerning the preconditions may be made, namely, that among the preconditions must be the whole of the branch or branches of mathematics relevant to the equation in question. That an equation must not violate mathematical rules is so obvious that it has not hitherto been mentioned, and there is no need to list it explicitly among the other preconditions. Thus the preconditions might contain any or all of the following types of statement: (i) Theorems of the relevant branches of mathematics; (ii) Logical principles (these, also, it is rarely necessary to state explicitly); (iii) Methodological principles; (iv) Scientific principles (however their status is regarded); (v) Scientific laws and/or particular statements with empirical content; (vi) Dictionary entries (i.e., statements linking physical concepts with their mathematical representation). In many cases it will be difficult to tell which condition is of which type of statement, but I think the above gives a comprehensive list of the types of statement which may be involved.

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[1] 'On the Generalized Theory of Gravitation', *IAO.*, pp. 341-356.

[2] NICHOLAS GRIFFIN, 'Einstein's Philosophy of Science', « *Scientia* », January-February 1971, pp. 25-37.

[3] ALBERT EINSTEIN, 'Reply to Criticisms' in SCHILPP, *op. cit.*, vol. ii, p. 680.

The precise nature of the preconditions matters much less than their existence. Their existence permits, at least in principle, a precise and objective analysis of simplicity claims. Moreover, they enable us to see that the criterion of mathematical simplicity, although it is used consistently as part of the physicists' methodological apparatus, provides no *a priori* justification of a scientific law. Despite the fact that the simplest laws are always sought (relative to some body of assumptions) this procedure is no source of *a priori* knowledge; nor does it provide any evidence for metaphysical or religious claims about the simplicity of Nature[2]. That Einstein realized this is shown by some of his more guarded statements about the philosophy of science, for example, his remarks about the unified field theory already quoted. We can, moreover, see a sense not only in which the simplest laws are always *sought*, but in which they are always *found* as well. Suppose that equation E is the mathematically simplest equation that satisfies a set of preconditions a, b, c and d. E will be tested - either by comparing predictions made by means of it with observed values, or by comparing it with other desiderata in use in science at the time (e.g., a law in classical mechanics might be tested mathematically to see whether it conflicted with the principle of causality, or the conservation laws). If the law satisfies these tests then it will be scientifically acceptable. It it fails to satisfy them, then those tests that it fails will be added to the list of prior conditions that such an equation will be required to satisfy. This newly augmented set of assumptions will form the basis on which a new simplest law is constructed, this time satisfying the augmented set of conditions. For example, suppose that the simplest equation satisfying a, b, c, and d gives rise to a prediction that a phenomenon P should be observable, whereas, when the necessary experiment is performed, result Q is obtained, and, moreover, the equation is inconsistent with the conservation of mass. The procedure then is to add to the original assumptions (a, b, c, d) two new assumptions: namely, that the experimental result Q should be predictable from the equation (e); and that the law of conservation of mass be satisfied (f). The physicist then seeks the simplest possible equation which satisfies the augmented set of conditions: a, b, c, d, e, f. Eventually this procedure will issue in success, and the successful law will be the simplest one that can be formulated to satisfy a certain (however greatly augmented) set of assumptions. That this procedure is actually followed can be seen from the formulation of the field equations for empty space in general relativity which we have already examined. On the basis of conditions (1), (2), (3), and (5) listed on page 14 we see that equations $R_{ijkm} = 0$ were the simplest which would satisfy all the conditions. However $R_{ijkm} = 0$ left the g_{ik} underdetermined and so the original set of preconditions was augmented by the addition of number (4), i.e., that the g_{ik} should be neither over- nor under-determined. With the full set of conditions the equations $R_{ik} = 0$ were formulated as the simplest possible equations which satisfied all the conditions.

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[1] See, e.g., the passages quoted in Sir ARTHUR EDDINGTON'S, *New Pathways in Science*, (Cambridge University Press, 1935), pp. 297-298.

[2] No sense can be attached to the sentence 'Our world is the simplest possible' - unless it means that the universe is empty, in which case it is obviously false, indeed, self-contradictory because it uses the pronoun 'our'.

There are two other cases which merit attention. The first deals with the possibility that the first law to be discovered that satisfies a complete set of preconditions is not the most simple law that satisfies them. If this law fits the facts (i.e., generates predictions which are subsequently confirmed) and, moreover, no more simple law will fit the facts as well this one does, then the set of preconditions can be augmented. A more interesting situation arises when a more simple law is subsequently found which satisfies the preconditions and fits the facts as well as the law that was discovered first. The two laws will in fact be empirically equivalent but it is the simpler law that will be incorporated into the body of scientific knowledge. It is obvious that simpler law will be preferred because it will be easier to manipulate and thereby lead to economy of work. (There might be exceptions to this as when the original law becomes very well established within the body of scientific knowledge before the simpler law is discovered). The second case concerns a situation in which no simple law can be found. In such a situation a law can always be formulated in which the number of parameters equals the number of observations covered by the law. Such a law is, however, highly undesirable and really amounts only to the admission that no more acceptable law exists. Conventionalists have frequently mentioned this possibility in support of their argument that scientific laws can never be unequivocally refuted. It is always possible to defeat any attempted refutation of a law by a sufficient number of *ad hoc* ammendments. Thus, for example, Poincaré replied to a suggestion of Jeans and Rayleigh, who attempted to preserve the classical radiation laws in the face of the empirical evidence that gave rise to Planck's quantum theory:

> 'But this is not the role of physical theories. Physical theories must not introduce as many arbitrary constants as there are phenomena to be accounted for...'[1].

In a similar vein Einstein rejected the unified field theories of Kaluza and Klein:

> 'Having considered this possibility most carefully, I feel that it is more desirable to accept the lack of internal uniformity of the original theory, because I do not think that the totality of the hypotheses at the basis of the five-dimensional theory [of Kaluza and Klein] contains less arbitrary features than does the original theory'[2].

The admission of laws of arbitrary complexity is thus tantamount to an admission of scientific failure.

Finally, we can add that our account of the role of simplicity in hypothesis choosing fits in with the procedure of curve-fitting mentioned in section I. In curve-fitting the preconditions for a choice of hypothesis are the data-points together with some assumption as to the maximum permissible ratio of the deviation of any data-point from a proposed curve to the magnitude of the measurements made to fix the data-point. (So that if the data-point lies off the curve by a distance which is small compared with the height of the curve in the vicinity of the point it will not be taken as evidence against the accuracy of the proposed curve). If the curve that is chosen to fit the original data-points does not fit data-points that are subsequently discovered then it is discarded and a new curve suggested which satisfies both the original data-points and the new. Always the simplest curve that will fit within the margin of error prescribed will be chosen.

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[1] The Report of the Solvay Congress of 1911 quoted in A. D'ABRO, *The Rise of the New Physics*, (Dover, 1951), vol. i, p. 33. For the conventionalist, considerations of simplicity play an ultimate role in the choice of hypothesis.

[2] 'Physics and Reality', *IAO.*, pp. 311-312.

IV.

So far we have considered only part of Einstein's simplicism. Part of Einstein's simplicism consisted in the belief that the reduction of the number of logically independent, concepts in a theory was desirable. Passages in his writings support this belief. Thus in his Herbert Spencer lecture he said:

> 'It is the grand object of all theory to make [the] irreducible elements as simple and as few in number as possible, without having to renounce ... any empirical content whatsoever'[1].

In an essay written a few years later he said that physicists were 'Seeking, as far as possible, logical unity in the world picture, i.e., paucity of logical elements'[2]. It is clear that we have also to consider the overall simplicity of theories in addition to the simplicity of particular physical laws. This sort of simplicity will be called 'theoretical simplicity' and we have to try and determine some criterion by which it can be measured.

Because Einstein was a hypothetico-deductivist we will be justified in this paper in treating physical theories as deductive systems. This makes our task of finding some criterion of theoretical simplicity much easier. There are fundamentally two types of simplicity that a deductive system may possess:

(a) Axiomatic simplicity. In an axiomatically simple theory the axioms are as few and as simple as possible.

(b) Constructional simplicity. In a constructionally simple deductive system the chains of inference from axioms to theorems are as few and as simple (i.e., short) as possible.

It is clear that axiomatic simplicity and constructional simplicity are inversely proportional: as axiomatic simplicity increases constructional simplicity decreases, and vice-versa. That Einstein was aware of this is shown by a number of passages, two of the most explicit are the following:

> 'Yet more and more, as the depth of knowledge increases we must give up [the advantages of closeness to experience] in our quest for logical simplicity and uniformity in the foundations of physical theory'[3].

And the converse:

> 'It is characteristic of [phenomenological] physics that it makes as much use as possible of concepts which are close to experience, but, for this reason, has to give up, to a large extent, unity in the foundations'[4].

A constructionally simple scientific theory would belong to what

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[1] 'On the Method of Theoretical Physics', *IAO.*, p. 272.

[2] 'Physics and Reality', *IAO.*, p. 293.

[3] 'On the Generalized Theory of Gravitation', *IAO.*, p. 349.

[4] 'Physics and Reality', *IAO.*, p. 302.

I have elsewhere called the 'classificatory science'[1]; the deeper reaches of theoretical physics involve axiomatic simplicity.

We have, therefore, to choose which type of theoretical simplicity we want. Fortunately, Einstein was specific about this. His most unequivocal remark was a marginal comment he added to the last chapter of Max Born's *Natural Philosophy of Cause and Chance.* In this book Born put this choice of simplicities as a choice between 'the logical simplicity of the foundations' and the simplicity of the mathematical formalism. Against this passage Einstein wrote on the manuscript: 'The only thing which matters is the *logical* simplicity of the *foundations*'[2].

There are grave difficulties in measuring the degree of axiomatic simplicity possessed by a theory. Moreover, the value of such a measure will be limited because few theories have the degree of rigour that permits their formulation as formally deductive systems. Despite the efforts of Woodger in biology, physics is still virtually the only science in which this has proved possible with even a minimum degree of rigour. Moreover, it could plausibly be maintained that once a theory has reached this stage of logical development it is fossilized and even a successful programme of axiomatization would no longer be of interest to scientists working in the field. 'When physical theories become definable with mathematical rigour, it may indicate that rigor mortis is setting in!'[3] Against this contention it can be objected that axiomatization can yeild valuable insights into the logical inter-relations of the concepts and laws employed. No one could doubt that analytical mechanics has been immensely fruitful in providing the physicist with a clearer view of the concepts he employs and in suggesting new insights and new programmes of research[4].

We cannot take the mere number of axioms as any guide to a theory's axiomatic simplicity for, by conjunction, the number of axioms may always be reduced to one. For this reason the rough characterization of axiomatic simplicity given above included the stipulation that the axioms should be as simple and as few in number as possible. This characterization has its own problems for the simplicity and paucity of the axioms may be inversely proportional; reduction of the number of axioms may, under certain circumstances such as conjunction, lead to an increase in the complexity of those that remain. Moreover, even if some sense could be identified in which a reduction in the number of axioms did not lead to an increase in their individual complexity, the attempt to minimize both the number and the complexity of the axioms falls foul of a theorem in game theory which forbids the minimization of two variables. To say that scientists seek the smallest number of simplest postulates is as meaningless as to say that you seek to type the largest possible number of words in the shortest possible time. We could seek to find the smallest number of axioms of a given complexity, or, for a given number of axioms, find those that are as simple as possible. But it would be impossible to specify in advance either a required number of axioms or a minimum level of simplicity.

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[1] 'Einstein's Philosophy of Science', *op. cit.*, pp. 30-31.

[2] Einstein's comment, together with the appropriate passage from the *Natural Philosophy of Cause and Chance* and some background material, is given in *The Born-Einstein Letters*, edited by MAX BORN, (Macmillan, 1971), pp. 163-164. (The italics are Einstein's). Unfortunately, not all of Einstein's marginalia are published. See also, *IAO.*, pp. 322, 344.

[3] E. J. KONOPINSKI, *Classical Descriptions of Motion*, (W. H. Freeman, 1969), p. 3.

[4] Cp. KONOPINSKI: 'This introduction of *deductive* procedure accounts for the spectacular progress made in physical theory'. (*op. cit.*, p. 2); and '... physical conceptions can rarely be adequately understood unless they are expressed in mathematical language'. (*ibid.*, p. 5). Konopinski's hesitations suggest that he feels that although mathematical rigour may be useful you can have too much of a good thing.

Clearly, some other approach must be found. The most hopeful suggestion is that axiomatic simplicity consists in reducing the number of axioms to a minimum and simultaneously ensuring that the axioms are logically independent of each other. This suggestion is in line with a number of Einstein's remarks on the subject:

> '... [as] the distance in thought between the fundamental concepts and laws on the one side and, on the other, the conclusions which have to be brought into relation with our experience grows larger and larger, the simpler the logical structure becomes - that is to say, the smaller the number of logically independent conceptual elements which are found necessary to support the structure'[1].

> '... conceptual systems ... aim at the greatest possible sparsity of their logically independent elements (basic concepts and axioms), i.e., undefined concepts and underived propositions'[2].

There are two sorts of logical independence required here. In the first place the axioms should be logically independent of each other, no axiom should be derivable from any one, or any combination, of the others (i.e., no axiom should be redundant). Secondly, no axiom should contain propositions which could stand independently as axioms. I shall call axioms which satisfy these two conditions 'proper axioms'.[3] There is a problem here as to what precisely is meant by 'contains'. Different philosophers will give different answers. Those who retain a reductionist view will hold that a proposition *p* contains a proposition *q* if and only if *p* can be analyzed in such a way that *q* can be shown to be one of its constituents. Even then philosophers will differ depending upon the techniques of logical analysis that they hold to be available. Phenomenalists will hold that proper axioms are sense-datum statements; physicalists will hold that they are statements about physical objects. I have purposely introduced a vague term, like 'contains', so as not to tie our discussion to any particular philosophical viewpoint.

Even so we cannot yet define the axiomatically simplest theory as the theory with fewest proper axioms, because the number of proper axioms required by a deductive system will depend upon the scope of the subject matter that it covers. There is nothing to prevent an indefinite reduction of the number of proper axioms by means of an indefinite reduction in the scope of the deductive system. Clearly the generality of a scientific theory is of great importance. Einstein's own work was very often concerned with attempts to avoid specializing assumptions. 'A theory is the more impressive', he wrote, 'the greater the simplicity of its premisses is, and the more different kinds of things it relates, and the more extended its area of applicability'[1]. But we cannot ask for both a minimum number of proper axioms and a maximum area of applicability, for that again runs foul of game theory by seeking to maximize two variables. (Incidentally, Leibnitz made this very mistake in the *Théodicée* when he described ours as the best of all possible worlds because it exhibited 'the greatest simplicity in its premisses and the greatest wealth in its phenomena'[2]). Thus we cannot in general compare the axiomatic simplicity of theories with different subject matter. There are, however, some cases in which comparisons may be made. Suppose we have two theories, T_1 and T_2, both fully axiomatized, of which T_1 has the fewer proper axioms. There are five cases:

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[1] 'On the Method of Theoretical Physics', *IAO.*, pp. 272-273.

[2] 'Autobiographical Notes', in Schilpp, *op. cit.*, vol. i, p. 13.

[3] This should be taken merely as a convenient tag. No sense of impropriety attaches to axioms which do not satisfy these conditions.

Case 1: T_1 and T_2 cover exactly the same subject matter. T_1 and T_2 are in fact different axiomatizations of the same subject matter. In this case we can compare the axiomatic simplicity of the two: the theory with fewest proper axioms, T_1, will be the simplest.

Case 2: T_2 is a special case of T_1. In this case all the empirical consequences entailed by T_2 are entailed also by T_1, which has fewer proper axioms. In addition T_1 entails further empirical consequences not entailed by T_2. Thus T_1 has the advantage over T_2 not merely in the paucity of its proper axioms but also in its scope, it is therefore simpler than T_2.

Case 3: T_1 is a special case of T_2. Here, T_1 has the advantage over T_2 in fewness of proper axioms, but T_2 has the advantage in scope. In this case it is not possible unequivocally to decide which theory is the simpler.

Case 4: T_1 and T_2 have some, but not all, of their empirically testable conclusions in common. Here also the simplicity of T_1 cannot be compared with that of T_2.

Case 5: T_1 and T_2 have no empirical consequences in common. Here again the simplicities are not comparable.

There are also a number of cases in which one axiomatized theory, T_3, unites two other theories T_1, and T_2. Let N_1, N_2 and N_3 be the number of proper axioms, of T_1, T_2 and T_3, respectively. If T_3 may properly be said to combine T_1 and T_2 then every consequence of

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[1] 'Autobiographical Notes', *op. cit.*, p. 33.

[2] Leibnitz apparently sought to avoid the obvious nonsense of saying 'Ours is the simplest possible world' by a piece of more subtle but no less egregious nonsense.

T_1 and T_2 will be a consequence of T_3. There will thus be two further cases:

Case 6: T_3 has exactly the same empirical consequences as T_1 and T_2. In this case the simplicity of T_3 is comparable to that of T_1 and T_2 taken together. If $N_3 < N_1 + N_2$, then T_3 is simpler; otherwise not.

Case 7: T_3 has consequences which neither T_1 nor T_2 have. In this case the simplicities can only be compared if $N_3 \leq N_1 + N_2$ (in which case T_3 will be simpler than, or as simple as, T_1 together with T_2). If $N_3 > N_1 + N_2$ then the simplicities cannot be compared.

The defects in cases 3, 4, 5 and 7 suggest that some other measure must be found. One suggestion might be that measure of a theory's simplicity is the ratio of the number of its proper axioms to the number of its empirically testable conclusions, the simplest theory having the smallest ratio. This suggestion comes to grief for another reason. Any theory will have conclusions expressed in the form of universal generalizations each of which will cover an infinite number of empirically testable particular statements. Thus the total number of a theory's empirically testable conclusions will always be infinite, and thus the complexity of any theory will, by this criterion, be zero. Moreover, the number of conclusions of a theory can always be reduced to one by conjunction. If we try to circumvent this problem by introducing the concept of a 'proper conclusion' analogous to that of a proper axiom we revert to the original difficulty because any universal generalization which a theory entails can be replaced by a conjunction of infinitely many particular statements also entailed by the theory.

There is a further objection to the notion of axiomatic simplicity. In very many different branches of physics variational principles enable one to condense a number of mathematical formulae into one. The equations of the general theory of relativity were deduced from a single variational formula by Hilbert in 1915, and Einstein provided an alternative derivation in 1916. Most theories where laws are expressed by means of differential equations can be deduced from a variational principle. There are, however, some areas of physics which are not amenable to such treatment, for example, thermodynamics[1]. Nonetheless, their scope is great enough to form a major obstacle for any attempted account of axiomatic simplicity. The fact that a great many theories will each be deducible from a single variational principle will give them all the same (minimal) axiomatic complexity. Although this does not prevent us from measuring axiomatic simplicity we could scarcely any longer regard axiomatic simplicity as a desideratum for, or a method of distinguishing between, different theories. It would be arbitrary in the extreme simply to exclude variational formulations. It might be possible to find *a priori* grounds for preferring some other formulation but, at the moment, I can find no clue as to what these grounds might be.

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[1] In fact Helmholtz did try to base thermodynamics on a variational principle but his attempt was not successful. See, WOLFGANG YOURGRAU and STANLEY MANDELSTAM, *Variational Principles in Dynamics and Quantum Theory,* (Pitman, 1960), pp. 93-95.

Thus axiomatic simplicity in physical theories is not without its problems. Another attempt to measure theoretical simplicity is suggested by Einstein:

> 'These fundamental concepts and postulates, which cannot be further reduced logically, form an essential part of a theory. It is the grand object of all theory to make these irreducible elements as simple and as few in number as possible'[1].

Einstein's own scientific work points in this direction. In general relativity the concepts of absolute space, absolute time, action-at-a-distance, and inertial system were removed. Admittedly there were other reasons for rejecting these concepts: Einstein, in an anniversary tribute to Newton, called them 'shadowy'[2]. A further conceptual reduction in general relativity was achieved by the principle of equivalence which asserted the identity of inertial and gravitational mass. Einstein did regard such a reduction in the number of concepts as desirable *per se*. He listed as one of the achievements of general relativity that 'Inertia, gravitation, and the metrical behaviour of bodies and clocks were reduced to a single field quality'[3]. We should, however, note that only certain sorts of concepts were dropped, namely, physical concepts. If mathematical concepts were included in our list of the basic concepts of relativity we should find the list much longer than a comparable list for Newton's theory.

This suggests a new criterion for theoretical simplicity. The simplicity of a theory depends upon the number of *physical* concepts fundamental to (i.e., undefined in) the theory. But again we must make the stipulation that this depends upon the scope of the theory for the number of undefined concepts might be indefinitely reduced by an indefinite contraction of the theory's scope. Thus this new criterion of simplicity falls foul of the same difficulties that beset the criterion for axiomatic simplicity. Thus, although we can find two criteria for theoretical simplicity, neither can be worked out in detail until we have some exact criterion for measuring the scope of a theory.

We might ask, finally, what use either axiomatic simplicity or conceptual simplicity (or, rather, conceptual poverty) is in science. The most obvious pragmatic value that an axiomatically or conceptually simple theory has lies in the insight it can give into both the concepts that remain and those that are removed. Two of the most important and fruitful insights in modern physics have been given by Einstein's destructive analysis of the concepts of absolute space and time, and by the principle of equivalence. Similarly a reduction in the number of proper axioms required by a theory is almost certain to increase our knowledge and understanding of the laws of the theory and their inter-relation. But in view of Einstein's overall philosophy of science the requirements of theoretical simplicity have an extra significance. Einstein regarded science as an attempt to bring order to sense experience, the greater the degree of order the more explanatory depth the theory possesses[1]. The greater the simplicity of a theory the better it orders our experience. Einstein's ultimate hope was that the whole of physics would be united in one theory which rested upon a very small number of axioms and undefined concepts. He hoped that his unified field theory would be, if not itself such a theory, at least a decisive step towards the creation of one. Einstein regarded theoretical simplicity as an essential characteristic of science. Talking of classificatory sciences he says, 'Such a state of affairs cannot, however, satisfy a spirit which is *really scientifically minded*'[2]. Had someone said to Einstein, 'Granted that this theory is much more simple in its postulates and concepts than any other theory which covers the same ground, what special advantage does that give it?' Einstein would have been stuck for an answer. He could not have answered in terms of other, more fundamental, principles which underlie scientific endeavour. For Einstein the question would have implied a criticism of the scientific endeavour itself.

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[1] 'On the Method of Theoretical Physics', *IAO.*, p. 272. See also, the passage from his 'Autobiographical Notes' quoted above, p. 21.

[2] 'The Mechanics of Newton and their influence on the Development of Theoretical Physics', *IAO.*, p. 258.

[3] *Ibid.*, p. 260.

V.

Einstein once gave a word of advice to philosophers of science: 'If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds'[3]. This remark has proved apposite to our own inquiry. Einstein's comments on simplicity are nowhere near systematic or precise enough for the philosopher of science. They do, however, serve to point out parts of his own scientific work to which he attached importance. An examination of these parts of his scientific output suggests that there is a sense in which the scientist seeks the simplest possible equation that satisfies certain prior conditions; and that for some scientists, including Einstein, the theoretical simplicity of the foundations of a deductive theory is a desideratum and, indeed, has played an important part in motivating their own research. There are many other criteria by which simplicity may be measured and many, if not all, of these have been held desirable by some physicist at some time in the history of physics. In the interpretation of Einstein's simplicism given here there seems to be nothing which even a tough-minded philosopher of science would be compelled to reject, although there are difficulties in achieving a criterion of theoretical simplicity that is at once comprehensive and precise. This interpretation, however, gives us no ground to suppose that the universe is simple, or that it is simpler than any other possible world - indeed we can find no sense in these remarks. Nor does it give any justification for Einstein's remark, with which this paper opened, that 'nature is the realization of the simplest conceivable mathematical ideas'.

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[1] See, NICHOLAS GRIFFIN, 'Einstein's Philosophy of Science', *op. cit.*, pp. 30-32.

[2] 'Physics and Reality', *IAO.*, p. 293, (my italics).

[3] 'On the Method of Theoretical Physics', *IAO.*, p. 270.

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AGGIORNAMENTI SULL'ETEROCROMATINA

Eterocromatina ed eucromatina sono le due componenti morfologiche (discernibili ad un livello descrittivo ossia al microscopio ottico) dell'assetto cromosomico di moltissimi animali e piante. Come si dirà meglio poi, la prima è caratterizzata da compattezza e intensa colorabilità (e anche colorabilità specifica) sia nel nucleo in riposo, sia durante la divisione cellulare; essa è sempre in quantità minore dell'eucromatina, che è meno compatta e colorabile della precedente.

All'eterocromatina è sembrato di poter attribuire il significato di sostanza geneticamente meno attiva dell'eucromatina o addirittura di sostanza geneticamente inerte, e di qui sorge l'interesse di indagarne la funzione (tuttora assai incompletamente nota) per rendersi conto, in definitiva, perchè una parte poco attiva o apparentemente inutile del patrimonio ereditario si conservi attraverso l'Evoluzione. Anzi questo solo argomento è sufficiente a far pensare che l'eterocromatina debba significare qualche cosa che, tecnicamente, è sempre stato difficile dimostrare. Prima di entrare nella discussione di ciò che sia e a che cosa serva l'eterocromatina, converrà notare che si può affermarne la presenza solo quando essa sia in quantità sufficiente per farsi riconoscere al microscopio, e che quando si dice che l'eterocromatina (e la cromatina in generale) è una sostanza, ciò significa che essa è un sistema di sostanze chimicamente definite, ancora non del tutto noto, fra le quali sicuramente si trovano obbligatoriamente acido deossiribonucleico (DNA) e istoni. Ci sono specie nelle quali la cromatina compatta e fortemente colorabile cui dal 1928 (Heitz) si dà il nome di eterocromatina è un elemento costante di tutte le cellule: oggi si usa chiamarla *eterocromatina costitutiva*. Altre, invece, mostrano fenomeni ciclici o casuali di comparsa di eterocromatina (cioè vi sono passaggi dalla eucromatina alla eterocromatina e, raramente, dalla eterocromatina alla eucromatina); a questa eterocromatina si dà il nome di transitoria o *facoltativa*. Infine occorre tenere presente che vi sono cromosomi interi eterocromatici (il caso più noto e che discuteremo più a fondo è quello dell'Y di *Drosophila melanogaster*), e porzioni di cromosomi eterocromatiche, come quelle che si trovano accanto al centromero, o zona di attacco del cromosoma al fuso[1]. Conviene centrare l'analisi dell'eterocromatina costitutiva sul moscerino dell'aceto (*D.melanogaster*), perchè è questa la specie nella quale l'informazione è più completa.

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[1] *Centromero*, piccola zona puntiforme identificabile in quasi tutti i cromosomi, collocata in una zona fissa del cromosoma e che è responsabile dei movimenti del cromosoma stesso durante la mitosi o divisione cellulare. *Fuso* è l'insieme dei filamenti (fibre) che si differenziano nel citoplasma per fungere da supporto ai cromosomi in movimento. Il centromero si muove sulle fibre che, dopo un decorso quasi parallelo, convergono verso due punti opposti del fuso, detti *poli*.

Partiremo dai criteri descrittivi (prima solo accennati), per passare a discutere la funzione.

Il carattere di compattezza prende tecnicamente il nome di *eteropicnosi*. Nella specie di Drosofila che abbiamo prescelto per le ragioni dette prima, nelle cellule in mitosi sono eteropicnotici tutto il cromosoma Y esclusivo del maschio, una parte (1/3 - 1/2) del cromosoma X, le regioni a destra e a sinistra dei centromeri delle altre tre paia di cromosomi (il 4º paio, piccolissimo, meriterebbe una discussione a sè, ma se ne sa ancora troppo poco in proposito). La costitutività consiste nel fatto che in ogni tipo di cellula finora studiata anche dell'embrione, durante la divisione, si ripete lo stesso quadro (Barigozzi e Halfer). Quando la cellula è in riposo, tutte le parti eterocromatiche collabiscono (o pare si fondano) a formare un'unica massa cui si dà il nome di cromocentro. A questa distribuzione di eteropicnosi corrisponde perfettamente un altro carattere riscontrabile nelle cellule in divisione (Barigozzi e coll.): un ritardo (rispetto all'eucromatina) nella sintesi del DNA. Come è noto, il DNA si sintetizza nei cromosomi durante un periodo abbastanza breve durante il riposo, in quelle cellule che dovranno poi dividersi. Durante esso, cui si dà il nome di S, prima si raddoppia (o si replica) la quantità inerente all'eucromatina, poi quella dell'eterocromatina. A questi due dati sovrapponibili si è aggiunto recentissimamente (Vosa) un terzo, che però sembra sovrapporsi solo in parte: la fluorescenza dopo trattamento con quinacrina. In queste condizioni diventano fortissimamente fluorescenti il cromosoma Y e i piccoli quarti cromosomi, la parte eterocromatica del cromosoma X a quella del 3º cromosoma lo sono meno fortemente.

È forse che con questa tecnica si svelano due forme diverse di eterocromatina? È troppo presto per affermarlo. Veniamo ora alla funzione dell'eterocromatina della drosofila. La nozione classica acquisita colle prime ricerche sistematiche sulla localizzazione dei geni in *Drosophila melanogaster* ha dimostrato che quasi tutti i geni localizzabili si trovano nell'eucromatina. Coll'eccezione di un solo locus[1] del cromosoma X (*bobbed*), recentemente ristudiato, tutto fece credere che l'eterocromatina non contenesse loci noti: di qui la conclusione (oggi non più valida) che essa sia geneticamente inerte. È stato solo con tecniche piuttosto complesse e raffinate che si è potuto scoprire una serie di attività ascrivibili all'eterocromatina, che però mantengono una caratteristica che la differenzia dall'eucromatina: mentre in questa si riesce a dimostrare l'allineamento di unità (i geni), ciascuna con una funzione distinta e individuata, nell'eterocromatina le funzioni sono piuttosto da attribuirsi a sistemi o blocchi di geni con azione individuale tenue. Le funzioni attribuite al cromosoma Y di *D.melanogaster*, riguardano il controllo della dimensione cellulare e di setole (Barigozzi e coll.) e più fattori che controllano il differenziamento degli spermatidi. Il gene *bobbed* già ricordato va considerato a sè, e di ciò si riparlerà. In qualche specie del genere *Drosophila* tuttavia (*D.hydei*) nel cromosoma Y si è inoltre potuto stabilire una vera mappa di geni (o blocchi di geni) a funzione estremamente specializzata sulla differenziazione degli spermatozoi (Hess, Meyer).

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[1] *locus* punto del cromosoma in cui risiede un gene.

Le funzioni attribuite all'eterocromatina della drosofila possono anche riguardare parte delle cellule di un organo, in maniera variabile, dando luogo alla cosiddetta *variegazione*, che appare in forma di chiazze. Per esempio, una drosofila in cui, in seguito ad uno spostamento di un blocco di eterocromatina del cromosoma X (ciò che si chiama mutazione cromosomica), un locus che controlla la colorazione dell'occhio viene a trovarsi vicino all'eterocromatina stessa, se esso locus è eterozigote per $\frac{\text{bianco}}{\text{rosso}}$ invece di dare un occhio uniformemente rosso (dominante), funzionerà dando chiazze rosse e chiazze bianche. La variegatura assume aspetti molto variabili, se si confrontano individui dello stesso genotipo e della stessa generazione. Il fenomeno (che presenta diverse complicazioni, e che qui viene per brevità riassunto solo nelle sue linee essenziali) viene interpretato come un disturbo alla funzione genica arrecato dall'eterocromatina in contatto. È evidente come questa affermazione dice poco, e lascia aperti tuttavia molti interrogativi, ma svela una funzione variabile dell'eterocromatina. Da quanto si è esposto, dunque, nella drosofila (e segnatamente in *D.melanogaster*) l'eterocromatina non è affatto inerte, anche se dotata di una attività meno cospicua di quella dell'eucromatina e poco mappabile.

Le nostre nozioni riguardanti la eterocromatina costitutiva, con o senza abbinamento di informazioni microscopiche e genetiche, non si limitano però al genere *Drosophila*, anche se la completezza delle ultime è quasi sempre minore in altri organismi.

Anzitutto va ricordato il Mais, dove si riconoscono due componenti eterocromatiche, rivelate dalla loro eteropicnosi e dalle loro funzioni genetiche così limitate, tanto che la loro presenza non è obbligatoria e il loro numero può variare senza effetti fenotipici: i cromosomi B e i *knobs*. I primi sono piccoli elementi a bastoncino, i secondi sono masse o zolle intercalate nei cromosomi. Dei cromosomi B si sa che ritardano la sintesi del DNA (Abraham e coll.), come l'eterocromatina della drosofila; per i *knobs* manca questa informazione. Le caratteristiche dei cromosomi B sono state riscontrate altresì in *Crepis capillaris* ed altre piante.

Se ne parla anche a proposito di animali. Inoltre dobbiamo considerare una vastissima categoria di specie animali e vegetali nelle quali si sa che elementi eteropicnotici sintetizzano con ritardo il loro DNA, ma di cui non abbiamo alcuna notizia della relativa funzione genetica. Per citare qualche esempio, basterà ricordare il cromosoma

X delle Tettigonie (Ortotteri) e le porzioni addossate ai centromeri di molti (o tutti?) gli autosomi di diverse specie di Roditori. Anche i cromosomi eteropicnotici il cui numero è variabile in diversi Insetti si possono interpretare come esempi di eterocromatina. È però chiaro che il termine « eterocromatico » qui vale solo come indicazione descrittiva, mancandone una prova certa di ordine funzionale.

Veniamo ora all'aspetto molecolare dell'eterocromatina costitutiva. Il problema della struttura dei geni dell'eterocromatina costitutiva è stato affrontato come indagine sulla struttura del DNA che la compone e come verifica della trascrizione, cioè della possibilità del DNA dell'eterocromatina a servire come stampo per l'RNA messaggero. Disgraziatamente le ricerche su questo punto si riferiscono anche a organismi nei quali la definizione di eterocromatina non è sorretta dai criteri (ripetiamo: eteropicnosi, azione genetica ridotta, ritardo nella replica del DNA) che si utilizzano nel caso della drosofila, onde il significato delle osservazioni compiute è poco omogeneo. Cominciamo col caso già ricordato, del *locus bobbed* di *D. melanogaster* (Ritossa e Spiegelmann) che riprendiamo a considerare qui. Si è potuto dimostrare che il locus è costituito da ripetizioni di sequenza di DNA, che funzionano come stampo per l'RNA ribosomale. Nel tipo selvatico tale locus funziona regolarmente e si identifica coll'organizzatore del nucleolo stabilendo così una relazione riscontrata in materiale diverso fra la localizzazione di parti eteropicnotiche e quella del nucleolo. La mutazione *bobbed* (che si conosce in più gradi di espressività) rappresenta una riduzione del DNA per l'RNA ribosomale. Va tuttavia notato che la funzione del locus in questione si stacca dagli altri che compongono il cromosoma Y, in quanto sembra funzionare durante lo sviluppo interferendo sulle setole che sono distribuite su tutto il corpo. Una somiglianza può essere trovata coi geni che controllano la differenziazione degli spermatidi in spermatozoi, colla riserva che questi funzionano solo per un breve periodo e specificamente (a quanto se ne sa) su queste cellule soltanto.

Altre nozioni di genetica molecolare sull'eterocromatina costitutiva sono state ricavate dallo *Xenopus laevis*. In questo anfibio anuro (Wallace e Birnstiel), il mutante senza nucleolo svela la stessa situazione della drosofila, cioè la collocazione dei cistroni ripetuti per l'RNA ribosomale nell'eterocromatina. Infine ricordiamo il caso della cavia (Yunis e Yasmineh), dove esiste anche della eterocromatina costitutiva. L'applicazione della tecnica già usata da Frenster sui linfociti di vitello ai nuclei del fegato di cavia ha dimostrato che il DNA dell'eterocromatina è costituito da due frazioni di DNA satellite, una pesante e una leggera, praticamente assenti nel DNA dell'eucromatina. Se si potesse generalizzare, si dovrebbe affermare che l'eterocromatina costitutiva differisce dall'eucromatina per la struttura (anche in termini di frequenza delle quattro basi) del suo DNA.

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Fin qui è stata considerata l'eterocromatina costitutiva, cioè quella che (in drosofila e in altri forse non molti numerosi casi) in tutte le cellule di tutti i tessuti offre la stessa distribuzione. In molti organismi, tuttavia, si osservano variazioni da una fase eucromatica ad una eterocromatica (e viceversa, talvolta) che si possono classificare in tre gruppi, e in ciò consiste la facoltatività.

Un primo e vastissimo gruppo comprende quei casi nei quali determinati tipi di cellule mostrano eteropicnosi con ritardo nella replica di DNA (questa correlazione messa in luce da Lima-de-Faria sembra essere generale) e certi altri non la mostrano. Sono noti tanto esempi di animali quanto di piante, e il primo che fu descritto è quello della cavalletta *Melanoplus differentialis* nel quale l'X è eteropicnotico negli spermatociti ma non lo è nelle spermatogonie (Lima-de-Faria). Un secondo gruppo (molto vicino al precedente) comprende i casi di eteropicnosi (o eterocromatizzazione) ciclica, nei quali tutti i cromosomi derivati dal padre diventano eteropicnotici durante la spermatogenesi e vengono più tardi eliminati. Si tratta di un fenomeno tipico di alcuni Insetti (Coccidi); il caso di *Pseudococcus obscurus* è stato particolarmente chiarito tanto nella trasformazione dei cromosomi paterni in eteropicnotici, quanto nella loro acquisizione della replica tardiva del DNA. Infine v'è il gruppo, che comprende numerose specie di Mammiferi compreso l'uomo, dove si osserva una eterocromatizzazione casuale di uno dei due X presenti nel sesso femminile, come dimostra il funzionamento di un marcatore collocato nell'X, e cioè il gene per la *G6pd*[1]. L'X che diventa eteropicnotico replica il suo DNA con ritardo, forma un cromocentro durante il riposo nucleare (corpo di Barr) e cessa di funzionare geneticamente. Infatti, loci eterozigoti mostrano ora l'allele dominante ora l'allele recessivo, a seconda che si sia eterocromatizzato l'uno o l'altro degli X nelle cellule femminili. Qui (se la concordanza non trova altre spiegazioni) i tre criteri fondamentali per qualificare l'eterocromatina sono presenti come nella drosofila. Che sia uno dei due X a eterocromatizzarsi è chiaro quando il numero di questi eterocromosomi sia normale, cioè due. Se esistono dei soprannumerari, la regola è che se ne eterocromatizzi n - 1: così con 3 X si avranno due corpi di Barr ecc. La ragione di questa limitazione è ignota. La casualità della eterocromatizzazione sembra essere assoluta solo quando i due cromosomi X sono normali; se uno dei due è caratterizzato da anomalie strutturali, si eterocromatizza selettivamente il solo X normale. Le attuali conoscenze del livello strutturale molecolare di questa specie di eterocromatina possono essere valutate solo tenendo conto del fatto che manca sempre (tranne che nel caso della eterocromatizzazione casuale di uno degli X nella specie in cui esistono marcatori genetici, come l'uomo e il topo o eventualmente altre) ogni indicazione diretta del significato genetico delle parti dell'assetto eteropicnotiche e con sintesi ritardata del DNA. Con questa limitazione, va ricordato il caso del grillo (*Acheta domesticus*) studiato da Lima-de-Faria molto recentemente, probabilmente generalizzabile ed altre specie di Insetti (*Tipula* e *Coccidi*, ad esempio). In questa specie si trova un corpo eteropicnotico a DNA (il corpo di Giardina) visibile durante la fase di riposo nelle oogonie e negli ooociti, attivo nelle sintesi dell'RNA e nella formazione del nucleolo o dei nucleoli. Esso contiene DNA satellite, come nel caso citato che riguarda l'eterocromatina costitutiva della cavia, contenente 56% di Guanina-Citosina. Si può ritenere che questo dato porti a concludere che il corpo di Giardina non sia altro che una amplificazione, cioè una enorme ripetizione di cistroni per l'RNA ribosomale, corrispondente ai casi offerti dalla drosofila, dallo *Xenopus* e dalla cavia.

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[1] G6pd è una sigla per indicare un enzima detto glucososeifosfatodeido genenasi. Questo enzima è presente o assente a seconda che nel genotipo si trova l'allele dominante o recessivo del gene. Nell'eterozigote si manifesta la presenza dell'enzima (l'allele dominante); se si manifesta l'allele recessivo significa che o non c'è allele dominante o esso non funziona, come nel caso di eteropicnotizzazione.

Altri esempi dedotti da cellule altamente differenziate (linfociti di vitello, secondo Frenster) sembrano permettere una generalizzazione che nessuno potrebbe oggi estendere a tutta l'eterocromatina di tutte le specie, ma che si configura come una delle caratteristiche della cromatina compatta, che si conviene di chiamare eterocromatina, sia essa costitutiva o facoltativa.

Resta da vedere la questione del blocco d'azione dell'eterocromatina, totale o parziale, che è tuttora in fase speculativa ed è incerto fino a che punto sia estensibile alle due forme, costitutiva e facoltativa. Frenster ha recentemente ipotetizzato, con qualche base sperimentale, che l'attivazione di punti particolari dell'eterocromatina avvenga perchè molecole di RNA si appaiano con il DNA al posto di istoni staccati: l'RNA agirebbe come derepressore, al contrario degli istoni. Lo stato prevalente dell'eterocromatina consisterebbe in uno stato di repressione, con conseguente incapacità dei geni a dar luogo a trascrizione.

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Le conclusioni generali che si possono trarre dalle informazioni che abbiamo esposto criticamente non sono ancora tali da farci comprendere il significato dell'eterocromatina nel suo complesso, ma ci permettono di fissare alcuni punti che erano totalmente ignorati fino a pochi anni fa. Il fatto più inatteso è la partecipazione attiva dell'eterocromatina associata alla formazione del nucleolo a meccanismi di sintesi dell'RNA ribosomale le cui conseguenze rendono questa eterocromatina essenziale, tanto che la sua mancanza (nel mutante senza nucleolo di *Xenopus*) allo stato omozigote produce letalità. Altro fatto importantissimo è l'insostituibilità di parte dell'eterocromatina dell'Y in più specie di drosofila per la differenziazione degli spermi: senza Y il maschio non si può riprodurre. Se anche altre parti dell'eterocromatina dovessero svolgere funzioni minori di tipo modificatorio, il concetto di inerzia genetica dato un tempo all'eterocromatina, risulterebbe addirittura capovolto.

Un altro aspetto che, in questi ultimi anni, ha cambiato molto la situazione, è la categoria (ancora eterogenea) dell'eterocromatina facoltativa, che rende il concetto di eterocromatina non sempre equivalente a quello di una forma stabile, ma anche a quello di stato funzionale. Oggi è però insostenibile negare l'esistenza di una eterocromatina costitutiva, come spesso ancora si ripete (Brown), almeno in alcuni casi ben stabiliti (*Drosophila melanogaster*, cavia).

I punti oscuri, anche nella parte più chiarita, restano sempre la relazione fra stato eteropicnotico e ritardo nella sintesi del DNA e perchè questo stato o specie di cromatina debba essere sede dei cistroni ribosomali. Più comprensibile è la relazione fra blocco delle funzioni e stato compatto (eteropicnotico), in quanto il rilasciamento del filamento di DNA è, almeno intuitivamente, più logico che rappresenti la condizione necessaria alla trascrizione. Il campo che rimane ancora da esplorare è ciò che resta al di fuori dei cistroni per l'RNA ribosomale e se resta qualche cosa oltre ad essi.

Chiuderemo coll'avvertire che l'avere escluso da questa discussione il fenomeno dell'eteropicnosi negativa è dipeso dalla convinzione (ormai generalmente accolta) che questo fenomeno abbia significati estranei a quelli del fenomeno che ci si è proposti di analizzare.

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TRANSLATION: J. LUBBOCK - ROME

RECENT INFORMATION ON HETEROCHROMATIN

Heterochromatin and euchromatin are the two morphological components (discernable at a descriptive level, or under the optical microscope) of the chromosome set of a very great number of animals and plants. As will be stated more fully later, the former is characterised by compactness and intense stainability (and also specific stainability) both in the resting nucleus and during cell division; it is always present in smaller quantities than the euchromatin, which is less compact and stainable than the former. It has seemed possible to attribute to heterochromatin the significance of a substance less genetically active than euchromatin, or even of a genetically inert substance; hence the interest attached to investigating its function (our knowledge of which is still very incomplete), in other words, to discover why an inactive or apparently useless part of the hereditary endowment is preserved through evolution. Indeed, this point alone is sufficient to suggest that heterochromatin must signify something that, technically, has always been difficult to demonstrate. Before embarking upon a discussion of what heterochromatin is and what purpose it serves, it will be as well to note that its presence can be observed only when there is sufficient quantity of it to enable it to be recognised under the microscope, and that when it is said that heterochromatin (and chromatin in general) is a substance, this means that it is a system of chemically definite substances - still not entirely known - among which certainly deoxyribonucleic acid (DNA) and histones are necessarily found. There are species in which the compact and highly stainable chromatin to which, since 1928 (Heitz), the name of heterochromatin has been given, is a constant element of all the cells: today it is customary to call it *constitutive heterochromatin.* Other species, however, show the cyclic or random appearance of heterochromatin (i.e. there are passages from euchromatin to heterochromatin and, rarely, vice versa); this type is called transient or *facultative* heterochromatin. Furthermore, it must be borne in mind that there are whole heterochromatic chromosomes (the most well-known case - and the one that we shall discuss more thoroughly - is that of the Y-chromosome of *Drosophila melanogaster*) and portions of heterochromatic chromosomes, such as those found near the centromere or area of attachment of the chromosome to the spindle[1]. It will be convenient to focus our analysis upon the constitutive heterochromatin of the fruit fly (*D. melanogaster*), because this is the species about which there is the most complete information.

We will start from the descriptive criteria (previously only mentioned briefly), and then turn to a discussion of its function.

Technically, the character of compactness is given the name of *hetero-*

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[1] *Centromere*: a small puntiform area, identifiable in nearly all the chromosomes, situated in a fixed area of the chromosome and is responsible for its movements during mitosis or cell division. *Spindle*: the ensemble of the filaments (fibres) that are differentiated in the cytoplasm to act as support to the chromosomes in movement. The centromere goes along the fibres which, after running almost parallel, converge towards two opposite points of the spindle, known as *poles.*

pycnosis. In the dividing cells (in the species of Drosophila that we have selected for the above-mentioned reasons), all the Y-chromosome exclusive of the male is heteropycnotic, as well as part (1/3 - 1/2) of the X-chromosome and the regions to left and right of the centromeres of the other three pairs of chromosomes (the 4th pair, which is very small, would deserve to be discussed on its own, but too little is yet known about it in this respect). The constitutive characteristic consists in the fact that in each type of cell so far studied also in the embryo, during division, the same picture is repeated (Barigozzi and Halfer). In the resting cell of several organisms, all the heterochromatic parts unite (or appear to fuse together) to form a single mass to which the name of chromocentre is given. This distribution of heteropycnosis is perfectly matched by another characteristic to be found in the cells in division: a delay (compared with the euchromatin) in the DNA synthesis. As is well known, the DNA is synthesised in the chromosomes during a fairly short period during resting stage, in those cells that will later divide. During this period (which is given the name « S » period), first the DNA of the euchromatin doubles (or replicates) and then that of the heterochromatin. These two comparable data have very recently been joined by a third (Vosa), though this last seems to be only partially comparable: fluorescence after treatment with quinacrin. In these conditions the small 4th chromosomes become intensely fluorescent, as well as the heterochromatic part of the X-chromosome and that of the 2nd and 3rd chromosomes, though no certain conclusion has yet been reached about it. Might it be that, with this technique, two different forms of heterochromatin are revealed? It is too early to affirm this.

We now come to the function of the heterochromatin of Drosophila. The classical notion that resulted from the first systematic studies on the localisation of the genes in *Drosophila melanogaster* showed that nearly all the genes that can be mapped are found in the euchromatin. With the exception of a single locus[1] (*bobbed*) of the X-chromosome, recently restudied, everything suggested that the heterochromatin contained no known loci: hence the conclusion (no longer valid today) that it is genetically inert. Only by means of fairly complex and refined techniques it has been possible to discover a series of activities that can be ascribed to the heterochromatin, though they conserve a characteristic that differentiates it from the euchromatin: whereas in the latter it is possible to demonstrate the alignment of functional units (the genes), each having a distinct and identified function, in the heterochromatin the functions are rather to be attributed to systems or blocks of genes. The functions attributed to the Y-chromosome of *D.melanogaster* concern the control of the size of cells and bristles (Barigozzi *et al.*) and several factors that control the differentiation of the spermatids. The above-mentioned *bobbed* gene is considered on its own, and we shall refer to it again. However, in certain species of the genus *Drosophila* (*D.hydei*), in the Y-chromosome it has also been possible to establish an actual map of genes (or blocks of genes) having an extremely specialised function in the differentiation of the sperm. (Hess, Meyer).

The functions attributed to the heterochromatin of Drosophila may also concern part of the cells of an organ, in a variable manner, giving rise to the so-called *variegation*, which appears in the form of patches. For example, when, in a drosophila, as a result of a shift of a block of heterochromatin of the X-chromosome (which is called a chromosome mutation), the locus that controls the colouring of the eye finds itself near the heterochromatin itself, if this locus is heterozygous for $\frac{\text{white}}{\text{red}}$, instead of having a uniformly red eye (dominant) the drosophila will show red and white patches. The variegation assumes very variable aspects, if a comparison is made between specimens of the same genotype and the same generation. The phenomenon (which offers various complications and is summarised here, for brevity, only in its essential lines) is interpreted as a disturbance in the genic function produced by the heterochromatin in contact. It is clear that this statement says little - indeed, it leaves many questions open - but it reveals a variable function of the heterochromatin. From what has been described, therefore, in Drosophila (and particularly in *D.melanogaster*) the heterochromatin is by no means inert, even if its activity is less conspicuous and the genes cannot be mapped.

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[1] *locus*: a point in the chromosome in which a gene is situated.

However, our notions regarding the constitutive heterochromatin, with or without the support of microscopic and genetic information, are not confined to the genus *Drosophila*, even if the latter is nearly always less complete in other organisms. First of all, we may mention maize, in which two heterochromatic components are recognised, revealed by their heteropycnosis and their limited genetic functions, such that their presence is not compulsory and their number may vary without phenotypic effects: B-chromosomes and knobs. The former are small rod-shaped elements, the latter are masses or lumps interspersed along the chromosome. Regarding the B-chromosomes, it is known that they retard the DNA synthesis (Abraham *et al.*), like the heterochromatin of drosophila; as regards the knobs, no such information exists. The characteristics of the B-chromosomes have also been encountered in *Crepis capillaris* and other plants. They are also mentioned in connection with animals. In addition, we must consider a vast category of animal and vegetable species in which it is known that heteropycnotic elements synthesise their DNA with a certain delay but of which we have no information regarding their relative genetic function. By way of example, it is sufficient to mention the X-chromosome of *Tectigonia* (Orthoptera) and the portions adjacent to the centromeres of many (or all?) of the autosomes of various species of Rodents. Also the heteropycnotic chromosomes having a number that is variable in different Insects may be interpreted as examples of heterochromatin. It is however clear that the term « heterochromatic » is valid here only as a descriptive indication, since there is no certain proof of its function.

We come now to the molecular aspect of constitutive heterochromatin. The problem of the structure of the genes of constitutive heterochromatin has been approached as a study of the structure of the DNA that composes it and as proof of transcription: namely, the possibility of the DNA of the heterochromatin to serve as template for the messenger RNA. Unfortunately, studies on this point refer also to organisms in which the definition of heterochromatin is not supported by the criteria (namely, heteropycnosis, reduced genetic action and delay in DNA replication) that are employed in the case of Drosophila, and hence there is little homogeneity in the significance of the observations carried out. Let us begin with a case already mentioned: that of the bobbed locus of *D.melanogaster* (Ritossa and Spiegelmann), which we will now consider once more. It has been possible to show that the locus consists of DNA sequence repetitions, which function as template for the ribosomal RNA. In the wild type, this locus functions regularly and is identified with the organiser of the nucleolus, thus establishing a relationship encountered in different material between the localisation of heteropycnotic parts and that of the nucleolus. *bobbed* mutation (which is known in several degrees of expressivity) represents a reduction in DNA through the ribosomal RNA. However, it must be noted that the function of the locus in question is detached from the others composing the Y-chromosome, since it seems to function during the development by acting upon the bristles that are distributed all over the body. A resemblance may be found with the genes that control the differentiation of the spermatids in spermatozoa, with the reservation that the latter function only for a short period and specifically (as far as is known) on these cells only.

Other molecular genetic notions on constitutive heterochromatin have been obtained from studies on *Xenopus laevis.* In this anouran amphibian (Wallace and Birnstiel), the mutant without nucleolus shows the same situation as drosophila: namely, the arrangement of the repeated cistrons for the ribosomal RNA in the heterochromatin. Finally, we may mention the case of the guinea-pig (Yunis and Yasmineh), in which constitutive heterochromatin also exists. Application of the technique already used by Frenster on calf lymphocytes to the nuclei of guinea-pig liver has shown that the DNA of the heterochromatin is composed of two fractions of satellite DNA, one heavy and one light, practically absent in the DNA of euchromatin. If it were possible to generalise, it would have to be stated that constitutive heterochromatin differs from euchromatin in the structure (also in terms of frequency of the four bases) of its DNA.

* * *

So far the case of constitutive heterochromatin has been considered: that is, the case in which all the cells of all the tissues offer the same distribution of euchromatin and heterochromatin. In many organisms, however, variations are observed from a euchromatic to a heterochromatic stage (and sometimes vice-versa), which can be classified into three groups.

The first group, which is vast, comprises those cases in which certain types of cell display heteropycnosis with delay in DNA replication (this correlation, revealed by Lima-de-Faria, seems to be general) and certain others do not display it. Examples both of animals and of plants are known, and the first to be described was that of the grasshopper *Melanoplus differentialis*, in which the X-chromosome is heteropycnotic in the spermatocytes but not in the spermatogonia (Lima-de-Faria). A second group (very close to the first) comprises the cases of cyclic heteropycnosis (or heterochromatisation) in which all the chromosomes derived from the father become heteropycnotic during spermatogenesis and are eliminated later. This is a phenomenon typical of certain Insects (Coccida); particular study has been made of the case of *Pseudococcus obscurus*, as regards both the transformation of the paternal chromosomes into heteropycnotic chromosomes and their acquisition of delayed DNA replication. Finally there is the group, comprising numerous species of Mammals (including man), in which a random heterochromatisation of one of the two X-chromosomes present in the female sex is observed, as is shown by the functioning of a marker located in the X-chromosomes: namely, the gene for the *G6PD*[1].

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[1] G6pd is a symbol used to indicate an enzyme known as glucose-6-phosphate-dehydrogenase. This enzyme is present or absent according as to whether the dominant or the recessive allele of the gene is present in the genotype. In the heterozygote, the enzyme (the dominant allele) is seen to be present; if the recessive allele appears, it means either that there is no dominant allele or else that it is not functioning, as in the present case.

The X-chromosome, which becomes heteropycnotic, delays in replicating its DNA, forms a chromocentre during nuclear repose (Barr's body) and ceases to function genetically. In fact, heterozygous loci sometimes reveal a dominant allele and sometimes a recessive allele, according as to which one of the X-chromosomes in the female cells is heterochromatised. Here (if no other explanations are found for the concordance) the three fundamental criteria for qualifying the heterochromatin are present as in drosophila. It is clear that it is one of the two X-chromosomes that is heterochromatised when there is the normal number of these heterochromosomes: namely two. If there are supernumerary chromosomes, the rule is that (n - 1) of them are heterochromatised: that is to say, with 3 X-chromosomes, there will be two Barr bodies, and so on. The reason for this limitation is unknown. The randomness of the heterochromatisation seems to be absolute only when the two X-chromosomes are normal; if one of the two is characterised by structural anomalies, only the normal X-chromosome is selectively heterochromatised. Present-day knowledge regarding the molecular structural level of this species of heterochromatin can be assessed only by taking account of the fact that (except in the case of the random heterochromatisation of one of the X-chromosomes in the species in which genetic markers exist, as in man and the mouse or possibly others Mammals there is never any direct indication of the genetic significance of the heteropycnotic parts of the arrangement having delayed DNA synthesis. With this limitation, mention should be made of the case of the cricket (*Acheta domesticus*) very recently studied by Lima-de-Faria, probably generalisable to other species of Insects (e.g. *Tipula* and *Coccida*). In this species a heteropycnotic DNA body (the Giardina body) is found, visible during the resting stage in the oogonia and in the oocytes and active in the RNA syntheses and in the formation of the nucleolus or the nucleoli. It contains satellite DNA, as in the case cited, which concerns the constitutive heterochromatin of the guinea-pig, containing 56% of G-C.[1] This fact suggests that the Giardina body is nothing but an amplification: that is, an enormous repetition of cistrons for the ribosomal RNA, corresponding to the cases offered by drosophila, *Xenopus* and the guinea-pig.

Other examples deduced from highly differentiated cells (calf lymphocytes, according to Frenster) seem to allow a generalisation that no-one today could extend to all the heterochromatin of all the species; rather, it appears as one of the characteristics of compact chromatin, which it is convenient to call heterochromatin, whether it be constitutive or facultative.

It remains to consider the problem of the total or partial block in the action of the heterochromatin, about which there is still speculation and it is uncertain to what extent it can be extended to the two forms, constitutive and facultative. Frenster has recently advanced the theory, with a certain experimental basis, that the activation of particular points in the heterochromatin occurs because RNA molecules pair with the DNA in place of detached histones: the RNA would act as derepressor, the opposite of the action of the histones. The prevalent state of the heterochromatin would consist in a state of repression, with consequent incapacity of the genes to give rise to transcription.

* * *

The general conclusions that may be drawn from the information that we have presented in this critical account are not yet such as to enable us to understand the significance of heterochromatin as a whole, but they allow us to establish certain points about which there was total ignorance until a few years ago. The most unexpected fact is the active participation of the heterochromatin associated to the nucleolus in mechanisms of ribosomal RNA synthesis, the consequences of which make this heterochromatin essential, such that its absence (in the mutant of *Xenopus* without nucleolus) in the homozygous state produces lethality. Another very interesting fact is the irreplaceability of part of the heterochromatin of the Y-chromosome in several species of drosophila for the differentiation of the sperm: without the Y-chromosome the male cannot reproduce. If other parts of the heterochromatin were also to perform minor functions of a modificatory type, the concept of genetic inertness that was once ascribed to heterochromatin would be completely upset. Another aspect which, in recent years, has considerably changed the situation is the category (still heterogeneous) of the facultative heterochromatin, which makes the concept of heterochromatin not always equivalent to that of a stable form but rather to that of a functional state. Today, however, it is unjustifiable to deny the existence of a constitutive heterochromatin - as is still so often done (Brown) - at least in certain well-established cases (*Drosophila melanogaster*, guinea-pig).

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[1] G = guanine; C = cytosine.

Even as regards the more fully explained part, there still remain certain obscure points: namely, the relationship between the heteropycnotic state and the delay in DNA synthesis and the reason why this state or species of chromatin must be the site of the ribosomal cistrons. More understandable is the relationship between the block of the functions and the compact (heteropycnotic) state, since the release of the DNA filament is, at least intuitively, more logical, than is represented by the condition necessary for transcription. The field that still remains to be explored is that which lies outside the cistrons for the ribosomal RNA, and the question of whether there is anything apart from them.

Finally, we point out that the fact of having excluded from this discussion the phenomenon of negative heteropycnosis depended on the now generally accepted conviction that this phenomenon has significances that are extraneous to those of the phenomenon that we have set out to analyse.

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C. Barigozzi

IS THE THEORY OF EVOLUTION DIFFERENT?

II - The Structure of the Entire Theory

This is the second part of a two part paper in which I attempt to analyse modern evolutionary theory in the light of claims which have been made about its nature by a number of philosophers. In this part I begin by considering a claim by Morton Beckner to the effect that the overall structure of evolutionary theory is not (and can never be) similar to that of Newtonian Mechanics. By considering the history of evolutionary theory and by examining in detail three examples drawn from different areas of evolutionary explanation, I argue that Beckner's claim is not well taken. In particular, I argue that one can discern an overall axiomatic or « hypothetico-deductive » structure to the theory, one in which the hypotheses of population genetics act as premises, integrating diverse areas of evolutionary study. It is for this reason that I have previously referred to population genetics as the « Evolutionary Theory Core ». I argue also that given this vision of evolutionary theory, Beckner is wrong when he denies that so-called evolutionary « laws » are in any sense illuminated by the theory. I conclude the paper by considering three arguments by T. A. Goudge, all of which purport to show that the axiomatic model is inappropriate for evolutionary theory. I argue that none of Goudge's arguments are sound, and I try to show that his case rests on mistakes commonly made about science in general and evolutionary theory in particular.

3. The Overall Structure of Evolutionary Theory. — By this stage of the paper the reader may have become quite impatient. He may have agreed with just about everything which I have written so far, even to the extent of agreeing that I have shown Smart to be quite mistaken in his assessment of biology; but still the reader might feel that I have not yet begun to tackle the supposed real subject of the paper, namely the provision of an analysis of the nature of evolutionary theory. He might with some apparent justice point to the fact that that which I have called the evolutionary theory core, normally known as population genetics, is not usually even included in college courses on evolutionary biology. Such courses are about subjects like paleontology, geographical distribution, systematics, (parts of) embryology, and so on — subjects which have all been conspicuous in my discussion only by their absence. The task I should have been addressing myself to, this critical reader might claim, is that of showing how all these various branches of evolutionary biology unite to make a theory of the type I discussed in the first section.

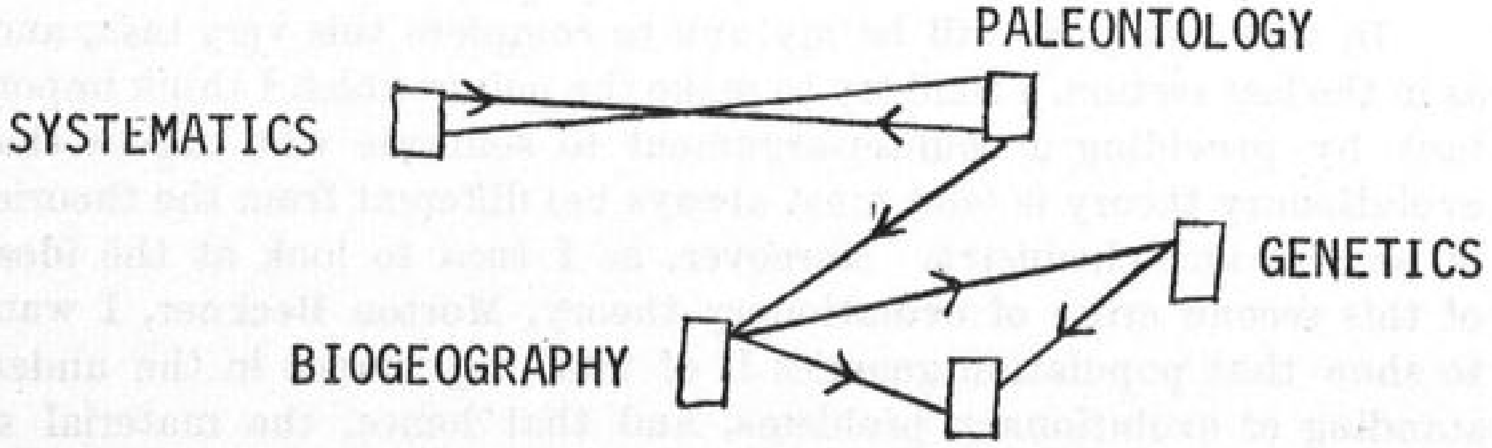
In this section it will be my aim to complete this very task, and, as in the last section, I shall try to make the points which I think important by providing a counter-argument to someone who argues that evolutionary theory is (and must always be) different from the theories of physics and chemistry. Moreover, as I turn to look at the ideas of this second critic of evolutionary theory, Morton Beckner, I want to show that population genetics is of vital importance in the understanding of evolutionary problems, and that hence, the material so far covered in this paper is not without relevance.

Beckner would agree with me, I think, in my claims about population genetics, conceding that it is part of evolutionary theory and that it is like physical theories, and he would even agree that there are other parts of evolutionary theory which are similarly like physics. Yet, despite this agreement, he would still want to deny that in its overall structure evolutionary theory is like, or ever really could be like, a theory in physics or chemistry. He writes:

> The scientific interest of modern evolution theory derives from its success in integrating into a single body of theory the results and data from the most diverse branches of biology: paleontology, genetics, ethology, systematics, biogeography, ecology, etc. ... [but] if we look in evolution theory for the pattern of theoretical explanation of Galileo's and Kepler's laws, we shall be disappointed. Evolution theory does not attain its ends by exhibiting, e.g., Williston's and Bergmann's principles as consequences of one or more hypotheses of greater generality. There are a number of small hierarchies of this character scattered about evolution theory, but the theory as a whole does not approach this type of organization. It is impossible to be dogmatic on the point, but it does seem to be true that *this fact is not due to the undeveloped state of biological subject matter* ... My own view is that evolution theory consists of a family of related models; that most evolutionary explanations are based upon assumptions that, in the individual case, are not highly confirmed; but that the various models in the theory provide evidential support for their neighbors. The subsidiary hypotheses and assumptions that are made for the sake of particular explanations in one model recur again and again in other related models, with or without modification and local adaptation. To use the metaphor of Agnes Arber, biological theory is less « linear » than, e.g., physical theory, and is more « reticulate ». (Beckner, 1959, 159-60, my italics).

If one tried to give a diagramatic representation of Beckner's view of evolutionary theory, then I think it would come out something like this.

Fig. 2.
Beckner's view of evolutionary theory.



(The rectangles stand for different branches of evolutionary biology, the arrowed lines show where conclusions of one branch are being used as premises in another. Obviously no attempt has been made at completeness, nothing should be inferred from the fact that some rectangles are higher than others, and the arrowed lines are just examples — the particular ones drawn in the diagram, do not necessarily stand for real connections).

Now clearly, it will not be a particularly simple job to show that Beckner is mistaken in his characterization of evolutionary theory. Apart from anything else, Beckner does not commit himself absolutely to this characterization, merely suggesting that it seems to him to be the most plausible. However, even if we brush over this point, as one starts to dip into the unbelievably diverse literature devoted to evolutionary studies, it rapidly becomes apparent that a position like Beckner's has great plausibility. Hardly ever does one find a whole book carrying through a rigorous formalized argument — instead, as Beckner says (if lucky) one finds shortish passages of deductive argument linked by passages which are either purely descriptive or argumentative only in a very loose sort of way. It is clear, therefore, that at best one can claim that if evolutionary theory is in some sense hypothetico-deductive, it is so only in a rather 'loose' sense. By this I mean something very much akin to what Carl Hempel means when he talks of evolutionary explanations as being merely 'sketches' [19]. All one can hope to claim is that the evolutionist has as an ideal the same model of a scientific theory as any physical scientist; but because of various immense practical difficulties, the most he can do is sketch in the major outlines of his theory. Because of the great quantity and complexity of evolutionary data, because so much vital information has been irretrievably lost in the past, because evolutionary studies are relatively new, the evolutionist must necessary omit important premises, and consequently at times he can offer only suggestive arguments rather than arguments which are rigidly deductive.

But why, the reader must be asking, should one bother to argue this way? Why bother to claim that evolutionary theory approximates to something which does not exist? Why not do as Beckner does, and accept evolutionary theory at its face value, as something which is radically different from anything which is to be found in physics or chemistry? Can one give any firm reasons why it should be accepted that evolutionary theory has the outlines of a hypothetico-deductive system, which it fails to realized completely because of practical rather than theoretical reasons?

It is not easy to find a reply to these very proper questions; but the reason why I think that evolutionary theory is better considered in a hypothetico-deductive way, rather than in the way suggested by Beckner, stems in the first place from the history of the theory. As is well known, although Darwin was the first person to create a full-scale evolutionary theory using natural selection as a major foundation, it was not until about 1930 that the modern *synthetic* theory of evolution really started to come into being. At the beginning of this century, Darwinism (*i.e.* the notion that natural selection plays a crucial evolutionary role) fell into disrepute amongst many leading biologists (particularly geneticists), and it took more than two decades before it was realized that Darwinian selection theory and Mendelian genetics complement each other, rather than form opposing theories. As soon as this was realized, then thanks to the work of theoretical biologists like J. B. S. Haldane, R. A. Fisher, and Sewell Wright, the fundamental parts of the population genetical theory, which I sketched in the second section, were rapidly developed [20]. After this happened, it would seem that the ideas and results of this theory about the genetics of populations spread rapidly to other areas of evolutionary studies — systematics, biogeography, and so on — and about 10 years later (*i.e.* around 1940) a number of important works appeàred in these latter areas [21]. These works are important in that they set the tone for the kind of work which has continued to be done in their various areas, and, although written up to 30 years ago, they are still fairly essential reading for anyone who wants to explore the various branches of evolutionary theory in some detail [22].

Now, as one studies these various works, some of which I listed at the beginning of the last section, one finds that a recurrent pattern starts to emerge. Their authors all seem to pay a great deal of attention to the efforts and results of geneticists, particularly population geneticists. Indeed, often these authors go to the extent of devoting sizable sections of their books to quite detailed expositions of topics like selection, mutation, and of results which can be derived from assumptions about selection and mutation. Moreover, in all these works, one finds that these discussions of genetics come fairly early on. For example, both Dobzhansky's *Genetics and the Origin of Species* and Huxley's *Evolution: The Modern Synthesis* are concerned with a wide spread of fairly basic evolutionary problems — species formation, the geographical distribution of species, morphological peculiarities of organisms (particularly Huxley), problems of ecology, trends in evolution, and so on — and both of them devote their early chapters to detailed expositions of results about mutation, selection, and other topics of concern to population geneticists. The same can be said of G. L. Stebbins' basic work in botany, *Variation and Evolution in Plants*, and perhaps most surprisingly, to G. G. Simpson's seminal books on evolution. Simpson approaches evolutionary studies from the viewpoint of a paleontologist; but in *The Major Features of Evolution*, at an early point, he examines in great depth genetical theory and the way that the units of inheritance behave in populations. Of course, not every evolutionary work of this period is so concerned with genetics;

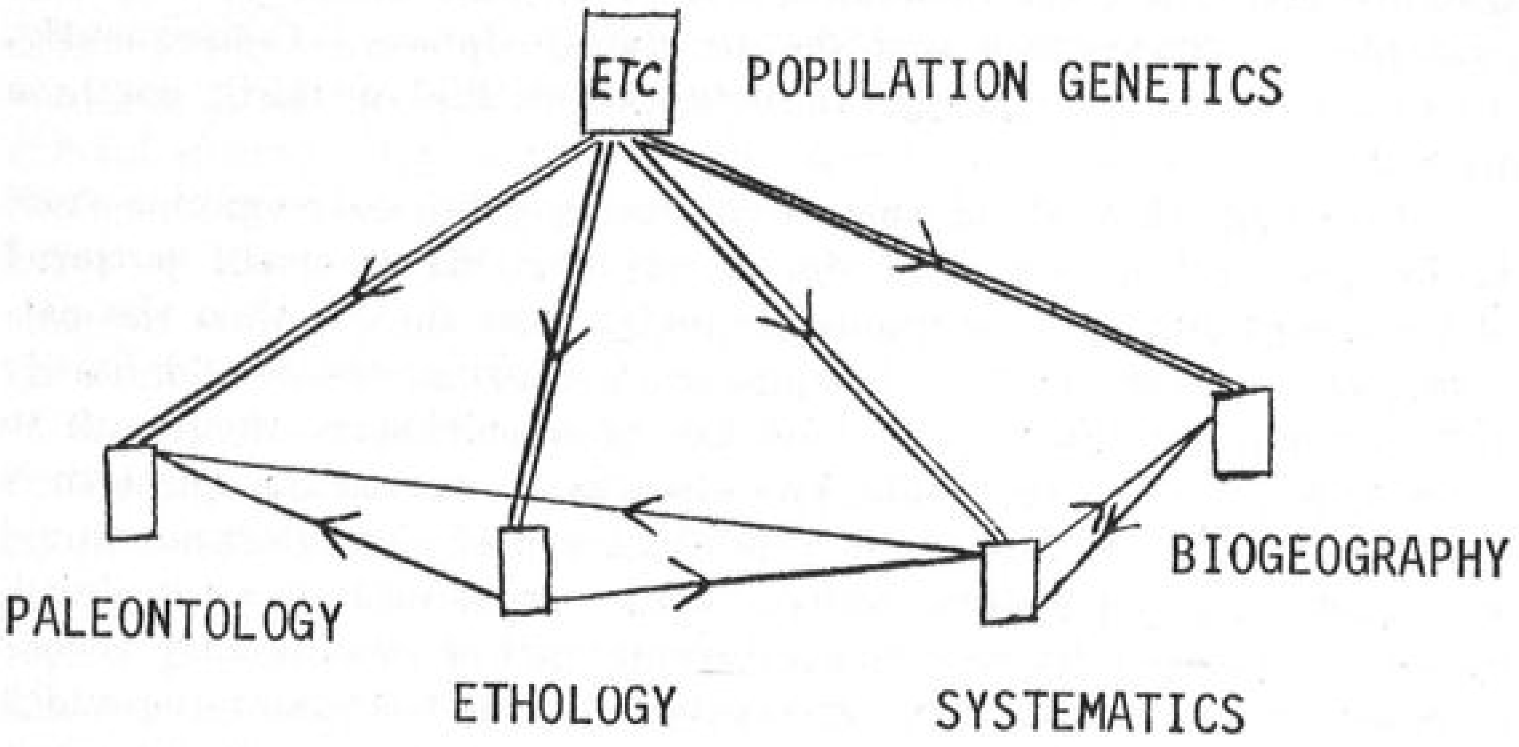
yet even works which keep genetical expositions to a minimum, acknowledge its importance, and refer readers to sources where population genetics gets discussed in detail. This was what E. Mayr does, for example, in *Systematics and the Origin of Species.* Consequently, it seems fair to say that in these works we find a fairly common pattern.

Assuming that all of these evolutionists did not organize their books quite at random, how do we explain this recurrent pattern? If we accept Beckner's account of evolutionary theory, then the pattern seems rather strange. Despite some overlap, we should hardly expect works on the various branches of evolutionary theory all to start with the same material. The obvious answer to the question is that evolutionary theory is much more structured than Beckner would allow, and that population genetics is not just something whose development was *temporally* prior to the development of evolutionary theory in branches like paleontology and systematics, but is something which is *logically* prior to the parts of the theory in these various branches. If this is so, then it is no surprise that the development of these various branches had to wait on the development of population genetics, and that when the important works appeared showing how evolutionary theory could be extended into the many branches like paleontology and systematics, their authors also thought it necessary to show how the results in the various branches depend for a full explanation on the work achieved by population geneticists. For even though these authors could not put their works in rigorous axiomatic forms, they nevertheless thought that explanatory knowledge in the various branches of evolutionary theory depends on knowledge of a jointly held theory about heritable character transmission in populations, that theory which in the last section I labelled the 'evolutionary theory core' [23].

Now, if what I am suggesting is correct, then it seems plausible to suggest that one can discern in the overall structure of evolutionary theory the outlines of a hypothetico-deductive system. In particular, we find population genetics acting as a body of upper-level hypotheses for the whole theory, and then, stemming down from this core, we find the various other branches all presupposing the core (not necessarily every part of the core — just some part). These branches are paleontology, ethology, systematics, and so on. Each branch has premises which are peculiar unto itself, and no doubt (as Beckner claims) many branches are interconnected, in particular, conclusions of one branch are sometimes used as premises by another; but the essential thing is that each branch is linked by its common relationship to one part of the theory which is logically prior to all the others, that which I call the 'ETC'.

Diagrammatically, this view of evolutionary theory which I am advocating is something like this.

Fig. 3.
Ruse's view of evolutionary theory.



(Again, no attempt has been made to give a complete picture; but in this case, the double arrowed lines, unlike the single lines, are not just examples. They really stand for actual links).

Obviously, even if this view of evolutionary theory seems plausible to the reader, I am not yet in a position to conclude that my view of evolutionary theory really is the correct one and that Beckner's is the wrong one. Even if one grants everything that I have said so far, there still remains the job of showing why one should accept that population genetics was more than just temporally first in the development and usual presentation of evolutionary theory. One can hardly argue for the logical priority of population genetics soley on the basis of the order of chapter headings in *The Major Features of Evolution* and like works. Consequently, to complete my defence of my view of evolutionary theory, I must now try to offer some arguments to show why population genetics should really be considered logically prior to all the explanations in evolutionary-branch areas.

At the outset it must be admitted that, given the loose form of evolutionary theory, like Beckner, I cannot offer any unassailable justification of my view of evolutionary theory. However, I think a persuasive case can be made for my position if we look at examples of explanations which are made in the various branches of evolutionary studies. If it can be shown, as I think it can, that a representative cross-section of explanations in these branches presuppose the ETC, then it would seem plausible to accept that the ETC really is a 'core' and that it is logically needed by the rest of evolutionary theory. Let us therefore turn to a consideration of such a cross-section of examples.

a) *An evolutionary explanation in systematics.* — The first example of an explanation of an evolutionary phenomenon that I want to discuss belongs to *systematics*, although explanations like this are of concern and interest to workers in just about every other area of evolutionary studies. From the point of view of the taxonomist or systematist [24], ideally [25], groups of organisms would be sharply distinguishable from other groups by virtue of their different morphological characteristics; moreover, organisms within the same group would be morphologically indistinguishable. In practice, it is found not only that different groups do not always look so very different, but that groups of what are obviously the same kind of organism contain quite broad ranges of morphological and behavioural characters [26]. One variety of these intra-group differences is of particular interest to evolutionists — namely, that where one has an interbreeding group reproductively isolated from other groups (*i. e.* a species), where there are several kinds of member of such a group, and where these different kinds seem to persist, despite the fact that there may well be very strong selection against one of the kinds of the group.

One might think that this kind of intra-group variation, a phenomenon known as 'polymorphism', would be a very temporary stage in the history of any species, since one might expect that very rapidly, the offspring of the successful members of the group would far outnumber the offspring of the unsuccessful members. In fact, this is so far from being the case, that evolutionists recognize that in most species 'balanced' polymorphism is the norm for some characteristics. Perhaps one of the most famous examples (certainly one of the most dramatic examples) of balanced polymorphism occurs in man. In certain parts of the world as many as 5% of the human population die in infancy or childhood from an inherited form of anemia ('sickle-cell' anemia). Since this anemia is caused by a recessive gene, this points to the fact that up to 40% of the population carry the gene (heterozygously). Obviously, a phenomenon like this calls for an explanation, since intuitively one would expect that so severe a selection against the homozygotes (for the gene) would soon reduce the incidence of the gene to minute proportions.

A tentative solution to this polymorphism might be based on the supposition that there is a high rate of mutation to the sickle-cell gene; but, in fact, the incidence of the gene is far too great to be put down to this cause. The true solution, it would seem, rests, first on the fact that the populations carrying the gene (in such high proportions) are just those which live where malaria is endemic, and secondly on the fact that the gene is not entirely recessive, but indeed confers an immunity from malaria to its heterozygotes (an immunity not possessed by the homozygotes for the normal gene) whilst not disabling the heterozygotes through anemia. Thus there is a strong selective pressure in favour of the heterozygotes over both kinds of homozygote [27].

Now, so far in this explanation of an instance of balanced polymorphism, all we have introduced is the supposition that there exist selective forces of particular kinds for particular gene carriers. It is at this point that evolutionists turn to population genetics for help, in order to show just how a selective advantage for a heterozygote will bring about a balanced polymorphism (and how, given different proportions, one can calculate different selective pressures, and *vice-versa*). Roughly, the pertinent calculations are as follows. Suppose one is considering two alleles *A* and *a*, and that the adaptive value of the heterozygote *Aa* is 1, and the adaptive values of the homozygotes *AA* and *aa* are $1\text{-}s_1$ and $1\text{-}s_2$, respectively. The frequencies of the different genotypes, before and after selection, can be calculated, thanks to the Hardy-Weinberg Law. They are as follows.

Genotype	*AA*	*Aa*	*aa*	*Total Population*
Adaptive Value	$1\text{-}s_1$	1	$1\text{-}s_2$	
Initial Frequency	q^2	$2q(1\text{-}q)$	$(1\text{-}q)^2$	1
Frequency after Selection	$q^2(1\text{-}s_1)$	$2q(1\text{-}q)$	$(1\text{-}q)^2(1\text{-}s_2)$	$1\text{-}s_1q^2\text{-}s_2(1\text{-}q)^2$

The rate of change, Δq, of the frequency of the *AA* carriers in one generation is:

$$\Delta q = \frac{q(1\text{-}q)[s_2(1\text{-}q)\text{-}s_1q]}{1\text{-}s_1q^2\text{-}s^2(1\text{-}q)^2}$$

If we make $\Delta q = 0$, and solve the equation for q, we get:

$$q = s_2/(s_1+s_2)$$

In other words, we can now see that even if the selection against the homozygotes for a particular gene is extremely severe, so long as the heterozygote is favoured, the gene will persist in the population. Thus, for example, suppose we have an instance like that of sickle-cell anemia, where all the homozygotes of one allele fail to reproduce, and suppose, for the sake of argument, the homozygotes of the other allele have but a 50% chance of survival and reproduction (compared to the heterozygote) [28]. Then we have

AA, 50% fit, with $1\text{-}s_1 = ½$ (*i.e.* $s_1 = ½$)
Aa, completely fit
aa, completely unfit, with $1\text{-}s_2 = 0$ (*i.e.* $s_2 = 1$)

Hence $q = \dfrac{1}{1/2} + 1 = 2/3$

From this it follows that, in any generation, 2/3 of the genes will be *A*, and 1/3 of the genes will be *a*. Hence, in each generation, 4/9 of the population will be *AA*, 4/9 of the population will be *Aa*, and 1/9 of the population will be *aa* and will die without reproducing. And the important thing to notice is that *this situation will continue indefinitely, unless conditions (e.g. relative selective advantages) change.* Thus we can see how, in a population, different morphological (or behavioural) forms can reach a 'balance', even though one or more of the forms might be at a selective disadvantage [29].

I think it is clear in examples of explanation like this, that is, explanations of instances of balanced polymorphism, a background of population genetics is presupposed. Let us therefore turn to a second kind of example of evolutionary explanation, one which is much more difficult (at least, a kind of explanation of a phenomenon where it would be much more difficult to spell out in full every part of the explanation).

b) *An evolutionary explanation in biogeography.* — A phenomenon which has long interested students of biogeography is that the organisms on islands off the shores of large land masses are often similar to the organisms on the mainland; but usually they are not absolutely identical. Moreover, the islands themselves often have two or three species of a particular genus of organism, and so again we have organisms which are similar to each other (and to the mainland organisms) but not identical. Furthermore, the organisms which have this kind of similarity are usually things like birds, reptiles, and insects. Islands rarely have organisms like mammals, and when they do, they are often very different from those on the mainland.

The explanation given by evolutionists of these kinds of phenomena, the most famous example being that of Darwin's Finches on the Galapagos Archipelago, is that the similarities are due to comparatively recent common ancestry, and that the island populations were founded by ancestors from the mainlands. It is argued that because of the rather different conditions of ocean islands from large land masses, the organisms on the islands evolve away from the mainland forms, and that this evolution brings with it reproductive barriers between the island and mainland forms. It is also argued that the passage between mainland and island is made infrequently (due to natural barriers like winds); but that if the passage occurs at intervals, the new arrivals will be reproductively isolated from the older inhabitants, and thus the new arrivals will evolve into a new species, similar to but different from those already there. Finally, it is argued that the kinds of organisms which will settle on islands obviously must be those with some chance of getting there. Birds, for example, have such a chance of being blown there by the wind. Large land animals on the other hand, have little or no chance of crossing comparatively large tracts of water [30].

Now obviously, in this explanation we find a number of propositions peculiar to the discipline (*i.e.* peculiar to biogeography) — for example, there is the proposition about the isolating effects of natural barriers like stretches of water, and there is also the proposition about the climatic effects of island life as opposed to mainland life. However, it seems to be equally the case that the explanation presupposes knowledge of the ETC, that is, of population genetics. Of course, evolutionists cannot hope to assign definite values to the rates of gene mutation and the intesity of selection that migrating populations will encounter. What they must assume is that these are roughly comparable to the kinds of values which we find today — in particular, they have to assume that mutation is a fairly rare phenomenon, and that the chances that a new gene will prove adaptively advantageous are slight (and that such chances grow even slighter the greater the overall morphological or behavioural effect one supposes the gene to have). But, with assumptions like these, knowing as they do that, thanks to the different new conditions encountered on islands, the selective pressures on migrating organisms is likely to be quite severe, evolutionists can, with the aid of population genetics, infer that the overall changes in gene-ratios from generation to generation are liable to be very great.

In fact, what evolutionists can infer is that in the early generations, the change of gene-ratio will probably be very drastic, but then, as things start to « settle down », the change will slow considerably, particularly if there is a steady mutation to any gene which is at a selective disadvantage. To see this, suppose for example that a small group of organisms migrate, and suppose that they carry a recessive gene with a frequency of q_o. This gene could well be maintained in their parent population, since a homozygote for the gene might be at a selective advantage (or possibly the gene might not be entirely recessive, and the heterozygote might have an advantage and thus one might have balanced polymorphism). Suppose however, that in the new conditions the gene is lethal for its homozygotes. Then, as we saw earlier, the number of generations (t) for the gene frequency to change from q_o to q_t is given by the formula

$$t = \frac{1}{q_t} - \frac{1}{q_o}$$

Hence, for example, suppose the initial frequency were 0.5. After 5 generations the frequency would be 0.167; after 10, 0.091; after 100, 0.010; and after 1000, 0.001. The gene frequency drops away drastically, although it is interesting to note that it never quite vanishes. Thus, with a result like this, we can now see how population genetics helps to fill out the evolutionist's explanation of the peculiar features of organisms which we find on ocean islands. Strong selective forces can have great effects on populations genetic compositions, even where the relevant genes are recessive.

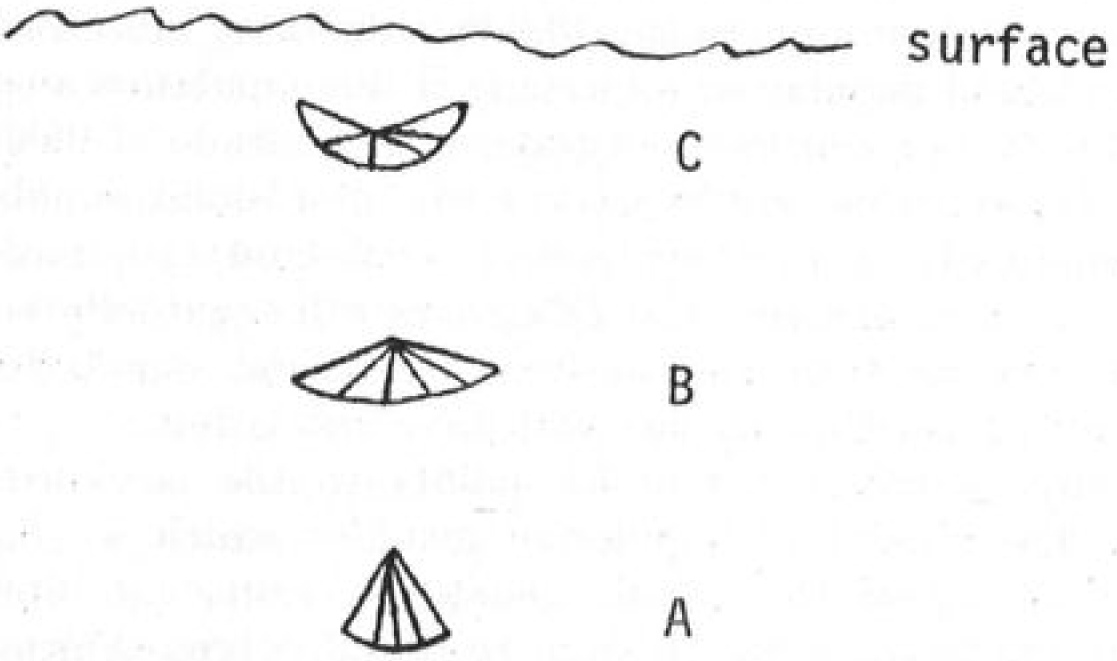
Actually, it is worth adding that population genetics can aid the evolutionist's explanation of the nature of island populations in at least two other ways from that which I have just mentioned. For a start, population genetics shows that even very small selective forces can make a great difference under the right conditions. It has been calculated that selection can be effective so long as $S > 1/N$, where N is the effective breeding number of the population being considered [31]. If a population grew to say 10,000, something more than possible even for an island population (especially if the population were insects), the selective force could have an order of magnitude of 0.0001 and be effective. Hence, what one expects is that the island populations will change genetically from their parent (mainland) populations, even under slight forces, and that the differences will eventually be sufficient to prevent genetic transmission from the island populations to the parent populations, that is, one will have speciation.

Secondly, population genetics points to the so-called 'founder' principle. The results of population genetics which we have so far considered all reveal the terrific genetic diversity an inter-breeding population will have, either through balanced polymorphism, or recessiveness of the genes, or so on. This holds, as we have seen, even when the selective pressure is very severe against certain genes. But this means that it is unlikely that there is such a thing as an 'average' member of a population — each member will probably have some genes possessed by very few other members. Hence, founders of island colonies will have (and pass on) genes possessed by very few of the mainland population members, and conversely, will not possess many of the mainland population genes or the genes possessed by later migrants to the islands. This, like slight selective forces, will also result in speciation, and with repeated migration, will lead to several closely related island species [32].

All in all, I do not think it can be denied that evolutionists' explanations of island populations are things which draw heavily on our knowledge of genetics, particularly population genetics. This being so, let us conclude our discussion of evolutionary explanations with one final example.

c) *An evolutionary explanation in paleontology.* — Unlike other evolutionists, the *paleontologist* has a dual task to perform, because for him organisms are not the 'given'. Rather, what he is faced with are fossils, and from these he has first to infer the actual existence, nature and course of life in the past, and then, having done this, he must explain why the life took the paths that it did [33]. As far as the first task is concerned, the paleontologist requires propositions which are special to paleontology, propositions from the ETC, propositions from other branches of evolutionary biology, and propositions borrowed from other sciences, particularly geology. To illustrate these needs, let us pretend that a paleontologist is faced with the following (simple) case. Suppose that digging in a particular place reveals fossils at three levels, and that the fossils are similar but not identical. Figuratively the situation is as follows:

Fig. 4.



First, the paleontologist must decide the ages, or at least the relative ages of the three fossils. Is A older than B and B older than C, or has there been a ground upheaval and is the true situation otherwise? To answer these questions he must rely on geology (and possibly the physical sciences, if he is using radio-active dating methods). Secondly, the paleontologist must decide that these objects A, B, and C, really are the remains of dead organisms, and not just curiously shaped pieces of rock. For this he will need peculiarly paleontological assumptions like « organism-like rocks are the remains of organisms » [34]. Thirdly, the paleontologist must assign functions to the major features of (what are now) his organisms. He must decide whether 'wings' and 'legs' and 'mouths' really are wings and legs and mouths. For this, he must rely on the conclusions of other branches of evolutionary biology, like morphology [35]. Also he will probably at this point require the help of sciences like geology. Does, for example, the rock formation in which the organisms were found indicate whether at the time of the lives of the organisms this particular place was land or sea? Finally, the paleontologist must fill in the gaps between A and B, and B and C. In particular, he must decide how rapid the change was from A's form to B's form, and from B's form to C's form. In order to do this he will undoubtedly have to rely on the ETC for estimations of the rates at which particular evolutionary change occur. (In particular, he will have to rely on conclusions like those which we discussed in the last example). Then, at last, having used all the different kinds of propositions which I listed, the paleontologist will have reconstructed a phylogeny, that is, he will have constructed a description of the evolutionary pathway taken by the organic forms whose remains are fossils A, B, and C.

This first stage of his work completed, the paleontologist will now be in a position to go on to attempt to explain the phylogenetic path of the organisms he is considering [36]. At this point, however, although the paleontologist has to work almost entirely by analogy, arguing from the characteristics of living organisms to the supposed characteristics of his dead organisms, his work and the explanations that he gives seem very similar to the work and explanations that one might give for the evolutionary development of contemporary organisms. In particular, such explanations will clearly entail references to the ETC.

As an example of this, let us consider an explanation offered by A. S. Romer in *Man and the Vertebrates*. Romer explains why the primitive amphibians during the Devonian period evolved into land-dwellers. It was not, Romer argues, because the amphibians wanted to breath air (they could already do this) or because they wanted to escape from enemies (they were among the largest in the water). Rather it was, paradoxically, to stay in the water.

> The Devonian, the period in which the amphibians originated, was a time of seasonal droughts. At times the streams would cease to flow... If the water dried up altogether and did not soon return...the amphibian, with his newly-developed land limbs, could crawl out of the shrunken pool, walk up or down the stream bed or overland and reach another pool where he might take up his acquatic existence again. Land limbs were developed to reach the water, not to leave it.
>
> Once this development of limbs had taken place, however, it is not hard to imagine how true land life eventually resulted. Instead of immediately taking to the water again, the amphibian might learn to linger about the drying pools and devour stranded fish. Insects were already present and would afford the beginnings of a diet for a land form. Later, plants were taken up as a source of food supply... Finally, through these various developments, a land fauna would have been established. (Romer, 1941) [37].

It is clear that in this passage, whatever the peculiarly paleontological assumptions may have been, although Romer is writing in a fairly informal manner, nearly all of his claims presuppose the truth of the basic premises of the ETC. For a start, Romer is obviously presupposing that when the droughts came, the amphibians did not evolve instantaneously into land organisms. It was, rather, a slow process. But why should Romer suppose this? It must be because he is assuming the truths of population genetics which show how heritable change can spread gradually through populations, even though the selective forces in favour of the changes may not be very great (or, may go against them for a while). Moreover, Romer assumes that this result held in the Devonian period, as well as at the present. Another thing that Romer takes for granted is that it makes sense to explain the lingering of the organisms on land in terms of the availability of food. He does not, for example, explain the lingering in terms of the amphibians' aesthetic enjoyment of land-life. Again, if we delve into the reasons behind Romer's confidence in the assumption which he does make, we find that he is relying on the truth of the ETC. The availability of food is obviously more pertinent to survival, and hence to reproductive efficiency, than is aesthetic enjoyment of land-life. But, as we have seen, it is only through understanding and knowing the ETC that we come to realize that reproductive efficiency, in particular, having a greater chance of having off-spring than a rival, can have significant effects on the genetic constitution of populations. Hence, here also, the explanation is linked to the 'core'. Finally in our examination of Romer's argument, it should be noted that it is because of his implicit appeal to reproductive efficiency, that Romer can dismiss the other possible causes of the amphibians' evolution — for example, he does not think that the availability of air on dry-land improved the survival (and hence reproductive) chances of the amphibians. All things considered, therefore, I would suggest that in the above passage, although it is an explanation from the paleontological branch of evolutionary theory, we find presupposed the truth of significant parts of the ETC. In other words, the explanation supports my conception of the structure of evolutionary theory [38].

One could go on indefinitely giving examples of explanations from different branches of evolutionary theory; but I hope that those which I have given will suffice to make my position plausible (if not overwhelmingly probable). In all three cases, I have tried to show that reference has to be made to a central core body of evolutionary thought — a body which is the same in all cases — and because of this I would suggest that evolutionary theory is more structured than Beckner allows. In particular, I would suggest that although the links from the uppermost hypotheses right down to the lowest level propositions are usually not rigidly deductive, one can discern enough of an outline to talk of evolutionary theory being like the theories of the physical sciences, namely in that they are all essentially hypothetico-deductive systems [39].

Before concluding this section, there is perhaps one final point which should be mentioned. It will be remembered that one of the reasons (in fact, the only concrete reason) that Beckner gives to support his conclusion that evolutionary theory is not hypothetico-deductive is that evolutionary theory supposedly does not allow us to derive certain widespread generalizations held by evolutionists. Unless this charge is briefly answered, the reader might think that although indeed much of evolutionary theory is as I have described it, other parts, which perhaps make a significant fraction of the theory, stand outside of the formal framework.

The two generalizations or 'laws' which Beckner mentions as standing outside of the formal part of the theory are Willinston's Principle and Bergmann's Principle. The first of these states that repetitive serial structures in animals evolve so as to become less numerous but more differentiated, and the second states that in colder regions the members of geographical races of warm blooded animals are larger than the races of the same species in warmer regions. Is Beckner right in claiming that the relation of these to evolutionary theory is different from the relation of, say, Kepler's Laws to Newtonian Mechanics? There are two points which show that Beckner's case is not very strong. In the first place, although at one point evolutionists delighted in framing 'laws' like those above and many others like Dollo's Law (« evolution cannot be reversed ») and Allen's rule (« the extremities of warm blooded organisms are smaller in colder regions than in warmer regions »), modern evolutionists pay relatively little attention to them. They have so many exceptions that they could hardly be more than rough rules of thumb. For example, B. Rensch found 20-30% of palearctic and nearctic birds were exceptions to Bergmann's Principle. For palearctic and neartic mammals the exceptions rose to 30-40%! (Rensch, 1960, 109) Most evolutionists would probably follow Simpson in refusing to call them 'laws', and in practically ignoring them in their evolutionary studies. (Simpson, 1963, 29).

The second point which can be made against Beckner's argument is that even though these 'laws' are riddled with exceptions, it is not quite true that evolutionary theory can throw no light on them. Whilst one cannot deduce the 'laws' from the theory (which is a good thing in view of the exceptions), thanks to evolutionary theory one can understand why the laws hold approximately, and also one can understand why they have so many exceptions. That a trend not unlike that described by Bergmann's Principle should be found to hold in the world is not surprising (given evolutionary theory) in light of the fact that selection in colder regions is likely to be somewhat different from selection in warmer regions. In particular, in warm blooded animals there will be greater selection in colder regions for methods of heat-conservation. An increase in size, although it increases the absolute surface area of the organism, *reduces* the body volume—surface area ratio, since volume is a cube function of length and surface area is only a square function. Hence, the larger animal, pound for pound, has a heat conservation advantage over the smaller animal. On the other hand, evolutionary theory also leads one to expect, and can explain why, there might be exceptions to this principle. Increasing the size is not the only way of improving heat-conservation characters — one could develop a more efficient outer-covering — and there could well be other selective forces at work, pressuring animals in colder regions in the direction of smaller size. For example, one species of pocket gopher is smaller in size in colder regions, because its colder regions are high altitude regions where suitable burrowing soil is much shallower than at warmer, low-altitude regions. (Mayr, 1942, 90).

In the light of this discussion, it seems fair to say that whilst something like Bergmann's Principle can certainly not be rigorously deduced from evolutionary theory, in an informal way the theory does throw light on the rule [40]. Moreover, the same seems to be true of other so-called 'evolutionary laws'. Consider the most famous of them all, Dollo's Law. This is perhaps the most reliable of all the laws, but it too has exceptions of a kind — back mutations occur, and lost combinations of genes can be found. Organisms can even reverse large scale evolutionary phenomena in a sense — organisms in some lines got smaller and then larger, nearly all the animals without sex had distant ancestors without sex and more recent ancestors with sex, and, as is well known, the ancestors of the whale left the sea and then returned. On the other hand, even though exceptions like these are enough to make Simpson refuse to grant that it is a real law, clearly the 'law' does have some plausibility — whales did not go back to becoming fish, for example. Moreover, Simpson himself justifies the law by an appeal to the kind of genetics to be found in the ETC. He writes:

> The statistical probability of a complete reversal or of essential reversal to a very remote condition is extremely small. Functional, adapted organisms noticeably different in structure have different genetic systems. They differ in tens, hundreds, or thousands of genes, and such genes as are the same have different modifiers and are fitted into differently integrated genetic backgrounds. A single structure is likely to be affected by many different genetic factors, and practically certain to be if its development is at all complex. The chances that the whole system will revert to that of a distinctly different ancestor, or even that this will happen for any one structure of moderate complexity, are infinitesimally small. (Simpson, 1953, 311).

Finally, it is worth noting that Simpson concludes this passage by appealing to a technical paper by the geneticist H. J. Muller for support [41].

More examples of this kind would be redundant. By now it is obvious that the 'laws' of evolution do not give Beckner much help. They are not that important, nor are they that isolated from the main body of theory [42]. Hence, I would suggest that no concrete reasons have yet been advanced to deny my characterization of evolutionary theory as something with a 'loose' hypothetico-deductive structure.

4. Three Arguments Against the Possibility of Evolutionary Theory ever being Axiomatized. — So far in this paper, although I have tried to counter the arguments of Smart and Beckner,

my major purpose has been a positive one. I have been concerned to outline what I take to be the true nature of evolutionary theory, rather than just to show other people's ideas wrong. Before concluding, however, it is important to defend my conception of the theory against a number of arguments which are commonly made — arguments which purport to refute formal interpretations of evolutionary theory. In the interests of brevity, I shall select but three such arguments, all of which are given by T. A. Goudge in his book, *The Ascent of Life.*

a) *Are evolutionary phenomena too complex for an axiomatization of the theory?* — The first argument Goudge gives is based on the complexity of evolutionary phenomena. He writes:

> The concepts which occur in a formalized axiomatic system have an absolutely fixed and precise meaning. In this respect they differ from concepts which refer to empirical facts. The latter concepts have an unavoidable imprecision, due to what has been called their 'open texture'. The more complex the facts to which they refer, the greater will the imprecision be. Now the concepts which belong to evolutionary theory refer to facts whose complexity is enormous. Hence to treat these concepts as though they were absolutely precise by incorporating them in a formal system, would be to distort them beyond recognition. (Goudge, 1961, 16).

Unfortunately, as it stands at the moment, this criticism is so broad that it not only excludes biological concepts from the realm of possible axiomatization, but it excludes the concepts of physics and chemistry as well. Physico-chemical concepts refer to 'empirical' facts, like planets and pendulums and acids; but if what Goudge writes were true, then these concepts could never be incorporated within axiomatic systems. Since they obviously can be, his criticism must be modified somewhat. Presumably, what Goudge really means is something to the effect that the concepts in an axiomatic system, whether they refer to empirical things or not, must have a fairly limited meaning, but that due to the complexity of evolutionary phenomena, evolutionary concepts could never have such a limited meaning, and therefore evolutionary concepts could never be incorporated within an axiomatic system. However, it is easy to see that even this modified criticism is gravely in error. Even if we grant the rather dubious truth of the premise that the concepts of an axiomatic system must have a fairly limited meaning, [43] Goudge's criticism still flounders on the fact that we encounter physical phenomena of enormous complexity — at least as complex as anything biological — and yet concepts which refer to these phenomena can be incorporated within the axiomatic systems of physics. Consider, for example, planets. These are highly complex phenomena — indeed presumably a full description of what we meant by the planet 'Earth' would involve a full description of all the life on it — but, despite this complexity, laws about the planets can be deduced from that axiomatic system known as 'Newtonian Mechanics'. Goudge must therefore be mistaken when, *a priori*, he rules out the possibility of axiomatic evolutionary systems because of the complexity of evolutionary phenomena. Physical phenomena are just as complex; but physics has axiomatic systems.

The assumptions which lead Goudge into his confusion are fairly often mistakenly made about science. It is assumed that science must be absolutely 'objective' and that therefore, before the scientist can start explaining and theory building, he must have 'all' the facts in front of him — a complete description of every facet of every item pertaining to his particular area of inquiry. Moreover, it is also assumed that the scientist must impartially incorporate every one of these facts into his finished result. Clearly these assumptions are quite false — there can be no such 'objectivity' of this kind in science. As Goudge notes, when dealing with empirical phenomena it is doubtful whether one could ever have such a 'complete' description. What Goudge fails to note is that this lack of a complete objective description does not bar even the most formal type of scientific activity. The physicist, the chemist, and the evolutionist, select for examination just those aspects of experience which interest them. None of them aim to be entirely comprehensive in their studies, neither the physicist when he talks only of certain features of planets nor the evolutionist when he talks only of certain features of organisms. But it is through this kind of selectivity and deviation from the impossible ideal of absolute objectivity, that the physicist is able to create his axiomatic systems. To deny the possibility of such selectivity to the evolutionist, and thus to rule out *a priori* an axiomatic evolutionary system, seems as unnecessary as it is unfair [44].

b) *Is the axiomatization of evolutionary theory premature?* — The second reason that Goudge gives in support of his claim that evolutionary theory could never be axiomatized is based on the newness of the theory. He writes as follows:

> The framework of statements constituting evolutionary theory is at present lacking in the tidiness and completeness which should exist before any axiomatization is undertaken. Since the theory is expounded discursively by biologists, it is often formulated in different ways with differences of emphasis on details. Modifications are being periodically introduced as fresh evidence comes to light and new theoretical interpretations are proposed. Any attempt to 'freeze' such a growing body of ideas in a formal scheme would result in grave misrepresentations. (Goudge, 1961, 16).

This criticism has a somewhat *ad hoc* air to it. It immediately makes one want to ask when exactly a theory does achieve the 'tidiness and completeness' requisite for an axiomatization? But as soon as this question is asked, we see that Goudge's position is founded on a rather naive view of theory-construction — a view which is not that dissimilar to the assumption which made Goudge argue as he did in his claim that the concepts of evolutionary theory could never be put in an axiomatic system. Goudge seems to imply that first of all a scientist gathers together a colossal body of information about his subject — concepts, laws, explanations, and such like — and only when he has done this, does he then sit down and try to build an axiomatic system which incorporates all of this information. The real story is, of course, quite different. Theory building and the discovery of new ideas, often go hand in hand. This truth applies even to axiomatic theory building, for in trying to axiomatize a limited amount of information, one often discovers ways of improving and extending one's information. Of course, there is always a danger that one might become too committed to one axiomatic system too early; but such a danger does not seem to be overwhelmingly great in the case of evolutionary theory. Indeed, as we have seen, evolutionists were prepared to revise their axiomatic system. The system we find today is very different from that which Darwin tried to build. Hence, I would suggest that in the absence of any reasons from Goudge which show just why evolutionary theory is presently so immature that an axiomatization would be dangerous, we can discount his second argument. Moreover, even if he could give such a reason, it would only rule out an axiomatization for the present — it would not show such an axiomatization to be logically impossible [45].

Before going on to consider the third argument which Goudge advances in support of his claim that evolutionary theory can never be axiomatized, it is perhaps interesting to note that both of Goudge's first two arguments are very similar to arguments which have recently been advanced against evolutionary taxonomy. Certain contemporary taxonomists have argued that any biological classificatory system which is based in some way on evolutionary principles is bound to be irreparably 'subjective'. They have also argued that evolutionary taxonomists get caught in vicious circles as they formalize their classifications and then adjust them when new taxonomic evidence is revealed [46]. Whilst I have much sympahty for the position of these critical taxonomists — evolutionists have certainly had a tendency to let their classificatory theories far outrun their evidence — blanket condemnations of this sort seem to fail for much the same reasons as do Goudge's arguments. For a start, the subjectivity of evolutionary sciences, in particular evolutionary classifiations, is no different from the subjectivity that one finds in other sciences. Indeed, even the alternatives offered to evolutionary classification — alternatives which are based on the morphological characteristics of organisms — rely on subjective decisions by the classifier. Decisions, for example, as to what mathematical techniques to use to cluster organisms together into groups. Secondly, as David Hull has argued elsewhere, to accuse evolutionary classifiers of circularity because they employ a sort of self-regulating process between their theory and the evidence, is to misunderstand both the logic and methodology of their practices. (Hull, 1967) Evolutionists' conclusions are sometimes unwarranted, but no one would want to claim that they are infallible. All that is claimed is the truth that, in continually àdjusting their theories to fit the evidence, they are following a legitimate path trodden by other scientists.

c) *Can a 'historical' science be axiomatized?* — We come now to Goudge's third and final argument. It is one which is overwhelmingly popular amongst those who would argue that evolutionary biology is different, and it revolves around the supposedly 'historical' nature of the theory of evolution [47]. Goudge argues as follows.

> The theory of evolution contains as an essential part of its structure a number of historical statements. These deal with such phenomena as phylogenetic trends, adaptive radiation, the extinction of past forms of life, etc. But no evolutionist would be likely to agree that the statements of natural history can be deduced as logical consequences of a set of postulates. Consequently, the presence of historical statements makes it impossible to construct a full axiomatization of the theory. (Goudge, 1961, 16).

As when dealing with Goudge's other arguments, it is important first to separate the obviously wrong from the more subtly wrong. Clearly, one *could* deduce statements of natural history from a set of postulates. Indeed, if one were so inclined, one could deduce anything at all from a set of postulates, just as long as one used the right postulates. Presumably, what Goudge really means is that given a set of (non-contradictory) postulates which do not make references to particular times, and if one excludes trivial counter-instances (such as $p, \therefore p \vee r$, where p is non-historical and r is historical), this claim seems to be well taken.

However, even though this weakened claim I am ascribing to Goudge is true, and even though an axiomatized evolutionary theory would not contain references to particular times in its premises, the existence of evolutionary statements which do refer to particular times does not rule out the possbiility of an axiomatized version of evolutionary theory. The reason for this is that if in fact the grounds given are sufficient to rule out the possibility of an axiomatized evolutionary theory, then we must also rule out the possibility of axiomatized physical theories, for in physics as well as in evolutionary biology, we find statements which are tied to particular times. This conclusion is obviously ridiculous, and as soon as we consider the relationship between physical theories and those statements about physical phenomena which refer to particular times, we can at once see how thin Goudge's arguments really are. What we find for example, is that statements such as « The ball hit the ground at time *t* », follow from the basic premises of Newtonian Mechanics *together with* certain other statements. It is these latter statements, rather than Newtonian Mechanics, which tell us about the ball's behaviour at other times (earlier or later). In other words, theories (of physics and chemistry) act a bit like sausage machines — you cannot get sausages out unless you put meat in, and similarly, you cannot get temporal statements (*i.e.* statements which refer to particular times) out unless you put temporal statements in. But, and this is the point that Goudge seems to have overlooked, you can put temporal statements in. Thus, from a physical theory (one which is axiomatized) together with these statements, one can deduce further temporal statements.

There seems to be no reason why the same should not hold for an evolutionary theory, even one which is clearly axiomatized. Indeed, I would suggest that just such a relationship does in fact hold between the contemporary theory of evolution and the statements surrounding it which refer to particular times. To see this, let us think back for a moment to Romer's explanation of the evolution of land vertebrates. Romer does not infer the evolution of land vertebrates just on the basis of the premises of evolutionary theory. Rather, he uses assumptions about mutation, selection and so on, and he conjoins these with further statements about events at particular times. For example, there is the statement telling one that the Devonian period was a time of drought. There is the statement that the water dried up to form small pools. There is the statement that the amphibians were larger than most other organisms in the pools. Now if Goudge is right, all of these statements must be part and parcel of evolutionary theory, and this is an assumption I find quite implausible. For example, the statement that the Devonian period was a time of drought is not an evolutionary statement at all! It is a statement about meteorology. Similarly, the statement that the water dried to form small pools is not evolutionary. It belongs, I suppose, to geology or geography. Even the statement that the amphibians were larger than most other organisms in the pools, does not strike me as having much to do with evolution *per se*. Like the other two statements, it may have evolutionary consequences; but it does not seem part of the theory, any more than say, the statement that one pendulum is larger than another, seems part of Newtonian Mechanics. We may use Newtonian Mechanics to calculate the different periods of the pendulums; but this does not make statements about the pendulums, part of the theory. Similarly, unless one legislates *a priori* that all statements about organisms are *ipso facto* evolutionary, the statement about the size of the amphibians (during the Devonian period) seems non-evolutionary. But if this is so, and such statements are indeed non-evolutionary, then even given the present rather loose form of evolutionary theory, the correct way to analyse explanations which use the theory and which are about events at actual specified times, is to argue that the propositions describing these events follow from the non-historical theory together with other propositions which specify particular times.

What leads Goudge to argue as he does? He has made a fairly common mistake, namely he has confused the non-historical *theory of evolution*, with historical *phylogenetic descriptions* about actual evolutionary events. Evolutionary theory in itself is no more historical than any other theory — indeed, in this sense the popular classification of theories into 'historical' and 'non-historical' is highly misleading (and could well be dropped). Evolutionary theory just seems historical because it is so commonly used to explain momentous events in the distant past, whereas physical theories usually (but by no means always) are used to explain events in the present or very near past. What is historical in evolutionary studies are these distant events, and the overall phylogenetic paths that evolution took. Admittedly, the construction of these paths is, as we saw in the last section, a major task of the evolutionist, and is, moreover, a task which calls for scientific ability of the highest order. However, the theory that the evolutionist uses to explain these phylogenetic paths is not the same as the descriptions of the paths. The two things are quite distinct, and for this reason, there is nothing peculiarly historical about evolutionary theory [48]. Indeed, if the major points of this paper are well taken, there is nothing very peculiar in any other way about evolutionary theory. Rather, it is a theory which is not so very different from the theories of physics and chemistry.

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Thanks are due to David Hull, Hugh Lehman, John Leslie, and Alex Michalos who criticized an earlier version of this paper.

NOTES

[19] For example, in Hempel (1965).

[20] Detailed references to the writings of these biologists can be found in the bibliography of Dobzhansky (1951).

[21] Particularly worthy of note are Dobzhansky (1937, 3rd edition 1951), Huxley (1942), Mayr (1942) Simpson (1944), (this was rewritten as Simpson, 1953) and, rather later, Stebbins (1950).

[22] For example, an introduction to evolutionary theory, Smith (1958), refers the reader to all of the works listed in the last footnote, recommending them for further reading.

[23] In support of what I am suggesting, it is worth noting that several of the evolutionists that I am considering do explicitly admit that what they are trying to do is show how the solution to evolutionary problems presupposes a knowledge of population genetics. For example, Simpson says that the purpose of his *Tempo and Mode in Evolution* was

« to try to apply population genetics to interpretation of the fossil record and conversely to check the broader validity of genetical theory and to extend its field by means of the fossil record ». (Simpson, 1953, ix) Dobzhansky states his agreement with this position even more categorically. He writes that, « Evolution is a change in the genetic composition of populations. The study of mechanisms of evolution falls within the province of population genetics ». (Dobzhansky, 1951, 16).

[24] My usage of the terms 'taxonomy' and 'systematics' follows Simpson (1961).

[25] By 'ideally' in this context, I mean 'from the point of view of easy classification'. I would not deny that people like Simpson and Mayr enjoy the challenge presented by the difficulties of organic classification.

[26] I talk about the problems that these groups raise, and other problems of organic classification, in Ruse (1969b).

[27] As we saw in the last section, this is an example of what is called 'overdominance'.

[28] I am not sure what the exact figure would be in the particular case of polymorphism for the sickle-cell gene. Probably selection would be rather less severe on the normal gene homozygote; but the assumption of a 50% chance of survival will make the necessary point and keep the arithmetic simple.

[29] The full story of sickle-cell polymorphism is much fuller than the version I have given here, and it involves, amongst other things, man's *cultural* dimension. I discuss sickle-cell polymorphism as an example of the interaction between man *qua* biological being and man *qua* cultural being in Ruse (1971c).

[30] See Lack (1947) for a fascinating account of the evolution of Darwin's Finches.

[31] The effective breeding population is a measure of the reproductive power of a population. It is equivalent to the number of breeding members a population would have, if the number of males and females were evenly divided and reproduction were random within the group. Some evolutionists believe a force of $S > 1/4N$ to be effective. (See Simpson, 1953, 118).

[32] Mayr discusses the founder principle in detail in Mayr (1942) and Mayr (1963).

[33] Of course, often other evolutionists want to make inferences about organisms they are not in any sense observing. The point is that, by definition, the paleontologist cannot avoid such inferences.

[34] This is another truism which might strike the reader as unnecessary. However, it is not contradictory to assume that fossils are curiously shaped pieces of stone put in the ground by God to test our faith in Genesis. On the other hand, this supposition is, I hope, false.

[35] A most interesting discussion of the way in which paleontologists infer the function of characters from the structure of fossils is in Rudwick (1964). But see Ruse (1969a) for a disagreement with Rudwick's conclusions.

[36] Actually, although the construction of a phylogeny seems to be *logically* prior to an explanation of it, in practice, I am sure a phylogeny and its explanation are usually found together.

[37] This passage is quoted by Goudge (1961), p. 71.

[38] I consider this explanation by Romer in rather more detail in Ruse (1971d). There, my intention is related to my present intention; but it is not quite the same. My concern there is to show that Romer's explanation is better considered as a sketch of a covering-law explanation, than as what T. A. Goudge takes to be some kind of explanation peculiar to evolutionary theory — a kind of explanation he calls 'narrative' explanation. Obviously, the claim that evolutionary theory is hypothetico-deductive includes the claim that evolutionary explanations fit the covering-law model. Possibly, the converse does not hold.

[39] I assume, without further argument, that if my point about the relationship of population genetics to the rest of evolutionary theory is well taken, then insofar as population genetics shares with physical theories the other features which I discussed in section one, the whole of evolutionary theory shares these features.

[40] Scholander (1955) argues that the explanation of Bergmann's Principle in terms of heat conservation is inadequate, for he thinks that there are far more efficient ways of conserving heat. In a reply, Mayr (1956) points out that nature often uses many different aids to meet her goal.

[41] Muller (1939).

[42] A thoughtfull discussion of the 'laws' of evolution can be found in Mayr (1963).

[43] Braithwaite (1953) points out that the theoretical terms of scientific theories usually have at least part of their meaning left open for future development.

[44] For a good discussion of the impossibility of doing science without making value judgements, see McLaughlin (1970). I discuss the question of objectivity in biology in Ruse (1970b) and Ruse (1971e).

[45] On top of this, Goudge would have to show either why population genetics lies outside of evolutionary theory, or why population genetics is not really axiomatized.

[46] See for example Sokal and Sneath (1963) and Sokal and Camin (1965).

[47] This argument is a particular favourite of G. G. Simpson, although I do not know whether Simpson would agree with Goudge in arguing that the historical nature of evolutionary theory precludes its axiomatization. See, for example, Simpson (1963).

[48] The distinction between evolutionary theory and phylogenies is well made by Bock and Wahlert (1963).

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CRONACHE - REPORTS

Un Libro Bianco sulla tutela dell'ambiente naturale

Il Consiglio Nazionale delle Ricerche italiano ha pubblicato, con un ritardo di due anni dovuto a varie cause, il suo Libro Bianco sulla conservazione della natura. Merita, intanto, di essere posto in rilievo il fatto che, per la prima volta in Italia, i problemi della difesa dell'ambiente naturale vengano affrontati con il rigore e la consapevolezza necessari, invece che con gli incerti strumenti della generica buona volontà e dei nobili propositi che hanno presieduto a tutte le iniziative fin qui assunte.

Il Libro Bianco contiene una precisa analisi della allarmante situazione creatasi in Italia, ed alla quale di ben scarso sollievo è stata l'azione velleitaria e frammentaria svolta così dai poteri pubblici come da gruppi ed associazioni. Alla diagnosi, formulata senza reticenze, così da assumere spesso il carattere di una denuncia, segue una serie di proposte e di suggerimenti.

Il prof. Giuseppe Montalenti, presidente della commissione del CNR che ha condotto il lungo studio, nel presentare il documento ha fra l'altro indicato i presupposti di un serio esame dei problemi connessi alla conservazione e alla tutela della natura:

« *Concezione globale dell'ambiente.* La scienza ecologica dimostra che non si possono artificiosamente tenere separati i vari fattori ambientali, come è necessario fare per il loro studio analitico. In natura gli elementi di un ecosistema sono collegati da legami di interdipendenza che talvolta sono intricati e difficili a scoprire. L'ambiente deve quindi sempre, per quanto possibile essere considerato nella sua totalità, e gli interventi - che per necessità di cose devono essere, come si suol dire, settoriali - dovrebbero risultare il più possibile coordinati in una visione d'insieme. Ogni ecologo può portare numerosissimi esempi di effetti secondari o terziari, determinati dall'alterazione di un termine dell'equilibrio, effetti che talvolta appaiono assai lontani dall'intervento primario e difficilmente prevedibili ».

« *Concezione giuridica dell'ambiente.* Non sono un giurista e perciò posso esprimere, a questo riguardo idee sbagliate. Ma non mi astengo dal farlo, proprio per richiamare l'attenzione dei giuristi su questo punto che mi pare assai importante. Se mi dimostreranno che la legislazione opportuna esiste: tanto meglio, si tratterà di applicarla. Mi pare che non esista nel diritto vigente in Italia (non so se esista in altri paesi) una definizione giuridica dell'ambiente come *patrimonio della* *comunità*, e di conseguenza non esistono norme che ne assicurino la tutela nell'interesse di tutti. Sostanzialmente prevale il concetto di proprietà privata: quello che non è proprietà privata è *res nullius*, come la fauna, o la flora spontanea. Vi sono sì, se non sono male informato, delle *res communes omnium*, ma la giurisdizione è sempre improntata sul concetto di proprietà privata, cioè dei vantaggi o dei danni che un proprietario può conseguire, da un'alterazione che un altro produce per esempio nell'acqua di un fiume, o nell'aria. Manca, se non vado errato, una legislazione che regoli i rapporti diretti fra uomo e ambiente, cioè con queste cose comuni, che sono patrimonio di tutti, e che, come *bene comune* devono essere difese.

Ritengo che sia giunto il tempo che i giuristi affrontino questo problema e propongano una legislazione adeguata ».

« *Concezione dei problemi ambientali a livello mondiale.* È ovvio che la tutela dell'ambiente e, in generale, dei beni comuni, è un problema che ha un aspetto locale, nazionale, ma da questo esorbita e va posto su di un piano internazionale, mondiale. Non ha senso considerare il problema degli uccelli migratori come un problema italiano, o jugoslavo, o scandinavo; non ha senso occuparsi dell'inquinamento del Mediterraneo se non su scala internazionale; così come non ha senso oggi considerare il problema della laguna di Venezia come un fatto che riguarda esclusivamente l'Italia e gli italiani ».

Il prof. Montalenti ha quindi esaminato i vari tipi di azione ai quali in Italia si affida la tutela dell'ambiente naturale. Ed ecco i punti fondamentali nei quali si articola questo sconfortante bilancio:

« *Ricerca.* Può essere rivolta, sia ad accertare le condizioni di biotopi o di equilibri biologici minacciati, o in procinto di scomparire, oppure ad argomenti particolari, come il grado di contaminazione di determinati ambienti, la tossicità di alcuni prodotti, i mezzi migliori per la difesa dalle azioni perturbanti, ecc.

« È un'attività di grande importanza; alla quale si dedicano in Italia, numerosi istituti e laboratori, nei settori di propria competenza. Molti Istituti Universitari, alcuni Istituti o Laboratori o Centri del CNR, i laboratori di vari Ministeri, nonchè diverse istituzioni non statali, hanno in corso programmi di ricerche attinenti più o meno strettamente a problemi della conservazione.

« La Commissione per la Conservazione della Natura, che non ha disponibilità finanziaria propria, ha organizzato (ottenendone il finanziamento dal Comitato per la Biologia e la Medicina) un programma di ricerche sulla fauna e la flora delle piccole isole. Queste come è noto rappresentano ambienti di particolare interesse, in quanto l'isolamento insulare, e le diverse caratteristiche geologiche, hanno permesso il differenziamento di popolazioni locali e l'insorgere di endemismi. In quasi tutte le nostre isole (Tremiti, Eolie, Egadi, Ponziane, Toscane, delle coste Sarde) salvo poche ma importanti eccezioni, la conservazione dell'ambiente naturale è gravemente minacciata, o già irrimediabilmente compromessa dalla destinazione a scopi turistici e dalla speculazione edilizia.

« Se si dà uno sguardo complessivo alle attività di ricerca nel campo della conservazione e della protezione della natura in Italia si rileva soprattutto la totale mancanza di *coordinamento*. Non vi è un organo centrale che indichi i problemi più importanti o più urgenti, e pensi a indirizzare e coordinare le ricerche. Sicchè queste sono per lo più affidate alla iniziativa e alla buona volontà di singole persone od organi di ricerca, che hanno necessariamente una visione parziale e perciò limitata dei problemi ».

« *Informazione e educazione dell'opinione pubblica.* Questa è un'attività assai importante, in un certo senso la più importante, perchè costituisce la base indispensabile per il successo di ogni azione concreta a livello legislativo ed esecutivo. È necessario che si formi una coscienza naturalistica, o ecologica che dir si voglia, affinchè la società sia in grado di esprimere, ai vari livelli, persone responsabili e conscie dell'importanza dei problemi e informate sui mezzi più adeguati per risolverli.

« In questo campo si può dire che si sono fatti grandi progressi, i quali sono tuttora in cammino. I vari mezzi di informazione, dalla scuola, alla stampa, al cinema, alla radio-televisione, si sono aperti ai problemi ecologici, tanto che, come ho detto all'inizio si può parlare oggi addirittura di « esplosione ecologica ».

Dopo aver ricordato l'azione svolta da diversi organismi, il prof. Montalenti ha così proseguito:

« È augurabile che scuola, mezzi di diffusione della cultura a livello popolare, giornali, cinema, radio-televisione, musei, giardini zoologici, orti botanici contribuiscano finalmente a destare l'amore e il rispetto per la natura, che è così scarso nel nostro paese, e a dare il senso dell'importanza della disciplina scientifica nella cultura moderna, importanza che in Italia è generalmente misconosciuta, perchè sopraffatta dall'imperio delle discipline letterarie, storiche e filosofiche nei nostri programmi scolastici ».

« *Segnalazione dei luoghi da proteggere.* Questo è uno dei campi in cui l'informazione più lascia a desiderare ed è più affetta dal cronico male italiano, la frammentarietà, l'occasionalità, la mancanza di coordinamento.

« La Commissione del CNR ha preso alcune iniziative a riguardo. Ha pubblicato (1968) un elenco delle zone, o fitocenosi di carattere mediterraneo, per le quali si propone una protezione integrale nell'ambito di Parchi Nazionali, o di Riserve Forestali, o di altro tipo, e che, quando già non siano demaniali, si propongono per l'acquisto da parte del demanio. Tale lavoro è stato compiuto dal prof. Roberto Corti dell'Istituto Botanico Agrario e Forestale di Firenze.

« A cura di chi vi parla è stata pubblicata (1967, Quaderno N. 38 della « Ricerca Scientifica ») una Relazione sulla protezione delle lagune e degli stagni costieri della penisola e delle grandi isole, con elenco e descrizione dei biotopi, a cominciare dalla laguna di Venezia. Oggi, che la conservazione delle zone umide, dopo gli eccessi della bonifica integrale è divenuto un problema di interesse mondiale, questo elenco dovrebbe essere tenuto nel massimo conto dagli organi esecutivi. Ma l'opera più cospicua che la Commissione ha fatto, in collaborazione col Ministero dei Lavori Pubblici, Direzione Generale dell'Urbanistica Servizio Studi e Programmazione, è il « Programma di ricerca territoriale sulle aree naturali da proteggere ». Esso consta di una « Carta dei Biotopi d'Italia » e di una serie di « Carte regionali dei biotopi » di cui fin ora è stata pubblicata solo quella del Lazio ».

Non meno scoraggiante il quadro che il prof. Montalenti ha tracciato dello stato della legislazione italiana in materia di difesa della natura:

« La situazione è caotica: anche qui gli interventi per lo più non sono coordinati, spesso sono episodici e frammentari.

« La ridda delle competenze, sia per la preparazione di interventi legislativi, sia con poteri esecutivi, è paurosa. Non meno di 14 Ministeri hanno competenze varie su problemi della tutela dell'ambiente, sono: in ordine alfabetico: Agricoltura e Foreste, Difesa, Esteri, Finanze, Grazia e Giustizia, Industria e Commercio, Interno, Lavori Pubblici, Lavoro e Previdenza Sociale, Marina mercantile, Pubblica Istruzione, Sanità, Trasporti, Turismo e Spettacolo. Ad essi possono aggiungersi la Presidenza del Consiglio e il Ministro della Ricerca Scientifica.

« Parecchie leggi elaborate da alcuni di questi Ministeri ed emanate dal Parlamento, sono buone, come pure molte disposizioni emanate direttamente dai Ministeri. Altre sono imperfette o del tutto carenti. In particolare la vecchia legge 29 giugno 1939 N. 1497 sulla protezione delle bellezze naturali e panoramiche tuttora in vigore è insufficiente soprattutto perchè in essa prevale la preoccupazione di tutelare gli elementi di valore estetico mentre quelli naturalistici sono trascurati. Anche in questo campo occorrerebbe: coordinamento. Un primo passo, a mia opinione potrebbe essere rappresentato da una raccolta e collezione di tutte le leggi, regolamenti e disposizioni singole in un testo unico.

« La Commissione del CNR, fin dal 1964-65 prese l'iniziativa di studiare questioni legislative e un Comitato ristretto, presieduto dal prof. M. Salmi, preparò uno schema di legge in cui la difesa delle bellezze naturali e panoramiche è impostata mettendo sullo stesso piano gli elementi naturalistici e quelli estetici. Ma il progetto è rimasto lettera morta. A questo riguardo, la Commissione ha proposto che la protezione delle bellezze naturali e panoramiche sia affidata, anzichè alla Sovrintendenza ai Monumenti, a « Sovraintendenza al paesaggio », di cui si propone la istituzione, e alle quali dovrebbe essere preposto un personale specificamente preparato in materie naturalistiche. Anche questa proposta è rimasta inoperante.

« Nel 1970 la Commissione ha elaborato una bozza di legge per la difesa della fauna, che dovrebbe sostituire la vigente legge per la caccia. Essa è pubblicata come Appendice IV al Libro Bianco. Successivamente un gruppo di Senatori ne prendeva spunto per presentare al Senato il disegno di legge N. 1780. Ci auguriamo che sia ripreso nella nuova legislatura.

« La Commissione si è anche occupata a lungo del complesso problema dei Parchi Nazionali in Italia, argomento quanto mai controverso, come tutti sanno, e del quale è meglio il tacer che il dire, tanta è la vergogna di certe situazioni ».

La stessa situazione caotica si riscontra nel settore degli strumenti esecutivi e degli organi di controllo.

« Alcuni Ministeri - rileva il prof. Montalenti - in particolare Agricoltura e Foreste, Pubblica Istruzione, Lavori Pubblici, Finanze (Demanio) hanno la potestà di imporre vincoli di istituire parchi e riserve, di approvare o respingere piani regolatori, in una parola di difendere i beni naturali e artistici. Molte di queste amministrazioni sono benemerite per l'opera di salvaguardia che sono riuscite a compiere. Ma per lo più operano in mezzo a grandi difficoltà, e spesso non sono in grado di resistere alle forti pressioni determinate da interessi economici privati, che si traducono in interventi politici.

« Alcuni dei poteri degli organi centrali sono poi delegati alle amministrazioni delle Regioni, sia quelle a Statuto Speciale, sia quelle a Statuto ordinario. A quanto mi risulta, in molti casi si è ancora allo stato *de iure condendo.* Occorrerebbe, comunque, che le esigenze della salvaguardia dell'ambiente naturale fossero opportunamente tutelate nel trapasso dei poteri.

« Per gli interventi spiccioli, le competenze si distribuiscono fra le autorità più varie: Prefetto, Amministrazione Provinciale, Ispettorato Provinciale del Lavoro, Medico Provinciale, Vigili del Fuoco, Consiglio Comunale, Sindaco, e via dicendo. Anche qui sarebbe auspicabile maggiore coordinazione e soprattutto più severa applicazione delle leggi vigenti. Le quali, come hanno dimostrato recenti interventi della Magistratura, spesso esistono.

« Occorrerebbe - ha aggiunto il prof. Montalenti - un organo centrale che avesse il compito di coordinare le molteplici attività delle varie amministrazioni, e che attuasse, per quanto possibile, in un paese come il nostro, in cui prevale l'individualismo, una politica unitaria dell'ambiente. Probabilmente è da evitare la istituzione di un apposito Ministero, cioè di un pesante organismo burocratico, che entrerebbe in competizione con gli altri già esistenti. Allo stato attuale sarebbe preferibile che un organismo come il Comitato Interministeriale per la Programmazione Economica (CIPE) assumesse il compito di programmare e dirigere la politica dell'ambiente ».

Il prof. Montalenti ha poi detto:

« La tutela dell'ambiente è uno dei problemi più importanti e più urgenti nel momento attuale, in Italia e in tutto il mondo. È superfluo accentuare qui ancora una volta questo fatto, di cui l'opinione pubblica è ormai bene informata da numerose e autorevoli fonti. In molti, forse troppi, convegni, colloqui, tavole rotonde, conferenze gli aspetti generali dell'argomento sono stati ampiamente dibattuti. I due rami del Parlamento hanno nominato apposite Commissioni, così hanno fatto alcuni Ministeri; ma i risultati concreti sono ancora del tutto insufficienti.

« Infatti riesce molto più facile denunciare uno stato di cose, anzichè prendere provvedimenti specifici. Si versano fiumi di lacrime sincere sullo sfacelo dell'ambiente in Italia.

« Finchè si rimane nel generico, tutto va bene - anche se qualcuno obbietta che le visioni che si presentano sono troppo apocalittiche - e più d'uno ancora vorrebbe attenersi alla politica del *laisser faire*, dell'affidarsi al caso, alla forza riparatrice della natura, ai meccanismi di autocontrollo, o che so io. Ma quando si scende al caso singolo, al fatto specifico, alle denunzie di situazioni precise e localizzate, e si tenta di imporre vincoli o limitazioni, e si toccano così interessi privati, subito si mettono in moto vivaci e violente reazioni, che mettono in moto uno schieramento di forze coalizzate, che oppongono resistenze spesso insormontabili.

« Ora bisogna persuadersi che è finito il tempo delle chiacchere, delle denunzie generiche, dei propositi vaghi. Bisogna instaurare una seria politica dell'ambiente. Siamo già in ritardo rispetto ad altri paesi. Bisogna cercare di coordinare le attività che per ora sono più o meno frammentarie ed episodiche.

Concludendo, il prof. Montalenti ha formulato una precisa proposta: « Ora ritengo - ha detto - che sia dovere del CNR, massimo organo di consulenza scientifica e tecnica dello Stato, mettere fra i suoi scopi, in posizione di priorità, un *programma per l'ambiente*. Il CNR può contribuire a diversi aspetti di questo argomento: in particolare: alla ricerca, alla segnalazione dei luoghi da proteggere, alla educazione del pubblico, alla preparazione del personale da immettere negli uffici destinati ai problemi dell'ambiente, nonchè, secondo il mio modo di vedere, agli studi legislativi ».

A White Paper on the Protection of the Natural Environment

With a delay of two years, due to various causes, the Italian National Research Council has published its White Paper on the preservation of nature. It deserves to be emphasised, meanwhile, that, for the first time in Italy, the problems of the protection of the natural environment are approached with

the necessary degree of strictness and awareness, instead of with the dubious instruments of general goodwill and high-minded proposals that have governed all the enterprises undertaken so far.

The White Paper contains a precise analysis of the alarming situation that has arisen in Italy, which has been very little relieved by the fanciful and fragmentary action both of public powers and of groups and associations. A diagnosis, formulated without reticence - such that it often assumes the character of an accusation - is followed by a series of proposals and suggestions.

In presenting the document, Prof. Giuseppe Montalenti, President of the NRC committee that has carried out the lengthy study, indicated the necessary presuppositions for a serious examination of the problems connected with the preservation and protection of nature:

« *Overall conception of the natural environment.* Ecological science shows that it is not possible to contrive a method of keeping the various environmental factors separate, as it is necessary to do for an analytical study of them. In nature the elements of an ecosystem are united by ties of inter-dependence that are sometimes intricate and difficult to discover. As far as possible, the environment must therefore be considered as a whole, and the various interventions - which must necessarily be what is usually termed 'sectorial' - should be as far as possible coordinated within an overall view of the question. Any ecologist can supply a vast number of examples of secondary or tertiary effects, brought about by the alteration of one of the terms of the balance - effects that at times appear very far removed from the initial intervention and are difficult to foresee ».

« *Legal conception of the natural environment.* I am not a jurist and so the ideas that I express on this subject may be wrong. But I do not refrain from expressing them, just in order to call the attention of jurists to this point, which seems to me to be highly important. If they can show me that suitable legislation exists, so much the better; it will merely be a question of applying it. As far as I know, in the laws in force in Italy (I cannot speak of other countries), there exists no legal definition of the environment as *heritage of the community*, and consequently there exist no laws for ensuring its protection in the interest of all. In substance, what prevails is the concept of private property: whatever is not private property is *res nullius*, such as the fauna or the spontaneous flora. Certainly, unless I am mistaken, there are *res communes omnium*, but jurisdiction is always based on the concept of private property: that is, of the advantages or harm that may result for a landowner from an alteration made by another - for example, in the air or in the water of a river.

« As far as I know, there is no legislation governing the direct relationships between man and his environment: namely, his relationship with those common things that are the heritage of all and, as *common property*, must be protected.

« I maintain that the time has come for jurists to tackle this problem and propose suitable legislation ».

« *Conception of environmental problems on a world scale.* It is obvious that the protection of environment and, in general, of common property, is a problem having a local or national aspect; but it goes beyond this and is to be regarded on an international, world level. There is no sense in considering the problem of migratory birds as an Italian, Yugoslav or Scandinavian problem; there is no sense in dealing with the problem of the pollution of the Mediterranean if not on an international scale; likewise, there is no sense today in considering the problem of the lagoon of Venice as a factor that exclusively concerns Italy and the Italians ».

Prof. Montalenti then discussed the various types of action to which the protection of the natural environment is entrusted in Italy. The following are the fundamental points in this disturbing balance-sheet:

« *Research.* This may be aimed either at ascertaining the conditions of biotopes or of balanced biological states that are threatened or on the point of disappearing or at particular subjects, such as the degree of contamination of certain environments, the toxicity of certain products, the best means of protection against upsetting actions, etc.

« Such activity is of great importance and is engaging the attention of numerous Institutes and laboratories in Italy, within the spheres of their particular competence. Many University Institutes, certain Institutes, Laboratories or Centres of the NRC, the laboratories of various Ministries, as well as various non-State institutions, are at present engaged upon research programmes more or less closely connected with the problems of the preservation of nature.

« The Committee for the Preservation of Nature, which has no available funds of its own, has organised (by obtaining the necessary financial means from the Committee for Biology and Medicine) a research programme concerning the fauna and flora of the small islands. As is well known, these islands represent environments of particular interest, since the insular isolation and the different geological characteristics have allowed the differentiation of local populations and the establishment of endemic features. In nearly all the Italian islands (the Tremiti, Eolie, Egadi, Ponziane and Tuscan islands, and those off the coasts of Sardinia), with a few important exceptions, the preservation of the natural environment is seriously threatened or is already irremediably compromised by being destined for purposes of tourism or for building speculation.

« An overall look at the research activities in the field of the preservation and protection of nature in Italy reveals above all the total lack of *coordination*. There is no central organ that indicates which are the more important or urgent problems or occupies itself with guiding and coordinating these activities. The result is that they are mostly entrusted to the enterprise and goodwill of individuals or research organs, whose view of the problems is necessarily partial and therefore limited ».

« *The information and education of public opinion.* This is a highly important activity - in a certain sense, the most important one - because it constitutes the indispensable basis for the success of any concrete action at legislative and executive level.

« What is necessary is the formation of a naturalistic - or, if one prefers it, an ecological - consciousness, so that society may be able to produce, at the various levels, persons that are responsible and conscious of the importance of the various problems and known what are the most suitable methods of solving them.

« In this field it may be said that great progress has been and is still being made. The various media of information, from the school to the press, the cinema, the radio and television, have been employed in voicing ecological problems, such that, as I said at the beginning, it is possible to speak today of an actual 'ecological explosion' ».

After mentioning the action taken by various organisms, Prof. Montalenti continued:

« It is to be hoped that schools, museums, zoological gardens, botanical gardens, newspapers, cinemas, radio, television and other media for the spread of culture at the level of the general public may finally help to arouse a love and respect for nature - of which there is so little in Italy - and to instil a sense of the importance of the scientific discipline in modern culture - an importance that is generally unacknowledged in Italy, since it is oppressed by the dominion of the literary, historical and philosophical disciplines in Italian school programmes ».

« *Indication of the places to be protected.* This is one of the fields in which information leaves most to be desired and is most affected by the chronic Italian evil: fragmentary activity, fortuitousness and lack of coordination.

« The NRC Committee has undertaken certain activities in this respect. In 1968 it published a list of the areas or phytocoenoses of a Mediterranean nature for which complete protection is proposed within the sphere of National Parks, Forest Reserves or other types, and which, when they are not already public property, are proposed for acquisition by the State. This work has been carried out by Prof. Roberto Corti of the Istituto Botanico Agrario e Forestale in Florence.

« The present speaker has been responsible for the publication (1967, Vol. 38 of « Ricerca Scientifica ») of a Report on the protection of the lagoons and enclosed coastal waters of the peninsula and the large islands, with a list and description of the biotopes, beginning with the lagoon of Venice. Today, when the preservation of water areas - after the excesses of complete land reclamation - has become a problem of world interest, this list should be given the maximum consideration by the relevant executive organs. But the most outstanding work that the Committee has produced, in collaboration with the Ministry of Public Works, the General Management of the Studies and Projects Service for Town-Planning, is the « Programme of territorial research on the nature areas to be protected ». It consists of a « Map of the Biotopes of Italy » and a series of « Regional Maps of biotopes » of which only the one for Lazio has so far been published ».

No less discouraging is the picture that Prof. Montalenti drew of the state of Italian legislation in the matter of the protection of nature:

« The situation is chaotic: also here interventions are mostly uncoordinated and are often episodic and fragmentary.

« The confusion of spheres of authority, whether for the preparation of legislative action or with executive powers, is frightening. No less than 14 Ministries have various spheres of authority with regard to problems of the protection of the natural environment. In alphabetical order, these are: the Ministries of Agriculture and Forestry, Defence, Education, Finance, Foreign Affairs, Health, Home Affairs, Industry and Commerce, Labour and Social Security, Mercantile Marines, Mercy and Justice, Public Works, Tourism and Entertainment, and Transport. To these may be added the Presidency of the Council of Ministers and the Ministry of Scientific Research.

« Many of the laws drawn up by some of these Ministries and issued by Parliament are good, as are also many provisions issued directly by the Ministries. Others are imperfect or wholly insufficient. In particular, the old law no. 1497, of June 29th 1939, still in force, on the protection of the beauties and scenery of nature, is inadequate mainly because it is prevalently concerned with the protection of elements of aesthetic value, while naturalistic elements are ignored. Also in this field, what would be needed is again 'coordination'. In my opinion, an initial step might be the assembling and collating of all the laws, regulations and individual provisions into a single text.

« As early as 1964-65, the NRC Committee embarked on the enterprise of studying legislative questions, and a select Committee, under the Presidency of Prof. M. Salmi, prepared a draft law in which the protection of the beauties and scenery of nature is presented by placing aesthetic elements and naturalistic elements on the same plane. But the project came to nothing. In this respect, the NRC Committee suggested that the protection of the beauties and scenery of nature be entrusted not to the Superintendency of Monuments but to a « Superintendency of the countryside »; it proposed the institution of such a body, to which should be attached staff specifically trained in this particular field. But this proposal also remained abortive.

« In 1970 the Committee devised a draft law for the protection of fauna, which was to replace the existing law on shooting. It is published as Appendix IV to the White Paper. Later, a group of Senators took this as the starting point for presenting bill no. 1780 to the Senate. Let us trust that it will be incorporated in the new legislature.

« The Committee has also been concerned at length with the complex problem of National Parks in Italy - a highly controversial subject, as everyone knows, which had better not be mentioned, such is the disgrace of certain situations ».

The same chaotic situation is found in the sector of executive instruments and organs of control. As Prof. Montalenti noted:

« Certain Ministries - in particular those of Agriculture and Forestry, Education, Public Works, Finance (State property) - have the power of imposing restrictions, establishing parks and reserves and approving or rejecting development plans: in short, of protecting the national heritage in the sphere of nature and art. Many of these administrations deserve praise for the work of protection that they have been able to carry out. But for the most part they operate amidst great difficulties and are often unable to resist the strong pressures from private economic interests, which take the form of political intervention.

« Again, certain of the powers of the central organs are delegated to the administrations of the Regions, both those having a Special Statute and those having an ordinary Statute. As far as I can see, in many cases one is still at the state *de iure condendo*. However, the needs of the safeguarding of the natural environment would have to be suitably protected in assigning the powers.

« For separate small-scale interventions, the spheres of competence are distributed among a large variety of authorities: Chief Administrative Officer, Provincial Administration, Provincial Inspectorate of Labour, Provincial Doctor, Fire Service, Communal Council, Mayor, and so forth. Also here, greater coordination is to be hoped for, and above all a stricter application of the laws in force. As has been shown by recent interventions of the Magistracy, these laws exist.

« What is required - Prof. Montalenti added - is a central organ that would have the task of coordinating the multiple activities of the various administrations and would put into effect - in a country such as ours, where individualism prevails - a unitary policy for the natural environment. It would probably be better to avoid the institution of a special Ministry - that is, a ponderous bureaucratic organism, which would come into competition with the other already existing organisms. In the present state of affairs, it would be preferable for an organism such as the Inter-ministerial Committee for Economic Planning (CIPE) to assume the task of planning and directing the policy for environment ».

Prof. Montalenti went on to say: « The protection of the natural environment is one of the most important and urgent problems at the present moment, both in Italy and throughout the world. It is needless to stress this fact again here, since public opinion is by now well informed about it from numerous authoritative sources. The general aspects of the subject have been fully debated in many (perhaps too many) meetings, interviews, round table discussions and conferences. The two branches of Parliament have appointed special Committees, as certain Ministries have also done; but the actual results are still wholly insufficient.

« In fact, it is much easier to make known a state of affairs than to take specific measures. Buckets of tears may truly be shed over the ruin of the natural environment in Italy.

« As long as the matter remains on the generic level, all is well - even if some object that the visions presented are too apocalyptic - and there are several that would like to follow the policy of *laisser faire* or of trusting to chance, to the restorative force of nature, to self-governing mechanisms, or the like. But when one gets down to individual cases, specific facts and the reporting of precise localised situations, and one attempts to impose restrictions or limitations and thus comes up against private interests, violent and strenuous reactions are immediately set in motion and united forces are marshalled, offering resistance that is often insurmountable.

« We must now realise that the time for empty talk, generic denunciation and vague proposals is over. A serious policy for the natural environment must be established. We are already behindhand in comparison with other countries. An attempt must be made to coordinate the activities that are at present more or less fragmentary and episodic.

In concluding his report, Prof. Montalenti made a definite proposal: « Now I maintain - he said - that it is the duty of the NRC, the chief consultant organ to the State in scientific and technical matters, to include among its aims, in a position of priority, a *programme for the natural environment*. The NRC can contribute to various aspects of this subject: in particular, to research, to the selection of the places to be protected, to the education of the public, to the training of the staff to be employed in the offices devoted to the problems of environment, and also, to my way of thinking, to legislative studies ».

Un messaggio all'umanità

Un messaggio per più di un verso drammatico, indirizzato all'umanità intera, è stato redatto e firmato nelle scorse settimane a Mentone da più di duemila scienziati appartenenti a ventitrè paesi. Nell'appello rivolto « a tutti gli abitanti del pianeta Terra », essi scrivono: « Ad onta delle distanze geografiche e delle differenze culturali e politiche, siamo tutti minacciati da un comune pericolo che per la sua vastità non può essere paragonato a nessun altro finora affrontato dall'uomo. I guasti irreparabili prodotti all'ambiente naturale si vanno aggravando con una rapidità che non ha precedenti; sostanze nocive penetrano di continuo nelle catene alimentari; residui petroliferi e scorie industriali hanno inquinato tutte le riserve d'acqua dolce e le acque costiere di tutto il mondo; le centrali nucleari proliferano e si accrescono i loro effetti deleteri sull'ambiente. Il disboscamento, le dighe, la monocultura, l'uso incontrollato degli antiparassitari e degli anticrittogamici, le miniere a cielo aperto, hanno determinato uno squilibrio ambientale che non potrà non avere conseguenze catastrofiche ».

« Sulla Terra - prosegue l'inquietante messaggio - vivono oggi tre miliardi e mezzo di uomini e nel 2000 ve ne saranno sei miliardi. Attualmente due terzi della popolazione mondiale soffrono per malnutrizione e si prospetta la minaccia della fame su larga scala. Negli arsenali sono state accumulate armi nucleari sufficienti a spegnere ogni forma di vita sulla Terra. Sappiamo con certezza che la Terra e tutti i suoi abitanti sono minacciati ».

A questo allarmante quadro, il messaggio dei duemila scienziati fa seguire la proposta di alcuni rimedi, definiti necessari ed urgenti, ai quali si possono affidare le superstiti speranze di salvezza. « Prima che i danni diventino irreparabili - dice il messaggio - debbono essere adottati i seguenti provvedimenti: differire l'applicazione di innovazioni tecnologiche delle quali non possano essere previsti con certezza tutti gli effetti; non usare trasporti di lusso, nuovi tipi di armi, preparati antiparassitari, nuove materie plastiche; non costruire dighe sui grandi fiumi, non distruggere la giungla, non estrarre minerali dai fondi marini. Inoltre controllare l'inquinamento, frenare l'aumento demografico, sollecitare le classi privilegiate a diminuire la quantità dei consumi. Infine tendere con il massimo impegno ad abolire le guerre, quali che siano le difficoltà che le nazioni debbano affrontare per pervenire ad un accordo ».

Chiunque si renda conto di quanto è avvenuto negli ultimi decenni sul nostro pianeta, sarà certo ben lontano dal considerare ingiustificato il tono apocalittico di questo appello. E tuttavia l'iniziativa dei duemila scienziati, indubbiamente nata dagli intenti più nobili e meritoria nel più alto grado, è destinata a restare priva di qualsiasi efficacia.

Essa, infatti, non riesce a liberarsi dal vizio di una generica neutralità rispetto ai conflitti che dividono il mondo. Non sembra, infatti, che siano in grado di accogliere l'invito a rispettare i delicati equilibri della natura quei due terzi dell'umanità che soffrono la fame. Nè quello stesso invito ha grandi speranze di essere accolto dalle potenti minoranze che hanno sempre fatto prevalere su ogni altro criterio il desiderio di realizzare il massimo lucro. Non è, in definitiva, la fame dei più a minacciare l'integrità dell'ambiente naturale, ma la sfrenata avidità di pochi: e non è probabile che a porle freno basti l'appello di un manipolo di studiosi.

A Message to Mankind

A message addressed to the whole of mankind, and dramatic in many senses, has been drafted and signed during these last weeks at Menton by over two thousand scientists belonging to twenty-three countries. In their appeal, addressed to « all the inhabitants of the planet Earth », they write: « In spite of geographical distances and cultural and political differences, we are all threatened by a common danger which, on account of its vastness, cannot be likened to any other so far faced by man. The irreparable damage done to the natural environment is steadily becoming more serious at a rate that is without precedent; harmful substances are continually penetrating the nutritional chains; petroleum residues and industrial slag have polluted all the fresh-water reserves and coastal waters throughout the world; nuclear power stations are proliferating and their harmful effects upon the environment are increasing. Deforestation, dams, mono-cultivation, the uncontrolled use of anti-parassite and anti-cryptogam substances, open-cast mining have resulted in an upsetting of the balance of the environment that cannot but have catastrophic consequences ».

« On the Earth - continues the disturbing message - there are living today three and a half thousand million human beings and in the year 2000 there will be six thousand million. At the present moment, two thirds of the world population are suffering from malnutrition and there looms the threat of hunger on a wide scale. In the arsenals there is sufficient accumulation of nuclear weapons to wipe out every form of life on the Earth. We know for a certainty that the Earth and all its inhabitants are threatened ».

In the message of the two thousand scientists, this alarming picture is followed by the proposal of certain remedies, defined as necessary and urgent, in which the surviving hopes of salvation may be placed. « Before the damage becomes irreparable - states the message - the following provisions must be adopted: to defer the application of technological innovations, the effects of which cannot be foreseen with certainty; not to use luxury means of transport, new types of weapons, anti-parassite preparations or new plastics; not to build dams on big rivers; not to destroy the jungle; and not to extract minerals from the sea bottom. In addition: to control pollution, check population increase and urge the privileged classes to reduce their consumption of goods; and finally: to make the greatest effort to achieve the abolition of wars, whatever may be the difficulties that nations have to face in order to reach an agreement ».

Anyone that is aware of what has happened on our planet during the last few decades will certainly be far from considering that the apocalyptic tone of this appeal is unjustified. Nevertheless, this enterprise of the two thousand scientists, undoubtedly prompted by the most noble intentions and meritorious in the highest degree, is destined to remain entirely ineffectual. In fact, it does not succeed in escaping from the fault of a generic neutral attitude towards the conflicts that divide the world. Those two thirds of mankind that are suffering hunger are certainly not in a position to respond to the invitation to respect the delicate balances of nature; nor is there much hope of any response from the powerful minorities that have always placed the desire to make maximum profit before any other criterion. In short, it is not the hunger of the many that is threatening the integrity of the natural environment but the unbridled greed of the few: and it is unlikely that the appeal of a handful of experts will suffice to check it.

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RECENSIONI - BOOK REVIEWS

Science and Synthesis (An International Colloquium organized by Unesco on the Tenth Anniversary of the death of Albert Einstein and Teilhard de Chardin), Springer-Verlag, Berlin, 1971, XVIII+206 pp., DM 38,50.

This volume is a collection of papers, discussions and lectures delivered at the international colloquium on the subject of « Science and Synthesis » which was organized by Unesco to mark the tenth anniversary of the death of Einstein and of Teilhard de Chardin as well as the fiftieth anniversary of the general theory of relativity. The more particular purpose of the colloquium was to pinpoint and focus attention upon a need which, as the organisers see it, underlay the work of these two scientists (though the direction of their thought was so very different) namely the need for synthesis, the need for a totally comprehensive conception of the universe and for a cosmology of the modern world. Following out this theme, the volume contains retrospective tributes to the work of Einstein interspersed with discussions on the relationship between synthesis and science, before concluding with a comprehensive re-examination of the work performed by Teilhard de Chardin. The first part includes a paper by F. Gonseth on Einstein's earliest scientific discoveries and on the philosophy of nature; one by Heisenberg on field theory and on the problems raised by indeterminism in physics; one by G. Holton on Einstein's conception of reality; Oppenheimer gives an account of his scientific contacts with Einstein and there is a series of papers by J. Huxley, B. M. Kedrov, D. Dubarle, G. de Santillana all of which expand Einstein's first pronouncement on theoretical physics into the more general field of the natural sciences, biology and history (here meaning history of the aims and achievements of science). The « round table » following upon this has three basic themes - namely: « Einstein: from plurality to unity »; « Towards a cosmology »; « Determinism and indeterminism » - after which there is an account of a debate on the organization of scientific research. Among the many who took part in this, besides those already mentioned, we find the names of L. de Broglie, J. P. Vigier, A. Matveyev, A. Lichnerowicz, J. Merleau-Ponty, O. Costa de Beauregard. The last part of the volume is devoted to Teilhard de Chardin, including contributions from J. Pivetau, P. Leroy, F. Meyer, P. Chouard and others.

At least *prima facie*, the philosophical orientation of the guiding lines for the Colloquium clearly appears to be that of Comte. In fact, according to the introductory address by R. Maheu, the key problem in scientific research is the problem of unification and synthesis. The demand that science makes *is* a demand for unity, This, however, does not mean merely a unity of knowledge, but above all a unity of minds, which is just what Comte wanted. That is how science manifests itself as a social fact: it is itself society. Thus the road is being cleared for a swift return once again in a Comtian direction, towards what was in origin a religious structure provided by the Church but is now broadened, in a more or less secular sense, from being a restricted community of believers to become a community of all mankind, a scientific and social *corpus* whose connecting tissue is the ever more rigorous discipline of scientific procedure.

It is from this standpoint that may best be understood why Einstein is coupled here with Teilhard de Chardin, or rather how it is proposed that the work of these two thinkers should be regarded. The standpoint is of interest in itself as revealing a cultural framework upon which science, and some of the institutions for research and education it shares with society at the present level, could be directed towards certain ends.

It is well known that Teilhard saw life as nothing other than a specific effect caused by the increasing complexity of matter. It is indeed just this parameter, expressing complexity, that lends meaning to the universe having evolved before the appearance of life. It is this, combined with the other fundamental parameter - namely, organisation centered upon self-knowledge - that enables us to chart the evolutionary progress of the universe and, after life appeared, that of mankind. It is man's cerebral organisation that places him at the highest point in the development of the universe, the point at which the moral world is revealed, and at which life is revealed to itself as self-organisation and self-knowledge penetrated by ethical feeling, manifesting man's original destiny, rendered explicit through evolution.

Pictured in this way, what better example could there be of the unity attained by the world of knowledge, and at the same time of the unity of self-knowledge by minds, than that given by Einstein as the key scientist of our age? What better example of how morality is associated with complesity of development than Einstein's sad participation in the destiny of man in the destiny of his life and of his possible disappearance? What better example could there be of general relativity, of an all-embracing cosmology, itself drawn from the cosmic evolution under examination?

It is on the scientific and technical plane that an outlook of this kind very soon shows its limitations. It is in fact impossible to read Einstein, and what he has written on special and general relativity, merely in the light of the simple evolutionary parameters of Teilhard de Chardin. The best of the contributions to this volume are those that more or less implicitly emphasize the continuity of the links existing between Einstein's scientific work and the times in which he lived, apart from theological and unhistorical considerations.

The most trenchant passages in this volume, those in which the personality of Einstein leaps to life, are those that describe his relations with Mach, with the central European scientific world, with American culture between the two wars and with the world of the young men of his time: Born, Heisenberg and others who pursued Einstein's work but seemed almost to be violating its most secret principles.

And just at that moment there appears a sort of hidden - yet not too hidden - providential design: a design for unity, for the synthesis of all science, making its appearance as a plan, a project, an imperative that demands to be pursued from within, for using science and those instruments that are peculiar to science in accordance with a prospect which is not predetermined but which can be given birth only as the result of a real effort to transform the structure of scientific and social research: an effort that devolves specifically upon those who are the bearers of science, that is to say upon men in the course of their practical scientific activity.

Thus it is the rationalistic realism of Einstein, which was an astringent critic of the remaining of positivism, that runs through the reminiscences and the testimonials contained in this volume like a secret thread, quietly cancelling out every attempt to use Einstein's thought in metaphysical ways in the light of the Comtian view to which reference has been made.

It is this encounter, this real debate superimposed upon the official one, that animates the volume under review and reveals its truest interest by allowing us to feel as if Einstein were alive within that wider discussion on the organization of culture in human society, central to the moment of history in which we ourselves live.

Roma, Università, Istituto di Filosofia.

A. Carsetti

L. Krauth, *Die Philosophie Carnaps* (Series: *Library of Exact Philosophy*, No. 1), Springer-Verlag, Wien, 1970, x+234 pp., DM 46.

There can be no doubt that R. Carnap was one of the greatest philosophers of this century and the most representative of that group of philosophers which is known as the Vienna Circle. His thought went through several phases which can, at least in the first instance, be grouped under two periods: the European period characterised by his unconditional adherence to neopositivism and the American period during which his ideas were profoundly influenced by current Americal philosophical trends, especially pragmatism. The book by L. Krauth displays Carnap's thought by way of a careful survey of the major philosophical problems considered by him, ranging from semantic studies in search of an empirical criterion of meaning to an analysis of the theoretical terms of science and inductive logic. The note of discouragement in Carnap's thought is brought out clearly and with depth of understanding. Reading of this book affords a complete survey of Carnap's fundamental work. In our opinion it has only one defect in that Krauth has made his estimate of Carnap's philosophy with almost complete disregard of its technical content. For, as Krauth imhself very justly remarks, philosophy meant to Carnap essentially the philosophy of science and consequently an analysis of the foundations underlying scientific disciplines. To disregard the technical part of his work is to run the risk of misrepresenting Carnap's thought and to convey a distorted idea of what he understood by philosophy.

Perugia, Università, Istituto di Matematica.

D. Costantini

M. Swain (ed.), ***Induction, Acceptance, and Rational Belief***, D. Reidel Publishing Company, Dordrecht, 1970, viii+232 pp., figs., Dfl. 40.

The main theme of this collection of essays in the « rôle that induction plays in theories of knowledge and rational belief... » The first, by F. Schick divides the various approaches to the theory of rational belief into three types (*subjectivism, empiricism and pragmatism*). However, the corresponding theories are complementary rather than incompatible. Besides, these theories have certain general principles in common (« overaching principles ») which are combined in the second essay (by M. Swain) into « a general theory of rational belief » which is intended to regulate « the admissibility of propositions into sets of rationally believed propositions ». H. E. Kyrburg Jr. continues the discussion of « overaching principles ». After an analysis of the rules of acceptance based on the principle of conjunction (« if two statements are rationally accepted, then their conjunction ought to be accepted »), he develops a system of rational beliefs without presupposing this principle. For G. H. Harman the problem of induction can be clarified only by the theory of knowledge, as « neither deductive logic nor the probability calculus has anything to do with inference ». Also K. Leher's essay dwells on this problem (« every belief that is justified is so either because of what it explains or because of what explains it ») and thereby defines a theory of the justification of induction by means of the explanation of inferences in a system of beliefs. I. Levi's essay is a criticism of Carnap's « theory of rational probability judgment » and, lastly, R. C. Jeffrey stresses the importance of coherence in a system of beliefs. At the end of the book there is a long bibliography on the subjects treated.

Roma, Università.

V. CINANNI

K. LAMBERT (ed.), *Philosophical Problems in Logic* (Some Recent Developments), D. Reidel Publishing Company, Dordrecht, 1970, VIII+176 pp., Dfl. 38.

The essays collected in this volume come from a colloquium held on free logic and modal logic at the University of California in 1968. In the first essay K. Lambert and B. C. van Fraassen demonstrate how, in questions concerning truth *ex vi terminorum*, the logic of possible objects gives better explanations than the logic of modal quantifiers. Language defined as logical space is insufficient to represent all the possible worlds and thus the modal operators acting on its structure cannot give us virtual objects not representable in it. On the same level of presuppositionless logics is professor Hintikka's discussion of the principle of substitutivity of identity of Quine. He makes an interesting proposal for a referential semantics with an ontology deriving from functions of individuation which allows various embodiments to be identified in different possible worlds as « manifestations of *one and the same* individual ». Two technical studies, the first (by R. H. Thomason) on two systems of calculus of first order predicates with definite descriptions, the other (by H. Leblanc and R. K. Meyer) on « Truth-value semantics for the Theory of Types », are followed by an important contribution by J. M. Vickers on the relationships between probability and non-traditional logics. The difficulties relating to the principle of indifference also depend on the use of a notion of probability based on the *possible case* notion. This is why it is necessary to state explicitly the relations between belief, probability, reasonableness and coherence in a general framework in which only implication is used. This is followed by a new proposal for a modal logic and a discussion of certain difficulties of a semantic origin.

Roma, Università.

V. CINANNI

A. C. CROMBIE, *Da S. Agostino a Galileo* (Storia della Scienza dal V al XVII secolo), Feltrinelli Editore, Milano, 1970, VIII+590 pp., figs., L. 5.000.

The main feature of medieval scientific thought is, as Crombie points out, a « great interest in the theory and logic of experimental science ». But, at least to the modern mind, it seems contradictory that the complicated epistemological discussions on the importance of experiment did not correspond in practice to any consideration of experimental data even by those who openly took up empirical positions. The revival of the idea of « Euclidean-type explanation » clearly implies the acceptance of a basic Neo-platonism which holds that deduction is the only possible kind of explanation and thus allows the extension of mathematics to physics and upsets the Aristotelian distinction between the various sciences. This « quantification of quality » can be compared with the new approach to the problem of motion and the correlated one of space; the state of rest loses its privileged position and between rest and motion there is no longer any ontological distinction. This is the period in which the basic premises were laid for the discussions on space and loci which were to lead to the « explosion of the spheres » and infinite space. During the Middle Ages there was no lack of technical progress. The new « machines » constituted the first *models* of the human body and of the world. On the other hand, the new instruments with their precise measurements tooks theory out of the realm of pure explanation and placed it in the wider field of explanation-prediction. A large contribution was given to this revolution by medicine, physiology and anatomy. The crowning achievement in this direction was W. Harvey's treatise on the circulation of the blood which constitutes « a model of method ». In reality, the development of medieval thought was not continuous.

The author reveals that there was a period of stagnation in the production of truly new scientific and methodological works during the XV century because « interest and intellectual originality were turned more to literature and plastic arts than to natural science ». This apparently explains the scientific revolution of the XVII century. The spread of medieval scientific thought by means of printed books marked the beginning of a *new phase* in the intellectual movement of the XIII and XIV centuries. Thus the attitude of the philosopher-mathematician of the XVII century towards experimentation was basically the same as that of the Neoplatonic methodologists of the XII and XIV centuries. Even Galileo based his *deductions* on experiments that could, in this time, only be carried out mentally.

Roma, Università.

V. CINANNI

M. **Bunge**, *La causalità* (Il posto del principio causale nella scienza moderna), (Series: *Universale Scientifica*, No. 47-48), Editore Boringhieri, Torino, 1970, 468 pp., figs., L. 2.500.

Directly controverting humanistic solutions to the problem of causality the author here asserts that this problem, so far from being one of *relations between ideas*, is a category of *connection and determination* corresponding to a real characteristic of the factual world. Within the general ambit of deterministic categories the principle of causation plays only a very limited role because statistical, teleological and dialectical connections are equally deterministic (*constant* and *univocal*). His treatment of the question includes an analysis of the various ways (empiricist, romantic and neopositive) in which the principle of causality has been formulated and criticized. Even if the hypotheses of continguity and antecedence are compatible with a causal connection they fail to express its dynamic characteristic of « *producing* things (which are new numerically if in no other respect) from other things ». Nor is this dynamic characteristic any better expressed in terms of functional dependence or by differential equations, for any relation, however logical or mathematical it may be, has need of additional semantic rules if it is to express an extra-logical characteristic. Causal chains are linear only within small regions of space and over limited periods of time for the purpose of « escaping from sharply defined qualitative discontinuities ». Causal doctrines isolate only one type of change; they take no account of « real change » which is a « combination of several types of change ». The fact that natural laws are not logically necessary tells us only that the law of causation is an ontological problem. Therefore « *its logical aspect is as much syntactical as semantic* » and every solution is subject to semantic rules as well as to the empirical sciences. In conclusion, some problems relating to scientific explanation, prediction and the part played by the causal principle in modern science are considered.

Roma, Università.

V. Cinanni

P. V. **Tavanec** (ed.), *Problems of the Logic of Scientific Knowledge*, D. Reidel Publishing Company, Dordrecht, 1970, xii+430 pp., Dfl. 95.

This book is a translation from the original Russian which was published in Moscow in 1964. It is a full anthology of writings on the general problems that arise in the philosophy of science examined from the standpoint of dialectical materialism. The very first paper in the book indicates precisely the presuppositions that underly the various investigations following, and there is unanimous criticism of methods derived from psychological and neopositivist conceptions of logic. The progression of scientific knowledge and the origin of new categories and new abstract concepts which science uses cannot be represented by formal logic. Science does not recognize logic as a real process sustaining the movement of thought or accept that the whole human race is subject to such a process. Moreover « a conceptual apparatus is developed in order *to be applied* to an empirical subject matter and to solve related practical tasks.

Logic is no exception! » Therefore the analysis of language at the syntactical and semantic levels is supplemented at a pragmatic level which can be explained in terms of man's linguistic activity, corresponding to « objectively conditioned, socially necessary, logical norms of the movement of thought, embodied in scientific and ordinary thought ». Dialectical logic built upon the foundation of dialectical materialism provides a complete answer to these needs. The following papers, all starting from similar presuppositions, deal in detail with all the main problems of scientific explanation (and with direct references to non-dialectical developments) ranging from the complex relationship between empirical and theoretical knowledge, the logic of probability, and questions relating to the inference by analogy, to the problem of definition and finally to the methods of statistical inference.

Roma, Università.

V. CINANNI

W. K. ESSLER, *Wissenschaftstheorie I* (Definition und Reduktion), Verlag Karl Alber GmbH, Freiburg, 1970, 162 pp., No price stated.

The present volume is the first of a series of four devoted to several of the most important branches of modern philosophy of science. The whole series is conceived as a work of highlevel divulgation and so makes constant demands on the reader's attention and effort. The subjects dealt with are: the theory of definition (the present volume), the relationships between theory and experiment, probability and induction, explanation and causality (the last three to be published later).

The present volume consists of a short but complete analysis of the most important aspects of the theory of definition. The author treats both real and nominal definitions and the criteria a correct definition must satisfy. He also examines other types of definition, such as implicit definitions, recursive definitions and explanatory definitions, thus giving a broad survey of this branch of studies.

The volume is opened by a short but exhaustive historical introduction which allows the reader to set the treatment that follows in its correct perspective.

Perugia, Università, Istituto di Matematica.

D. COSTANTINI

J. HINTIKKA, *Models for Modalities* (Selected Essays), (Series: *Synthese Library*), D. Reidel Publishing Company, Dordrecht, 1969, x+220 pp., Dfl. 34.

A series of articles published by the Author in various reviews between 1957 and 1969 are reprinted in the present volume. Although the articles all have a common theme, they should not be considered as the various chapters of a single book and can be read quite separately.

The subject dealt with is modal logic examined from the semantic standpoint with a view to illustrating the philosophic use of modal logics and the philosophic methodology underlying such a use. The Author is undeniably one of the greatest contemporary logicians and the present volume provides a good opportunity of making an overall analysis of the considerable contributions he has made to the development of these logics which constitute one of the most fascinating sectors of modern symbolic logic. The book is thus of great interest, but a fairly advanced knowledge of symbolic logic is required to be able to understand it.

Although not directly related to modern inductive logic, these essays enable a better understanding to be had of Hintikka's system of inductive logic and are thus useful reading matter also for specialists in the foundations probability.

Perugia, Università, Istituto di Matematica.

D. Costantini

W. A. Deterline, *Introduzione all'istruzione programmata*, Zanichelli Editore, Bologna, 1970, xvi+148 pp., L. 1.800.

There is the danger that programmed learning will become a sort of mythical panacea for all kinds of teaching ills including overcrowding, or else big business. This well-conceived and sensible handbook by W. A. Deterline has appeared just at the right moment and G. Gozzer's introduction sums it up nicely as an answer to the question: «programmed learning, a miracle or a menace?»

The answer is simple and practical. The book starts off from the methods of Pressey, Skinner and Crowder and goes back over the principles of learning and then on to those of self-teaching. A specific answer as to learning efficiency can only be given for each individual case, according to the subject, the level of studies and the number and quality of available teachers. For, if a lot of good teachers were always available, the importance of programmed learning would be reduced considerably.

Moreover, in actual fact, a teaching technique based on the criteria emerging from modern information theory and synectics has still to be developed.

The book contains a good and well-selected bibliography. There is also an appendix with a very skilfully 'programmed' presentation of information on tests and their interpretation.

Roma, Studio di La Nuova Critica.

V. Tonini

R. E. Schofield, *Mechanism and Materialism* (British Natural Philosophy in an Age of Reason), Princeton University Press, Princeton, 1970, 336 pp., $9.50.

The eighteenth century was the age of Enlightenment and of the triumph of Reason, which was to have provided a solution to all the problems the human mind set itself. Following the Cartesian inspiration, it was above all on the continent of Europe that Enlightenment found the most fertile soil for its development.

In England the patron saint of science was Bacon and, according to Schofield, the age of Enlightenment in this country was tempered by common sense. The epistemological declaration characterizing British scientific works throughout almost the whole country was the refusal to put forward hypotheses and the assurance that a cautious and complete inductive process would be followed. English natural philosophy during the age of Enlightenment is thus generally considered to be essentially non-rational and empirical. This erroneous judgement which, generally speaking does not cause anything like the surprise it should, is found in all the literature dealing with XVIII century English science.

In this survey, the author sets out to debunk this belief and to show that English science in the eighteenth century did not consist only of empirical experimentation, nor was there a kind of hiatus between the geniuses of seventeenth-century mechanical philosophy and the geniuses of eighteenth-century classical physics. Between these two great periods there was a continuity of thought which the author traces back and illustrates by means of a careful examination of texts of physics, chemistry and physiology. This continuity of thought can also be traced through works of a clearly vitalistic nature up to the mechanism and scientific materialism of the XIX century.

The author starts his presentation with Newton's heritage and endeavours to clarify, not so much what Newton had to say, nor what nineteenth-century scientists thought of Newton, but rather what Newton learned and took over from English physicists and natural philosophers of the XVIII century. Part I, comprising three chapters, illustrates the development of mechanism and of corpuscular theory and its dynamics in the first half of the XVIII century. In the five chapters of part II, the author examines the state of natural philosophy in England after the Newtonian concept of ether had, by and large, been accepted. He also deals with the interactions with Continental physics and philosophy. This period, to which Schofield gives the title Ether and materialism, goes from 1740 to 1789. The third part, called Neo-mechanism, consists of two chapters and covers the period 1760-1815, which was profoundly influenced by the work of the Dalmatian R. J. Boscovich (1711-1787) *Theoria philosophiae naturalis* (Vienna 1758, Venice 1763). With the work done on the concepts of field and force and fluid dynamics by important workers such as Michell, Cavendish, Hershel, Priestley and Hutton, this historical period leads scientific thought back to Neo-mechanism, in an atmosphere of dominant materialism. The inter-regnum going from 1789 to 1815, in which Boscovich's ideas are taken up and passed on by John Robinson, John Leslie, Thomas Young, Humphrey Davy and his assistant Michael Faraday, becomes continuous with physics and English natural philosophy of XVIII century flows into the great age of classical physics, which was mainly the creation of English scientists.

From this point of view the author's decision to restrict his survey to eighteenth-century English authors of natural philosophy is fully justified both logically and historically. This thorough exhaustively documented work will be of great interest to historians of physics and chemistry and, partly, also to those of biology.

Roma, Università, Istituto di Genetica.

G. Montalenti

F. Giannelli, *Human Chromosomes DNA Synthesis* (Monographs in Human Genetics, No. 5), S. Karger, Basel, 1970, VIII+156 pages. No price stated.

The two main functions of the chromosomes are reduplication of deoxyribonucleic acid (DNA) on which is inscribed the genetic information and synthesis of ribonucleic acid (RNA) on which this information is transcribed to be later carried into the cytoplasm to the effector organs of the protein synthesis.

The duplication of chromosomes occurs during the period between two successive mitoses. It involves the DNA synthesis, the molecular structure of which repeats that of the molecule of origin, The scheme of the double helix structure, proposed by Watson and Crick (1953) and today generally accepted, as it is confirmed by numerous experimental data, offers a model for explaining the duplication of the long strand that constitutes the DNA molecule. Many questions regarding the methods of the duplication process have been solved by the use of substances labelled with radioactive isotopes. In particular, by supplying the cell with one of the bases that constitute the DNA, thymidine, in which the hydrogen atoms are replaced by the radioactive ^{3}H isotope, called tritium, it is possible, by recognising the location of the tritium by suitable methods, to follow the metabolism of the tritiated thymidine and hence have an idea of the kinetics of the construction of a new DNA molecular chain. One of the methods that has given the most accurate results is autohistoradiography, which consists in spreading a thin layer of photographic emulsion over the cells that have incorporated tritiated thymidine and waiting until the tritium atoms, emitting beta particles, produce black spots in the emulsion. Microscopic observation of the film thus impressed and of the specially stained underlying cells can therefore reveal the places where an intense incorporation of thymidine - that is to say, a DNA synthesis - has occurred.

In this way, after this technique had been introduced by Taylor (1957), it has been possible to study the DNA synthesis in the individual chromosomes and in the various parts of them. In particular, it has been possible to make a comparison between the regions that, on the basis of their staining properties, had been called euchromatic and heterochromatic.

In this monograph, Giannelli has summed up all the discoveries that have been made relative to DNA synthesis in human chromosomes, setting them against the background of a general survey of the mitotic process.

The autoradiographic method, by showing up different rates of duplication characteristic of the various regions of the different chromosomes, enables these chromosomes to be identified (which, in the human karyotype, with 23 pairs of chromosomes, presents considerable difficulties). It provides important comparisons with regard to chromosome anomalies, which have assumed a great importance in the study of various diseases. It has also made an essential contribution to Mary Lyon's theory of the inactivation of one of the two X chromosomes in the cells having a female chromosome set (XX).

Further important results may be expected from this technique, with regard to the morphology and physiology of the chromosomes. Hence, this accurate and clear synthetical survey may have an important function in stimulating and guiding further studies.

Roma, Università, Istituto di Genetica.

G. Montalenti

T. S. Hall, *Ideas of Life and Matter* (Studies in the History of General Physiology, 600 B.C. - 1900 A.D.), 2 vols., The University of Chicago Press, London, 1969, xii+420 and viii+400 pp., tables, £9.00.

The old problem: what life is, how it can be defined, and what are the conditions that permit the clustering of matter to form living organisms, has cropped up periodically during the centuries in which philosophical and biological ideas have been developed. The solutions that have been offered can be grouped into two broad categories, namely mechanicism and vitalism. With the establishment of experimental biology, starting from the XVII century, the various mechanicistic or vitalistic interpretations seek their basis in experimental data.

The author gives a fairly full account of the main theories that were formulated in classical antiquity and later from the Renaissance up to the end of the XIX century. The problem is still open today, and modern biology has stated it on a molecular and evolutionary basis, arriving at solutions that certainly represent a considerable advance compared with those of the last century. The author deliberately avoids dwelling on these new ideas, apart from making a fairly general mention of them, which does not allow him to penetrate into the heart of the present-day problems.

In general, it may be said that over the centuries there has been a succession of ever closer approximations to the mechanicistic solution, as investigation has proceeded and has led into the field of the physical interpretations of phenomena that at first seemed irreducible.

It may also be said that the major achievements of biology - from physiology to experimental embryology and genetics and finally to molecular biology - have always been the result of methods of research governed by mechanicistic principles.

As the author points out, this survey is not complete (it may be noted, for example, that no mention is made of the work of H. Driesch, the great champion of neo-vitalism at the end of the XIX century). He considers it as a preliminary report and proposes to explore more fully some fields that are only briefly mentioned here, and to analyse them in greater detail. As it stands, it is a useful volume of consultation, even if it cannot be considered a synthetical work.

The subject matter is divided into several sections, devoted to: classical antiquity, the Renaissance (*Toward a new biology*), the XVII century (*Life and air*); animism and mechanicism in the XVIII century; vitalism and mechanicism after the discovery of the cell. The last section is devoted to the problem of life and matter after 1860, and a final chapter gives some very hasty and incomplete information on the position of modern biology.

The bibliographical references of the basic texts are given in notes at the foot of the page. At the end of the second volume there is a list of the secondary texts: histories of the sciences, biographical and bibliographical sources.

Roma, Università, Istituto di Genetica.

G. Montalenti

G. Vandebroek, *Evolution des vertébrés (De leur origine à l'homme)*, Masson & C.ie, Paris, 1969, xx+584 pp., figs., 120 F.

The task that the author has put before himself is to describe the evolutionary lineage starting from the lower Chordates, ending at Man. This is not a treatise insofar as it does not cover sistematically all the aspects of the line of descent, nor all the facets of the problem. It is the summary of the ideas of the author and of a number of factual achievements that paleontology owes to him. Some of the original contributions are published here for the first time.

The first chapter illustrates, mainly on embryological ground, various hypotheses on the origine of higher Metazoa. The author supports the so called mesentheric hypothesis, according to which the Protostomata derive from Cerianthidae, which have transformed their blastoporal side into a ventral side. The Chordates would derive from the same original stock.

The second chapter deals with some general aspects of Chordate evolution, studied mainly on the basis of tegumentary, excretory and circulatory systems. The following chapters are devoted to such topics as: the evolution of lower Chordates and the origin of Tetrapods; the origin and evolution of Mammals up to the higher Primates. As an interlude, chapter VII deals with the intraspecific variation and the genetical mechanism of evolution; this, being by necessity rather short, is perhaps superfluous in this context, because the topic is convenientely developed in so many modern books.

The last two chapters, VIII and IX are devoted to the evolution of Prehominidae into Hominidae, and to a brief description of human races and anthropological types.

The main topic of this book, namely the mammalian evolution, is dealt with particularly on the basis of the study of teeth, to which the author has contributed with original work.

This book is well produced and abundantly illustrated.

* * *

E. H. Colbert, *Evolution of the Vertebrates (A History of Backboned Animals Through Time)*, 2nd ed., John Wiley & Sons Ltd., London, 1969, xviii + 536 pp., figs., £6.25.

Colbert's book, on the contrary, is a real textbook on vertebrate paleontology, in which there is set forth a general review of the evolution of the animals with backbones, from Amphioxus to Man, as based on fossil records. The author tries to avoid as much as possible highly technical terms in order to make the book understandable to general students and to lay readers. In the present edition, issued fourteen years after the first, new acquisitions in the field of vertebrate paleontology have been taken into account as far as possible. In the last chapter a very useful table is included showing the correlation of coenozoic vertebrate bearing sediments in the different part of the world. A selected list of references is appended, in which the most recent books on evolution, vertebrate zoology, and vertebrate paleontology are indicated. The book is well produced; it is a good textbook for students in biology and in geology.

Roma, Università, Istituto di Genetica.

G. Montalenti

A. Adams, *The Eternal Quest* (The Story of the Great Naturalists), Constable and Company Ltd., London, 1970, 510 pp., £3.50.

This volume is intended for a wide public rather than for specialists. Each chapter is devoted to a naturalist, the events of whose life are fully related. The historico-biographical aspect definitely prevails over an analysis of their scientific work. The naturalists concerned are: Linnaeus, Buffon, Lamarck, Cuvier, Alexander Wilson (an American ornithologist), J. J. Audubon (a Franco-American ornithologist), von Humboldt, Lyell, Agassiz, Darwin, Wallace, Huxley, Mendel. As to the criteria adopted in making his choice, the author states: « My goal was to write man's discovery of his place in the natural world (in so far as he has discovered it) and to show the qualities and characters of some of the men who did the discovering. Bearing this in mind, I set up certain standards for the selection of the central figures in each chapter: Was the man's life interesting in itself? Did it reveal anything significant about the various ways that naturalists have performed their work? Were their results important? In this fashion I tried to strike a balance, but even so, the choices were not easy: too many well qualified candidates deserved admission ».

In fact, the choice appears somewhat questionable; above all, the omission of certain great biologists such as Redi, Spallanzani and Pasteur would seem unjustified.

But in spite of these limitations, the volume, which makes pleasant reading and is provided with a good bibliography, may prove useful for informing the general reader about certain aspects of the history of biology and certain great theoretical and practical problems that confront mankind today.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

H. EIDMANN and F. KÜHLHORN, *Lehrbuch der Entomologie*, 2nd ed., Verlag Paul Parey, Hamburg, 1970, 634 pp., figs., DM 68.

The world of insects is so vast and multiform (over 700,000 species being known, grouped in no less than 30 highly differentiated orders) that it would seem hopeless to assemble the fundamental ideas of entomology in a single volume.

In the first edition (1941) of this volume, intended for university students, Prof. Eidmann managed to include a considerable number of facts, presented organically in the form of a summary. In this second edition, edited by Dr Kühlhorn, account has been taken of the discoveries of the last thirty years. The very extensive material is subdivided into 9 chapters, covering the following subjects: position of the insects in the animals system; the habitus, dimensions and number of insects; external morphology and anatomy, with notes on physiology; reproduction; development; ecology; and systematics. The bibliographical references are reduced to a few treatises (there is no mention of the classical Italian works by Berlese, Silvestri and Grandi). The illustrations are diagrammatic and are mostly clear.

The volume well fulfils the intention of the authors: to provide an introduction to entomology for the use of students of zoology, agriculture and forestal sciences.

Roma, Università, Istituto di Genetica.

G. MONTALENTI

F. M. SALZANO and N. FREIRE-MAIA, *Problems in Human Biology* (A Study of Brazilian Populations), Wayne State University Press, Detroit, 1970, 200 pp., figs. and tables, $8.95.

The population of Brazil, made up of a mixture of numerous families originating not only from South America itself (Indios) but also from the most diverse localities of the Euro-Asiatic and African continents, offers extremely interesting material for the study of various demographic, social and genetic problems. The authors, who are also responsible for important genetic studies, have summarised in this publication the main historical, demographic and genetic data, as well as certain anthropological information on the population of Brazil. Of particular interest to the geneticist are the data on consanguineous marriages, inbreeding among isolated groups and the genetic flow between them, the «genetic load», the analysis of genetic polymorphisms, malformations, chromosome aberrations and hereditary diseases.

The volume is mainly intended for non-specialists, so that the various concepts and methods of studying the main problems are explained in terms accessible to the layman, with the aid of a glossary and of suitable bibliographical references.

Roma, Università, Istituto di Genetica.

G. Montalenti

H. Harris and K. Hirschhorn (eds.), *Advances in Human Genetics*, Vol. I, Plenum Press, New York, 1970, xii+340 pp., figs. and tables, $19.50.

The enormous number of specific papers that are published every year, and the continuous increase in periodicals on biology, makes it practically impossible to keep pace with the literature, even in a very limited sector. Hence the necessity for synthetical reviews that summarise the main results obtained in the various subjects. This necessity is met by such periodicals as are entitled « *Advances in ......* », which are becoming increasingly numerous. The present volume is the first of a new series devoted to Human Genetics, a discipline that has shown exuberant development in recent years, which is expected to continue in the near future. The editors of these *Advances* are H. Harris of London, to whom are due some of the most significant developments in human biochemical genetics, and K. Hirschhorn of New York, who is a an authority in the study of chromosome anomalies in man: two of the most important and most promising aspects of human genetics.

The subjects covered by this first volume are all of great current interest: Analysis of the data of genealogical trees (J. H. Edwards); Autoradiography in human cytogenetics (O. J. Miller); The genetics of the immunoglobulins (H. H. Fudenberg and N. E. Warner); The human genetics of membrane transport, with particular regard to the amino-acids (Ch. R. Scriver and P. Hechtman); Genetic disorders of intestinal digestion and absorption (J. Frézal and J. Rey). The subjects announced for the later volumes are also among the most interesting and topical.

In short, this enterprise arouses the highest expectations and will undoubtedly prove very useful to general geneticists as well as to specialists in human genetics.

Roma, Università, Istituto di Genetica.

G. Montalenti

B. Rossi and S. Olbert, *Introduction to the Physics of Space*, McGraw-Hill Book Co., London, 1970, xiv+454 pp., figs. and tables, £9.35.

The work of Bruno Rossi as a scientist and writer is so well known to all that he needs no introduction in these respects. This book is an addition to the others that Rossi has written on various branches of physics in a remarkable sequence of scientific interests, following a truly exceptional logical and didactic development. The combined clarity and succinctness of the exposition, the completeness of the treatment, the recurrent appeal to intuition, and the regular recourse to the general principles of physics (without bringing in any undesirable or laborious mathematical formalisms) are the outstanding features of this notable work, which, like Rossi's other books, is bound to become a classic of its kind.

The book arose from notes of lectures given by the authors at M.I.T. over several academic years, which have already been used for some time in both teaching and research. It will therefore be useful to students and also to research workers who are not specialists in the field, or to a space physicist seeking to increase his knowledge of the underlying principles of space physics.

There are fourteen chapters, and five appendices dealing with supplementary matters such as the application of the nabla operator and special relativity. The authors' introduction is followed by an account of the behaviour of charged particles in electric and magnetic fields, leading to a discusslon of geomagnetic effects on cosmic rays. The book then proceeds to a study of plasma physics (motion of the guiding centre, adiabatic invariants, dynamics of magnetically trapped particles) and of fundamental plasma-particle interactions, so as to arrive at an adequate view of the physical behaviour of plasma in space. The final chapter deals briefly with physical knowledge of the solar corona and the interplanetary plasma.

There are few bibliographic references, which confirms the largely didactic intention of the authors.

Napoli, Osservatorio Astronomico di Capodimonte.

R. Falciani

R. S. White, *Space Physics*, Gordon & Breach, London, 1970, xiv+318 pp., figs. and tables, £5.25.

In a field so quickly developing as space physics, it is indeed difficult to write a book that fully illustrates every aspect and trend. A choice has therefore to be made, a few topics being treated thoroughly, or else a larger number more generally. The author of this book has chosen the latter, so as to give his readers the broadest possible picture of space physics. The six chapters deal with the earth's radiation belts, the atmosphere and the ionosphere, the sun, interplanetary space, and the magnetosphere, thus illustrating the aspects of space physics that are most closely related to the earth. The treatment is deliberately kept simple, with the chief aim of describing present knowledge and unresolved problems in the various branches of research, rather than going into explanations and theoretical justifications.

The book may be of use to all who, while not already concerned with the branches of research discussed, are seeking information about them. There is a long and up-to-date list of references at the end of each chapter, forming a very valuable source of information and guidance for anyone wishing to study further the matters dealt with in the book.

Napoli, Osservatorio Astronomico di Capodimonte.

R. FALCIANI

V. P. SHCHEGLOV, *Jan Hevelius - The Star Atlas*, « Fan » Press, Uzbek S.S.R., Tashkent, 1970, 36 pp., plates, no price stated.

This republication of the famous atlas of the Polish astronomer Jan Hevelius (1611-1687) resulted from celebrations held in the Uzbek republic of the U.S.S.R. to commemorate the foundation of the city of Samarkand 2500 years ago.

In the fifteenth century, Samarkand was a centre of astronomical studies, in particular those of Ulugh Beg (1394-1449), grandson of Tamerlane the Great, who built a large observatory on the Tashkent road near Samarkand. The existence of this observatory and some of the results of observations made there have long been known, but only in the present century have Russian archaeologists and astronomers made further series of investigations which in 1948 led to the complete discovery of Ulugh Beg's observatory.

Under the auspices of the Uzbek Academy of Sciences, Professor Shcheglov, the director of the Tashkent Observatory, has now produced an attractive new edition of Hevelius' atlas, in connection with these celebrations. In an introduction he tells how its history is connected with the work of Ulugh Beg. The basic instrument was a arc of circle made entirely of marble, some 40 metres in radius and 60 degrees in extent, whence the name of « sextant » given to the instrument, carved out of pure roch and aligned on the local meridian. The observer probably used a suitable alidade to determine the meridian passage times and altitudes on the horizon of the various celestial bodies. In particular, systematic observations of the Sun gave the dates of the equinoxes and solstices and the inclination of the ecliptic. The constant of precession, the length of the tropical year, and the positions of the brighter stars were also determined.

In this way, Ulugh Beg was able to compile a catalogue of 1018 stars which was much more accurate than his predecessors had achieved. This great work is a splendid memorial of the most brilliant period of Eastern astronomy.

Ulugh Beg's observations and results were several times republished in the West, and were included in Hevelius' *Prodromus astronomiae* (Gdańsk, 1960). In this book and in the companion star atlas are some noteworthy engravings, including one of Ulugh Beg himself. Another shows six astronomers whose star catalogues contributes to Hevelius' book, seated at a circular table with Urania, the muse of astronomy. Ulugh Beg is at her right hand, an indication of Hevelius' high regard for the Samarkand astronomer.

Shcheglov's introduction is followed by an exact reproduction of the 56 plates of the star atlas. At the centre of each is an artistic and original representation of a constellation figure, surrounded by the outlines of neighbouring constellations. The value of this edition with regard to the history of astronomy need not be stressed.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

Z. Kopal, *Widening Horizons* (Man's Quest to Understand the Structure of the Universe), Stanmore Press, London, 1970, 176 pp., figs., £1.50.

This book by Zdenek Kopal, the well-known astronomer and professor at the University of Manchester, might even have been called « Unbounded Horizons ». It gives a brief account of the history of astronomy, from antiquity through the falloff in the Middle Ages and on to its renaissance achieved by the founders of modern astronomy. This part occupies the first 92 pages. Then the author takes the problem of determining stellar distances as a basis for discussing the gradual expansion of known space towards the unattainable depths of the universe, towards those unbounded horizons which even the powerful research tools of mankind cannot survey. The book ends with a short glossary of astronomical terms. It is written in a simple and clear style readable even by amateurs.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Abetti

The Growth Points of Physics (Proceedings of the Florence Inaugural Conference, Florence, Italy, 8-12 April 1969), European Physical Society, Carouge-Geneva (reprinted from *Nuovo Cimento*, Bologna), 1970, xl+568 pp., figs. and tables, no price stated.

The possibility of establishing a European Physical Society was discussed in April 1966 by an international group of physicists who met in Pisa under the chairmanship of Prof. Gilberto Bernardini, rector of the Scuola Normale Superiore there. It was immediately clear that there was general support for the setting up of such a society, which would be bound to make a considerable contribution to European cultural unity, on account of the important role of physics in the development of modern society.

A working group was formed, later becoming a steering committee, to put into effect the ideas discussed in Pisa. They met, together with delegates from the national physical societies, at Geneva in 1966, London in 1967, Geneva and Prague in 1968. Finally, at a further meeting in Geneva on 26 September 1968, the European Physical Society was officially established, comprising at that time eighteen national physical societies and academies, and some sixty eminent physicists.

The inaugural conference on the Society was held at Florence on 8-12 April 1969, jointly with the International Union of Pure and Applied Physics. The conference was very suitably devoted to a series of review papers collected in this book, dealing with aspects of physics which are at present rapidly developing. There are 29 papers by international authorities, which fall into the five basic categories of modern physics: (1) astronomy, astrophysics, cosmology and relativity; (2) nuclear and elementary-particle physics; (3) solid-state physics; (4) atomic, molecular and plasma physics; (5) quantum optics and electronics. These reviews are preceded by two historical papers (by Weisskopf and Blackett) and two papers on scientific policy (by Amaldi and Casimir).

The book is to a large extent understandable even by non-specialists, and forms a milestone in the history of physics.

Catania, Osservatorio Astrofisico.

G. Godoli

J.-C. Pecker, *Experimental Astronomy* (Astrophysics and Space Science Library, Vol. 18), D. Reidel Publishing Company, Dordrecht, 1970, x+106 pp., figs. and tables, Dfl. 28.

The title of this book is not meant to refer to practical astronomy, but to stress the experimental nature of astronomical research in contrast to astronomy regarded as a purely observational science. According to Pecker, no scientific observation is ever entirely passive, for it involves the choice of the observed object and of the instruments and methods used. The astronomer who calculates a sequence of star models for comparison with observations is doing what Pecker calls « numerical experiments ». True experiments have been performed by launching artificial meteorites from rockets, and the rockets and artificial satellites themselves constitute a type of experiment in which the atmosphere does not affect the observations.

The book, is, in addition, deeply humanistic. In his preface, the author sadly reflects on the serene and fertile reflection that was possible in the observatories of days gone by, but is now destroyed in today's crowded establishments where computers swamp research workers with floods of data. Pecker's intention is to demonstrate to young astronomers that this state of affairs, though excessive, does represent progress if the results are properly used, which often is not the case.

The book opens with an extensive group of citations from Comte, whose positivism represents (like all philosophical systems) a more or less accurate summary of knowledge at a certain time, but is rightly criticized by Pecker, in particular as regards Comte's view of astronomy.

Next come a brief introduction to celestial mechanics, the fundamentals of astronautics, a summary of the chief results of space research, and a chapter on the plurality of inhabited worlds, which begins with the dialogue between Fontenelle and his Marquise, but continues with an accurate account that is perhaps the most interesting part of the work (Van de Kamp's planetary systems, the Ozma project, etc.), and ends by deploring the readiness with which sometimes disingenuous and dishonest boastings are imposed on the world at large.

The author's conclusion deals with future methods of astronomical research and the more than exponentially increasing number of astronomers. The translation from the French, by Robert Kandel, is excellent.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai

H. J. Habing (ed.), *Interstellar Gas Dynamics* (Proceedings of the Sixth Symposium on Cosmical Gas Dynamics organized jointly by the International Astronomical Union and the International Union of Theoretical and Applied Mechanics, Yalta, Crimea, U.S.S.R., 8-18 September 1969), D. Reidel Publishing Co., Dordrecht, 1970, xxiv+368 pp., figs. and tables, Dfl. 75.

This book is publication no. 39 of the International Astronomical Union. As the title indicates, it deals only with interstellar gas, thus excluding stellar atmospheres and interiors, intergalactic matter, interplanetary gas, and the spiral structure of the galaxies. But many of the topics excluded, which have been the subject of other symposia, appear also from time to time in the Yalta symposium, because of the close connection between all the topics pertaining to cosmology.

One purpose of the symposium was to establish contact between astronomers and mathematical physicists. Many of the latter were young workers seeking new subjects of research while at the same time being unfamiliar with the language of astronomy; unfortunately, a number of older theorists who had attended previous meetings were absent, and one may also criticize the attitude of some theorists who were more interested in promoting their own ideas than in an impartial study of the physical problem.

A great merit of this meeting was the elimination of short papers, which often (if not always) involve a profitless waste of valuable time. Instead, sound and substantial accounts of the position regarding specific topics were followed by long discussions to establish in particular the future lines and programmes of research. Despite the large number of participants (140), this approach was successful, and the discussions were recorded and form perhaps the most important part of the book.

According to a suggestion by Burgers and Oort, the clouds should be considered not as isolated objects but as fluctuations in the interstellar medium, which is to be regarded as a continuum of atoms, ions, grains, cosmic rays, light and molecules (the last of which have been under investigation very recently), with the magnetic field as an essential constituent. The book is divided into two parts: (I) Description and General Theory of the Interstellar Medium, (II) Interaction of Stars and Interstellar Medium.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai

A. Dollfus (ed.), *Surfaces and Interiors of Planets and Satellites*, Academic Press, London, 1970, xii+570 pp., figs. and tables, £9.00.

In recent years our knowledge of solar-system dynamics and planetary physics has been greatly increased by advances in astronomical techniques (whether optical or radio and radar) and by space research programmes. Many constants have been determined more accurately, and some new and unexpected phenomena have been discovered, for example that the rotational and orbital periods of Mercury are not equal, as had been supposed, and that the surface temperature of Venus is considerably higher than the previously accepted value.

It cannot have been easy or simple to collect all this material from a vast number of papers in many journals, and a valuable piece of work has been done by Audouin Dollfus and his nine colleagues in bringing together as a single book so many results, and moreover comparing them with those of « classical » measurements.

The book is intended for a wide range of research workers: in planetary astronomy (including those wishing to take up the subject) and in neighbouring fields such as geophysics, geodesy and physics. It comprises ten articles, six in English and four in French. The topics dealt with are the measurement of masses, diameters, thermal emission spectra, surface photometry, internal constitution, and magnetism of members of the solar system, with one chapter on possible Martian biology. The chapter on radar studies of the planets is especially interesting.

Firenze, Osservatorio Astrofisico di Arcetri.

G. Calamai

P. J. Ucko and G. W. Dimbleby (eds.), *The Domestication and Exploitation of Plants and Animals*, Gerald Duckworth & Co. Ltd., London, 1969, xxvi+582 pp., figs. and tables, £7.35.

This big volume contains the reports of a seminar on archaeology and related sciences, held in London in May 1968, which was attended by archaeologists, anthropologists and biologists. The subject - namely, how and when man has domesticated animals and plants - is important in itself, but is even more so if viewed in relation to the complex problem of the relationships between man and his environment. The subject is decidedly an interdisciplinary one and it is obvious that only an approach of such a kind can lead to a decisive advance in our knowledge, including those notions that might be considered strictly archaeological. In fact, as Stuart Piggott states in the conclusions, a large part of the work has been, in a certain sense, a preparation of material for a definition of the Neolithic Period or a doubting of its very exitence.

The numerous contributions may be grouped under five headings: 1) origins of domestication; 2) methodological problems; 3) regional studies; 4) studies on particular systematic groups; 5) human nutrition. Of obvious general interest are the contributions of a methodological nature, but perhaps even more so are those on the origin of agriculture considered in an ecological context. In this respect, mention may be made of the article by David R.

Harris, in which the analytical methods used for the natural ecosystems are applied to the « agricultural systems ». On the basis of this analysis, for example, Harris maintains that initially man must have confined himself to the transformation of non-specialised ecosystems, in which the great variety of spontaneous species probably encouraged the use of numerous species of plants and animals and perhaps stimulated an experimental activity of domestication. These ecosystems are probably to be sought on the fringes of non-specialised ecosystems, such as forests, in which lived populations given over to hunting, fishing and harvesting, though in a non-specialised manner. At this point, however, it is necessary to distinguish between an agriculture based on seed production and one based on the production of tubers and rhizomes. Space does not allow mention of other articles: this was only one example of the type of help that the naturalist can give the archaeologist in those cases in which, at least at present, the findings of excavations are almost non-existent.

Siena, Università, Istituto di Botanica.

G. Sarfatti

P. Bec, *Manuel pratique de philologie romane*, Vol. I: *Italien, Espagnol, Portugais, Occitan, Catalan, Gascon*, (Series: *Connaissance des Langues*, No. 5), Editions A. & J. Picard, Paris, 1970, x+558 pp., maps, 68 F.

In line with the purposes and intentions of the series in which it appears (the *manuels pratiques* of the *Connaissance des Langues* series, directed by Henry Hierche), the present *Manuel pratique de philologie romane* by the Provençal specialist Pierre Bec aims at providing the reader, especially the university student, with a general knowledge of Romance linguistics and philology by means of a survey of passages taken from representative works written in the various Romance languages. The first volume contains the sections dealing with Italo-Romance (which, because of the extremely conservative nature of its main variety, Tuscan, is also used to give some idea of vulgar Latin and 'proto-Romance'), Ibero-Romance, with the sub-groups, Spanish and Portuguese, Occitano-Romance, with the sub-groups, Occitanic, Catalan and Gascon. The second volume (to be published) will be devoted to French and Rumanian. The annotation of the various texts (a *laude* by Jacopone da Todi for the Italian, a passage of the *Cid* for Spanish, a song by Bernart de Ventadorn for Old Provencal, etc.) is complete and accurate. The ample cross-references and frequent digressions into general linguistic phenomena allow the reader to reconstruct a broad panorama of the most important problems of Romance philology. This reconstruction will be further facilitated when the second volume is published, as it is to contain an index of forms, divided up according to language, as well as an index of all the phenomena dealt with in the commentary. At the end of the volume there are interesting geolinguistic maps showing the present situation in Romania, the distribution of dialects in Italy, etc.

Firenze, Università.

G. Ghinassi

D. E. Reichle (ed.), *Analysis of Temperate Forest Ecosystems* (Series: *Ecological Studies*, Vol. 1), Springer-Verlag, Berlin, 1970, xii+304 pp., figs. and tables, DM 52.

The volume edited by David E. Reichle is the first of a series aimed at providing informations on the methods of studying the ecological systems and at giving informations of the results obtained. The series originated as a result of the International Biological Programme, and this first volume relates to works having a primarily methodological purpose, presented at a seminar of the Terrestrial Productivity section of the IBP in the United States.

The purpose of the seminar was to make an examination of the existing information and to establish a conceptual basis for the analysis of a forest ecosystem. The subject matter is divided according to eight headings, covering the analysis of the ecosystem in general, primary producers, consumer organisms, decomposer organisms, the cycle of the elements and the water cycle.

Consistent with the purpose of the seminar, there is, in most of the works (18 in all), a tendency to summarise the knowledge acquired on a certain problem and to approach the methods of study critically. In this respect the volume constitutes an extremely interesting aid for anyone working in the field of ecological research in the strict sense or for the control of the forest ecosystem.

The initial works, assembled under the title « Analysis of an ecosystem », give a good survey of the possible lines of research by examining the various possibilities of qualitative and quantitative approach to the study of ecosystems. In the individual works covered by the other titles, the authors have endeavoured not so much to adopt a general approach as to define actual systems and develop models simulating actual systems, which will enable new working hypotheses to be deduced for formulating the subsequent research.

Firenze, Università, Facoltà di Agraria.

G. Sanesi

O. Kinne (ed.), *Marine Ecology* (A Comprehensive Integrated Treatise on Life in Oceans and Coastal Waters), Volume I: *Environmental Factors*, Part 1, John Wiley & Sons, Ltd., London, 1970, xvi+682 pp., figs. and tables, £12.50.

> « Alles ist aus dem Wasser entsprungen!!
> Alles wird durch das Wasser erhalten!»
> (Goethe: Faust)

With these words C. L. Hubbs (Copeia) expressed, in 1935, an old but ever more relevant concept: « Science's only hope of escaping a Tower of Babel calamity is the preparation from time to time of works which summarise and which popularise the endless series of disconnected technical contributions ». One of the most useful works of this type will certainly be this Treatise of Marine Ecology, of which part 1 of the first volume has now appeared.

Of the three chapters that make up the volume, the first, by A. W. Collier, « considers ocean and coastal waters as life-supporting environments ..... describes briefly the ocean basins, their principal water masses and circulation, the sea-land boundary, the property of sea water and the chemical cycles in the seas ».

The second and third chapters, after a general introduction by N. G. Jerlov and O. Kinne respectively, are devoted to the functional and structural responses to light and temperature of bacteria, fungi and blue-green algae (by W. Gunkel and C. H. Oppenheimer respectively), of plants (by J. A. Hellebust and F. Gessner resp.), of invertebrates (by E. Segal and O. Kinne resp.) and of fishes (by J. H. S. Blaxter and J. R. Brett & E. T. Garside resp.).

The volume concludes with author-, taxonomic- and subject-index.

The whole work is to include 5 volumes, each of which may be divided into several parts: vol. I - environmental factors (part 1: see above; part 2: salinity, water movement, turbidity; part 3: substratum, pressure, dissolved gases, organic substances, ionizing radiation, factor combinations); vol. II - physiological mechanisms; vol. III - cultivation; vol. IV - dynamics; vol. V - ocean management.

Firenze, Università, Museo Zoologico.

B. Lanza

E. Laszlo (ed.), *Human Values and Natural Science* (Proceedings of the Third Conference on Value Inquiry, State University of New York, College at Geneseo), Gordon and Breach Science Publishers, London, xiv+292 pp., £6.25.

There is much food for thought in this book; it contains the proceedings of the third of an annual series of interdisciplinary conferences on the problems related to human and ethical values: that is, about one of the most intriguing and dramatic issues of to day's social and philosophical scenes. The complexity and extent of the subject, together with the emotional climate in which it is embedded, may perhaps excuse the vagueness and inconclusiveness of some papers and, in general, a lack of strictness, notwithstanding the elaborate organization of the symposium.

Nevertheless, some contributions are not only interesting but also original; for instance, the one by Margenau and Oscanyan on « A scientific approach to the theory of values », the review by J. O. Wisdom on « Why the gap is under attack », the scientifically up-to-date « Determinism, responsibility and the person » by N. W. MacGill. In addition, there is the warm and fascinating manifesto by L. L. Whyte, who invites responsible men to set up an authentic human order, with an appeal to the fundamental ordering activity of the human mind.

Roma.

A. Serrai

LIBRI RICEVUTI - BOOKS RECEIVED

Le opere elencate nella rubrica "Libri ricevuti,, non vengono raggruppate per materie. Le varie discipline scientifiche si sono andate intersecando e integrando al punto che ogni tentativo di classificazione, privo di attendibilità e di rigore, sarebbe puro atto d'arbitrio. Si è preferito, invece, seguire nella compilazione della rubrica l'ordine alfabetico, tenendo conto del cognome degli autori delle opere. Per comodità dei lettori elenchiamo qui sotto le opere che vengono segnalate nelle pagine seguenti.

The works listed in the section "Books received,, are not grouped by subjects. The various scientific disciplines now overlap and integrate one another to such an extent that any attempt at classification would be unreliable and inexact and thus purely arbitrary. We have preferred to follow alphabetical order in compiling this section, according to the surname of the authors of the works listed. For the convenience of readers we list below the works mentioned on the following pages.

ADAM J., SCHARF J.-H. and ENKE H. - Methoden der statistischen Analyse in Medizin und Biologie.
ADNÈS A. - Adenès, dernier grand trouvère.
AGENO M. - La costruzione operativa della fisica.
AGENO M. - L'origine della vita sulla Terra.
ANAND N., BINDRA J. S. and RANGANATHAN S. - Art in Organic Synthesis.
ARONSON L. R., TOBACH E., LEHRMAN D. S. and ROSENBLATT J. S. (eds.) - Development and Evolution of Behavior.
ASIMOV I. - La vita e l'energia.
ASTIER J.-L. - Géophysique appliquée à l'hydrogéologie.
Atlante geografico generale Zanichelli.
BALTAY Ch. and ROSENFELD A. H. (eds.) - Experimental Meson Spectroscopy.
BARBIERI G. and ALMAGIÀ R. - L'Italia.
BASS J. - Cours de mathématiques.
BASS J. - Mathématiques.
BATHURST R. G. C. - Carbonate Sediments and their Diagenesis.
BATSCHELET E. - Introduction to Mathematics for Life Scientists.
BAYET J. - Croyances et rites dans la Rome antique.

Beadle G. W. and Beadle M. - Le langage de la vie.
Beaujeu-Garnier J. - La Géographie.
Bec P. - Manuel pratique de philologie romane.
Beers R. F. Jr. and Braun W. (eds.) - Biological Effects of Polynucleotides.
Bertolini B. - Lo sviluppo di una rana.
Biass-Ducroux F. (ed.) - Glossary of Genetics.
Boener H. - Representations of Groups.
Born M. - Structure atomique de la matière.
Botré C. - Potenziali di membrana e fenomeni bioelettrici.
Broecker W. S. and Oversby V. M. - Chemical Equilibria in the Earth.
Bronwell A. B. (ed.) - Science and Technology in the World of the Future.
Bunge M. (ed.) - Problems in the Foundations of Physics.
Butery A. - Mathématiques pour les sciences sociales.
Buzon J. et coll. - Structure et propriétés moléculaires.
Cagnet M., Françon M. and Mallick S. - Atlas optischer Erscheinungen.
Carnap R. - I fondamenti filosofici della fisica.
Carrier R. - Biochimie médicale.
Christophorou L. G. - Atomic and Molecular Radiation Physics.
Chvidchenko I. (ed.) - Large Space Programs Management.
Circuits à semiconducteurs dans l'industrie.
Côme D. - Les obstacles à la germination.
Cookes H. St. L. - The Eyes, Brain and Nerve System in Relation to the Earth's Magnetism.
Cutter E. G. - Plant Anatomy: Experiment and Interpretation.
Davies G. L. - The Earth in Decay.
Dubach U. C. and Schmidt U. (eds.) - Recent Advances in Quantitative Histo- and Cytochemistry.
Dumitriu A. - Theory and System.
Ebel F. and Birnbaum O. - Schöne und seltsame Welt der Orchideen.
Energlyn and Brealey - Analytical Geochemistry.
Erixon S. † (ed.) - The Possibilities of Charting Modern Life.
Escalle J.-E. - Après l'infarctus.
Faurre P. - Navigation inertielle optimale et filtrage statistique.
Finney D. J. - Probit Analysis.
Flügge S. - Practical Quantum Mechanics.
Forsdyke A. G. - Meteorological Factors in Air Pollution.
Franconi C. (ed.) - Magnetic Resonances in Biological Research.
Gardner M. - Nouveaux divertissements mathématiques.
Giese A. C. - Fisiologia della cellula.
Gorbatskii V. G. - Exploding Stars and Galaxies.
Grassé P.-P. (ed.) - Traité de Zoologie. Anatomie, Systématique, Biologie.
Grouchko D. - Operations Research and Reliability.
Grundmann Ch. and Grünanger P. - The Nitrile Oxides.
Grüsser O.-J. and Klinke R. (eds.) - Pattern Recognition in Biological and Technical Systems.
Gurzadyan G. A. - Planetary Nebulae.
Handscombe E. - Electrical Measuring Instruments.

HARDIN G. - Birth Control.
HARRÉ R. - The Method of Science.
Inquinamenti dell'aria, dell'acqua e del suolo.
JAGER K. W. - Aldrin, Dieldrin, Endrin and Telodrin.
JAMMER M. - Storia del concetto di forza.
Journal de l'année.
KENNEDY D. M. - Birth Control in America.
KING L. S. - The Road to Medical Enlightenment 1650-1695.
LAMOTTE M. and BOULIÈRE F. (eds.) - Problèmes d'écologie: L'échantillonnage des peuplements animaux des milieux aquatiques.
LARGEAULT J. - Enquête sur le nominalisme.
LEGRAND G. - Algèbre linéaire et multilinéaire et géométrie différentielle.
LEIPP E. - Acoustique et musique.
LOMBARD J. - Psychologie énergétique et typologie fonctionnelle.
LOWE P. G. - Classical Theory of Structures.
LUTZ H. (ed.) - Invertebrate Organ Cultures.
MARTINEZ E. - Le tredici stelle.
MASON B. (ed.) - Handbook of Elemental Abundances in Meteorites.
MAXWELL A. E. (ed.) - The Sea.
MCGONNAGLE W. J. - Nondestructive Testing.
MCGREEVEY W. P. - An Economic History of Colombia 1845-1930.
MEINCK F., STOOFF H. and KOHLSCHÜTTER H. - Les eaux résiduaires industrielles.
MOISIL GR. C. (ed.) - Logique, automatique, informatique.
MORPURGO G. - Il mondo della cellula.
MORRIS N. M. - La commande automatique dans l'industrie.
NELSON A. F. - The Development of Lester Ward's World View.
NEVEU J. - Bases mathématiques du calcul des probabilités.
PACHOLCZYK A. G. - Radio Astrophysics.
PIATIER A., CAHUZAC P. and CHAMBADAL L. - Economie et mathématiques.
PICARD H. and ANTONINI A. - Traitement médical étiologique de la coxarthrose,
POHL R. W. - Trattato di Fisica.
POUQUET J. - Les sciences de la Terre.
Radiation Including Satellite Techniques.
RECHCIGL M. JR. (ed.) - Enzyme Synthesis and Degradation in Mammalian Systems.
ROEDERER J. G. and ZÄHRINGER J. (eds.) - Physics and Chemistry in Space.
ROLLIN P. - Phytochrome, photomorphogenèse et photopériodisme.
ROUBINE E. - Introduction à la théorie de la communication.
Satellite and Computer Applications to Synoptic Meteorology.
SCHMIDT A. - Philosophische Studien zur Populationsgenetik.
SEGRÈ E. - Enrico Fermi, fisico.
TAYLER R. J. - The Stars: Their Structure and Evolution.
TONINI V. - Scienza dell'informazione, cibernetica, epistemologia.
VON FRITZ K. - Grundprobleme der Geschichte der antiken Wissenschaft.
VUCINICH A. - Science in Russian Culture (1861-1917).
ZEL'DOVICH YA. B. and NOVIKOV I. D. - Relativistic Astrophysics.

ADAM, J., SCHARF, J.-H. and ENKE, H., **Methoden der statistischen Analyse in Medizin und Biologie,** Veb Verlag Volk und Gesundheit, Berlin, 1971, VIII+240 pp., figs. and tables, DM 39.

CONTENTS. - *I: Die Lösung linearer Gleichungssysteme,* by H. ENKE. - *II: Varianzanalyse,* by J. ADAM. - *III: Die mehrfache lineare Regression,* by H. ENKE. - *IV: Faktorenanalyse,* by J. ADAM and H. ENKE. - *V: Kurze Einführung in die nichtlineare Regressionsrechnung,* by J.-H. SCHARF.

ADNÈS, A., **Adenès, dernier grand trouvère** (Recherches historiques et anthroponymiques), Editions A. & J. Picard, Paris, 1971, 196 pp., 32 F.

CONTENTS. - Introduction. - *I: Adenès: l'homme et l'oeuvre:* - Biographie d'Adenès. Le poète, le narrateur, sa personnalité. - *II: Adenès: étude du nom:* - Le nom, du vivant d'Adenès; la mésaventure du nom. - *III: Recherches parallèles sur le nom de quatre trouvères:* - Quenes de Béthune, Aymes de Varennes, Philippe Mouskés, Jakemes Makes. - Conclusion: *La tradition des noms propres de personnes au cas-sujet.* - Notes 1°: *Sainte Ada ou Adenète (Adnette):* Au VII^e siècle, au Mans, une abbesse bénédictine. La dévotion à sainte Ada-Adnette. Berte « par la forest dou Mans ». Saint Julien du Mans l'Hospitalier; le saint Julien légendaire. - Note 2°: *La transmission du nom Adenès-Adenet.* - Note 3°: *La persistance du nom des quatre autres trouvères.* - Index.

AGENO, M., **La costruzione operativa della fisica,** Boringhieri Editore, Torino, 1970, 232 pp., figs., L. 3.500.

Questo libro si chiede come sia possibile costruire una scienza e presenta al lettore un primo esempio di come una scienza possa essere costruita prendendo le mosse da definizioni esclusivamente operative. Dopo un esame preliminare della scienza e della sua problematica, l'Autore procede alla costruzione dei concetti fondamentali della fisica: in particolare della meccanica, della termologia e dell'elettrologia. Segue una terza e ultima parte dedicata agli approfondimenti critici. La posizione caratteristica di questa opera è chiedersi che cosa faccia in realtà lo scienziato quando lavora alla costruzione della sua scienza, quanto vi sia di arbitrario in ciò che egli fa e quanto di obbligato dai dati di osservazione e di esperienza. Questo esame si conclude col riconoscimento della natura soggettiva delle leggi fisiche.

AGENO, M., **L'origine della vita sulla Terra** (Un problema esemplare della ricerca scientifica), Zanichelli Editore, Bologna, 1971, VIII+298 pp., figs. and tables, Lire 3.400.

L'autore, un fisico dedicatosi alla ricerca biologica, chiarisce in quest'opera i termini del problema che, oggi come in passato, appassiona un gran numero di studiosi: come ha potuto avviarsi, storicamente, quel complesso di reazioni chimiche altamente specifiche che portano dagli acidi nucleici alla sintesi delle proteine e dalle proteine alla riproduzione degli acidi nucleici e danno all'intero processo vitale il suo carattere tipico di processo chimico divergente? Nel libro vengono anche affrontati i problemi di diversa natura connessi alla scienza ed al suo metodo.

CONTENTS. - *I: Scienza e storia.* - *II: Uno sguardo generale orientativo al mondo della vita.* - *III: Il passato della Terra.* - *IV: Le testimonianze del passato.* - *V: I passi dell'evoluzione chimica.* - *VI: Dalla chimica alla biologia.* - *VII: L'evoluzione biologica.*

ANAND, N., BINDRA, J. S. and RANGANATHAN, S., **Art in Organic Synthesis,** Holden-Day, Inc., San Francisco, 1970, XVI+414 pp., $11.00.

The purpose of this book, is to present, using structural formulae and flow charts, some of the outstanding accomplishments of organic synthesis since 1940. In doing so, it is hoped to illustrate the power and creativity of organic synthesis, and at the same time, to provide an insight into the basic philosophy and strategy employed in design and execution of organic synthesis. Each of the 100 handpicked examples in the book describes the synthesis of a natural product or of an unusual or strained molecule synthe-

sized for checking certain predicted properties of a compound. Each synthesis is preceded by a brief introductory paragraph, highlighting the salient features of the synthesis and its novel aspects. This is followed by a flow chart describing the synthesis along with the reagents and reaction conditions employed for effecting each transformation.

ARONSON, L. R., TOBACH, E., LEHRMAN, D. S. and ROSENBLATT, J. S. (eds.), **Development and Evolution of Behavior** (Essays in Memory of T. C. Schneirla), W. H. Freeman and Company Ltd., Berkshire, 1970, XVIII+656 pp., figs. and tables, £5.60.

The twenty-eight papers written for inclusion in this volume commemorate Dr. Schneirla's protean influence in challenging outmoded ways of thinking about behavior in general and about the development and evolution of behavior in particular. Most are examples of how contemporary investigators are utilizing Schneirla's ideas in theoretical discussions and in experimental and field studies. Written by his former students and associates, many of whom are themselves eminent behavioral scientists, these papers exemplify the far-reaching influence of Dr. Schneirla's seminal theories on controversial issues in behavioral science and the application of his concepts to the most important problems in behavior: instinctive behavior, emotional behavior, evolution of the communicative process and of social behavior, perception, and maternal behavior. There is extensive coverage of the phyletic problems encountered in studying behavior in invertebrates, vertebrates, infrahumans, and humans; both behavioral evolution and behavioral development are discussed in many of the papers.

ASIMOV, I., **La vita e l'energia** (Le basi fisiche e chimiche della biologia moderna), Zanichelli Editore, Bologna, 1970, VIII+360 pp., figs. and tables, L. 3.500.

Non è un problema nuovo quello che pone Asimov ai lettori: il funzionamento degli organismi viventi, la loro capacità di accrescersi e di moltiplicarsi, di adattarsi alle più diverse condizioni ambientali suggeriscono in ogni momento complessi fenomeni energetici. Il moto, il calore, il movimento delle particelle, le reazioni chimiche, i fenomeni elettrici, sono alcuni dei capitoli che via via ci introducono alla seconda parte, quella dedicata al « corpo ». « Ancora sui problemi della vita e della non vita » è il capitolo che inizia questa trattazione condotta sui fenomeni e sulle strutture molecolari la cui importanza è stata messa in evidenza dagli sviluppi sorprendenti della biologia di questi ultimi anni. Oggi noi sappiamo che le centinaia di reazioni biochimiche sono dovute a dei catalizzatori, gli enzimi, che Asimov mette al centro della sua trattazione. L'ultimo capitolo « La sorgente ultima » è dedicato al problema di dove provenga in ultima analisi l'energia necessaria all'uomo come ai più piccoli microrganismi.

ASTIER, J.-L., **Géophysique appliquée à l'hydrogéologie,** Masson & C.ie, Paris, 1971, VIII+278 pp., figs., 120 F.

L'ouvrage est divisé en trois parties. Dans la première partie, on passe en revue les méthodes de prospection; dans la seconde, on examine, pour chaque grand problème hydrogéologique, les possibilités de ces différentes méthodes. La troisième partie est consacrée au carottage des sondages. L'auteur a cherché à réaliser un traité pratique. Il a évité les longs développements mathématiques mais donne toutes les formules simples nécessaires pour le calcul de l'ordre de grandeur des anomalies à attendre. Il n'est pas entré dans le détail de la mise en oeuvre mais fournit suffisamment d'éléments sur les appareillages, la composition des équipes et les rendements moyens pour permettre à l'hydrogéologue de préparer des programmes et d'en contrôler l'exécution.

Atlante geografico generale Zanichelli, Zanichelli Editore, Bologna, 1971, XII+218 pp., figs., L. 3.000.

L'*Atlante Geografico Generale* che la Zanichelli propone al pubblico italiano è nato dalla collaborazione tra l'esperienza scientifica e didattica di alcuni insegnanti e geografi italiani e la collaudata capacità tecnica di una delle più note case cartografiche europee. L'opera si propone di offrire un'immagine chiara del nostro pianeta. Tale obiettivo può

essere raggiunto grazie alla moderna tecnica cartografica che permette di rendere con evidenza i fatti salienti della geografia. L'Atlante, nonostante la facile leggibilità, non si propone di servire unicamente a fini scolastici, ma di fornire tutte quelle informazioni che si rivelano sempre più di frequente necessarie a chiunque voglia seguire gli avvenimenti mondiali. L'opera, oltre al materiale cartografico, contiene un numero rilevante di immagini fotografiche che possono essere di notevole utilità.

BALTAY, Ch. and ROSENFELD, A. H. (eds.), **Experimental Meson Spectroscopy,** Columbia University Press, London, 1970, XIV+664 pp., figs. and tables, £6.75.

The Second International Conference on Experimental Meson Spectroscopy was held at the University of Pennsylvania on May 1 and 2, 1970. It was attended by about 200 particle physicists, representing most of the experimental groups working on particle properties, and including some theoretical physicists. This volume is not intended as a proceedings but rather as a collection of articles loosely based on material presented at the conference. There have been included some papers not presented at the conference, and a number of presented papers were not written up.

BARBIERI, G. and ALMAGIÀ, R., **L'Italia,** Unione Tipografico-Editrice Torinese, Torino, 1971, XX+596 pp., tables, plates and maps, No price stated.

CONTENTS. - *I: L'Italia, il suo nome, i suoi confini.* - *II: Come si è formata l'Italia. Lineamenti di storia geologica.* - *III: Gli aspetti del paesaggio naturale.* - *IV: L'azione del mare e le forme costiere.* - *V: Le Alpi e i paesaggi alpini.* - *VI: I rilievi subalpini e la pianura padano-veneta.* - *VII: L'Appennino e i suoi paesaggi.* - *VIII: Monti e conche dell'Antiappennino tirrenico.* - *IX: Aspetti del rilievo della Sicilia e della Sardegna.* - *X: Il clima e le sue variazioni.* - *XI: Sorgenti, fiumi e laghi.* - *XII: Suoli e vegetazione. La fauna.* - *XIII: L'uomo: caratteri etnici, sociali e demografici.* - *XIV: Le sedi umane: case, città, strade.* - *XV: L'opera trasformatrice dell'uomo sul suolo agricolo e forestale.* - *XVI: L'economia agricola e la vita rurale.* - *XVII: L'economia industriale e i traffici.* - *XVIII: Lo Stato italiano.* - *XIX: Regioni e città dell'Italia settentrionale.* - *XX: Regioni e città dell'Italia centrale.* - *XXI: Regioni e città dell'Italia meridionale e insulare.*

BASS, J., **Cours de mathématiques,** VOL. III: **Topologie, Intégration, Distributions, Equations intégrales, Analyse harmonique,** Masson & C.ie, Paris, 1971, 406 pp., figs., 64 F.

CONTENTS. - I. Topologie. Espaces métriques. Espaces vectoriels normés. *I: Rappel de notations et de définitions.* - *II: Topologie générale.* - *III: Espaces métriques.* - *IV: Généralités sur les espaces fonctionnels.* - *V: Exemples d'espaces de Banach. Espace de Hilbert.* - II. Intégration. *I: Tribus. Fonctions mesurables. Mesures.* - *II: Intégrale de Lebesgue.* - *III: Espace L^1. Espace L^2.* - III. Distributions. *I: Généralités.* - *II: Opérations sur les distributions.* - *III: Convolution.* - *IV: Séries de Fourier.* - *V: Transformation de Fourier.* - *VI: Equations de convolution.* - IV. Opérateurs compacts. Equations intégrales. *I: Opérateurs compacts.* - *II: Algèbres d'opérateurs compacts.* - *III: Opérateurs hermitiens.* - *IV: Equations intégrales.* - *V: Opérateurs différentiels. Problèmes aux limites.* - V. Fonctions stationnaires. Analyse harmonique. *I: Avant-propos.* - *II: Fonctions de corrélation.* - *III: Fonctions presque-périodiques.* - *IV: Fonctions pseudo-aléatoires.*

BASS, J., **Mathématiques,** VOL. II: **Analyse** (PC Première année), Masson & C.ie, Paris, 1971, XVI+552 pp., figs., 48 F.

CONTENTS. - *Introduction.* - *I: Nombres réels. Suites.* - *II: Fonctions d'une variable réelle.* - *III: Intégrales simples.* - *IV: Fonctions élémentaires.* - *V: Formule de Taylor. Infiniment petits et infiniment grands. Développements limités.* - *VI: Généralisation des intégrales définies.* - *VII: Fonctions de plusieurs variables.* - *VIII: Théorie des courbes.* - *IX: Surfaces.* - *X: Equations différentielles du premier ordre.* - *XI: Equations différentielles linéaires du second ordre à coefficients constants.* - *XII: Intégrales doubles et triples.* - *XIII: Intégrales curvilignes. Intégrales de surface.* - *XIV: Méthodes numériques.* - Exercices.

BATHURST, R. G. C., **Carbonate Sediments and their Diagenesis** (Series: *Developments in Sedimentology*, No. 12), Elsevier Publishing Company, Amsterdam, 1971, xx+620 pp., figs. and tables, Dfl. 90.00.

The author of this book examines three main areas in which progress has been outstanding: the deposition of carbonate sediments, their diagenesis, and the detailed microstructure of the sedimentary particles. The recent expansion of knowledge about the genesis of limestones and related rocks is critically analysed. Since limestones are rocks composed of the remains of shell sands, coral reefs and other tropical sea floor sediments made of calcium and magnesium carbonates, modern deposits of these in the Bahamas, the Persian Gulf and elsewhere are treated at length. Much new detail of the microscopic structure of the sedimentary grains is given, based on work with the electron microscope. The second half of the book is devoted mainly to a consideration of the physical and chemical processes by which the unconsolidated sediments are changed to solid rock.

BATSCHELET, E., **Introduction to Mathematics for Life Scientists** (Series: *Biomathematics*, Vol. 2), Springer-Verlag, Berlin, 1971, xiv+496 pp., figs. and tables, DM 49.

The book can be used as textbook in classroom as well as for self-teaching. It also contains a good number of references. Numerous problems for solution, most of them tested in classroom, are offered at the end of each chapter. Beginning with the real numbers, the book continues with sets, relations, functions. Emphasis is placed on power functions, trigonometric functions, and especially on the exponential function and its inverse. Differential- and integral calculus, including differential equations, are mainly developed for applications to the analysis of biological systems, an area also known as cybernetics. In addition, the book contains linear algebra (vectors and matrices) and probability. Not covered are statistics and computer methods, but it is believed that these topics can be much better understood with a solid mathematical background.

BAYET, J., **Croyances et rites dans la Rome antique** (Series: *Bibliothèque Historique*), Payot, Paris, 1971, 384 pp., 49,60 F.

Cet ouvrage est un recueil des travaux que Jean Bayet avait consacrés à l'histoire des religions et en particulier aux rituels et aux croyances de la religion romaine. Ces différents textes ont été réunis par quatre éminents latinistes (Raymond Bloch, Jacques Heurgon, André Mandouze, Robert Schilling).

CONTENTS. - *I: Le rite du fécial et le Cornouiller magique. - II: Présages figuratifs déterminants dans l'antiquité gréco-latine. - III: Causalité primitive. - IV: La croyance romaine aux présages déterminants: aspects littéraires et chronologie. - V: Les « Cerialia », altération d'un culte latin par le mythe grec. - VI: Le suicide mutuel dans la mentalité des Romains. - VII: Les « Feriae Sementiuae » et les Indigitations dans le culte de Cérès et de Tellus. - VIII: Symbolique, sensibilité, techniques dans l'histoire des religions. - IX: Les plus anciennes conceptions du psychisme chez les Grecs et chez les Latins. - X: Le phénomène religieux dionysiaque. - XI: Allocution d'ouverture du VIII^e Congrès d'histoire des religions. - XII: Les sacerdoces romains et la pré-divinisation impériale. - XIII: Prodromes sacerdotaux de la divinisation impériale. - XIV: Les malédictions du tribun C. Ateius Capito. - XV: Les cendres d'Anchise: Dieu, héros, ombre ou serpent?*

BEADLE, G. W. and BEADLE, M., **Le langage de la vie** (Introduction à la génétique), (Series: *Initiation aux nouveautés de la science*, No. 14), Masson & C.ie, Paris, 1970, xii+244 pp., figs., 29 F.

La génétique qui, depuis Mendel, s'oriente de plus en plus vers la biochimie, est certainement sur le point de révolutionner de nombreux domaines de la science. Les dernières découvertes des généticiens sur l'hérédité et les variations, ainsi que leurs conséquences sur notre façon d'envisager l'évolution, ont totalement transformé notre conception du monde vivant. Le présent ouvrage est dû à la collaboration d'un Prix Nobel de médecine, dont les travaux ont largement contribué à l'élaboration des théories nouvelles, et d'une journaliste. Après une définition des gènes, de leur composition moléculaire et de leur évolution, le problème est replacé dans son contexte général, depuis l'origine de la vie jusqu'aux grandes questions de l'évolution et du devenir de l'homme.

La génétique proprement dite est alors abordée: lois de Mendel et leurs modifications, division cellulaire et transmission de l'hérédité, études des chromosomes, mécanismes biochimiques.

BEAUJEU-GARNIER, J., **La Géographie** (Méthodes et perspectives), (Series: *Géographie appliquée*), Masson & C.ie, Paris, 1971, 144 pp., figs., 24 F.

CONTENTS. - I. Qu'est-ce que la Géographie? *I: La Géographie mise en question. - II: L'unité de la Géographie. - III: La Géographie Humaine ne peut se passer de la Géographie Physique. - IV: La Géographie Physique, aussi, est liée à la Géographie Humaine. - V: Deux exemples extrêmes d'intervention du Géographe. - VI: La specificité géographique.* - II. La méthode géographique: critiques et perspectives. *I: Les processus généraux. - II: L'observation. - III: L'explication et les modèles. - IV: La précision: vers une nouvelle géographie? - V: Méthode géographique et méthode scientifique.* - III. L'espace géographique. *I: L'espace du Géographe. - II: Espace géographique et espace économique. - III: A propos de l'espace statique: la notion d'homogénéité. - IV: L'analyse de l'espace. - V: Le mythe de la « région ».* - IV. La division de l'espace. *I: Suggestions et contraintes du milieu physique. II: Principes de l'action humaine.* Conclusion. Esquisse d'une typologie de la vision de l'espace. *I: Intérêt et perspectives.*

BEC, P., **Manuel pratique de philologie romane,** VOL. II: **Français, Roumain, Sarde, Rhéto-Friulan, Francoprovençal, Dalmate. Phonologie. Index** (Series: *Connaissance des Langues*, No. 6), Editions A. & J. Picard, Paris, 1971, 644 pp., maps, 86 F.

Le manuel de Philologie Romane du Professeur P. Bec se place surtout dans une perspective historique et prend ses références dans les textes anciens. Néanmoins, des allusions aux langues modernes y sont nombreuses et les renvois aux formes et aux structures contemporaines y sont systématiques pour mettre en lumière d'une façon plus nette le double aspect de la synchronie et de la diachronie ainsi que la typologie de chacun des idiomes. Le manuel n'est pas un traité de linguistique, mais conformément aux méthodes de la philologie moderne, il exploite les résultats de la linguistique structurale et plus particulièrement de la phonologie diachronique. Outre les nombreux textes commentés, l'ouvrage est complété par 2 index importants: un index analytique, et un index de près de 20.000 formes. Il est pourvu pour chacune des langues étudiées, d'une bibliographie critique.

BEERS, R. F. Jr. and BRAUN, W. (eds.), **Biological Effects of Polynucleotides** (Proceedings of the Symposium on Molecular Biology, held in New York, June 4-5, 1970), Springer-Verlag, Berlin, 1971, XVI+344 pp., figs. and tables, DM 86.

This volume, based on the contributions and discussions of a symposium held in June 1970, deals with a large number of biological effects of polynucleotides, with special emphasis on the effects of synthetic polymers on virus infections, immune responses, tumor development, and biosynthesis. The material represents a review of a rapidly developing area of research that encompasses many disciplines allied to molecular biology and clinical medicine. The material presented shows that nucleic acids, aside from their informative qualities, have important regulatory effects that are relevant to our understanding of the complex interactions within and among cells and between organisms and their environment.

BERTOLINI, B., **Lo sviluppo di una rana** (Introduzione all'embriologia), Zanichelli Editore, Bologna, 1971, 94 pp., figs., L. 2.000.

È passato molto tempo da quando gli scienziati, osservando al microscopio le prime immagini confuse e approssimative di un uovo fecondato, credevano di vedere in esso l'animale già pre-formato. Tutto il complesso fenomeno della formazione di un individuo completo a partire dalla cellula uovo fecondata si riduceva così a un processo di ingrandimento. Il trascorrere degli anni e lo sforzo di molti scienziati hanno distrutto interpretazioni così ingenue, ma non hanno ancora dato una risposta esauriente. Questo libro

è una breve e semplice introduzione all'embriologia dedicata ai ragazzi (il volume fa parte, infatti, della collana Biologia per i giovani, diretta da Giorgio Tecce) ai quali l'autore pone e illustra alcuni dei problemi fondamentali della biologia connessi direttamente o no con un fenomeno che ogni anno si ripete ai bordi degli stagni quando in primavera le rane depongono le uova e pochi mesi dopo, all'inizio dell'estate, i nuovi piccoli salgono tra le erbe della riva.

BIASS-DUCROUX, F. (ed.), **Glossary of Genetics,** in English, French, Spanish, Italian, German, Russian; Elsevier Publishing Company, Amsterdam, 1970, XVI+436 pp., Dfl. 85.00.

This glossary has been compiled to provide the user with a rapid and adequate translation of the terms in current use in the field of genetics. Much care has been taken to avoid a mere transcription of words from one language into the others. Each word has been given special attention to ensure at least one equivalent or, where necessary, several equivalents corresponding to its specific meaning. Different spellings or synonyms have also been indicated, when neither an adequate equivalent nor a readily acceptable neologism could be found, the sense of the word has sometimes been conveyed in a short paraphrase.

BOENER, H., **Representations of Groups** (With Special Consideration for the Needs of Modern Physics), North-Holland Publishing Company, Amsterdam, 1970, XIV+342 pp., figs., Dfl. 50.

The contents of this book are purely mathematical; it is also intended for the physicist, and this requirement has determined the selection of subject matter and the style of presentation. No knowledge of higher algebra is assumed beyond the familiar material of a first course. On the other hand only so much of the general theory is given as is necessary for the principal concern of the book, which is to give a complete treatment of the representations of certain particular groups. These are the symmetric and alternating permutation groups and the most important linear groups, including the full linear group and the rotation and Lorentz groups of arbitrary dimension.

CONTENTS. - *I: Matrices. - II: Groups. - III: General Representation Theory. - IV: Representations of the Symmetric Group. - V: Representations of the Full Linear, Unimodular and Unitary Groups. - VI: Characters of the Linear and Permutation Groups. The Alternating Group. - VII: Characters and Single-Valued Representations of the Rotation Group. - VIII: Spin Representations, Infinitesimal Ring, Ordinary Rotation Group. - IX: The Lorentz Group.*

BORN, M., **Structure atomique de la matière** (Series: *Physique*), Librairie Armand Colin, Paris, 1971, 414 pp., figs. and tables, 68 F.

Ce livre constitue une introduction à la physique quantique. L'auteur élabore le formalisme quantique de façon progressive, à partir des données expérimentales, qu'il expose en détail. Dans chacun des chapitres (la théorie cinétique des gaz, les particules élémentaires, l'atome et son noyau, ondes et corpuscules, la structure de l'atome et les raies spectrales, le spin de l'électron et le principe de Pauli, les statistiques quantiques, la structure des molécules, la théorie quantique des solides, la physique du noyau), le lien entre données expérimentales et théoriques est clairement établi. Pour faciliter à l'étudiant la compréhension de ce lien, les démonstrations de caractère mathématique sont reportées en fin d'ouvrages.

BOTRÉ, C., **Potenziali di membrana e fenomeni bioelettrici,** Carlo Manfredi Editore, Milano, 1970, XII+92 pp., figs. and tables, L. 3.000.

Nel 1934 Danielli proponeva una prima teoria sulla struttura della membrana biologica e il doppio strato lipidico compreso fra due strati proteici entrava così a far parte del mondo scientifico. Negli ultimi dieci anni si è assistito a un fenomeno del tutto singolare: il risveglio d'interesse per le membrane ed i loro problemi è stato accompagnato dall'ingresso della chimico-fisica in un campo che sembrava esclusivamente e naturalmente riservato a biologi e fisiologi. Il contributo maggiore alle nostre conoscenze, in questi ultimi anni, deriva appunto dall'applicazione dei principi della chimico-fisica ai fenomeni di membrana. In quest'opera il lettore non esperto di termodinamica troverà, almeno inizialmente, scoraggiante la folla di equazioni riportate in maniera non del tutto a lui familiare. Se riuscirà a superare la sua istintiva diffidenza, non potrà non restare affascinato da questo mondo di pochi centesimi di micron che determina la vita.

BROECKER, W. S. and OVERSBY, V. M., **Chemical Equilibria in the Earth,** McGraw-Hill Book Company, Düsseldorf, 1971, XII+318 pp., figs. and tables, DM 69.30.

This book is for one-semester or two-semester courses in the application of chemical methods to problems in the earth sciences. The first five chapters treat fundamental concepts of physical chemistry such as volume, energy, entropy, equilibrium, and rates. The above is an introduction to thermodynamic logic from a macroscopic and an atomic point of view. The remaining material deals with the most common types of chemical reactions encountered in the earth sciences. In these chapters, theories are first presented and then applied to particular geochemical problems. Special features include integration of theory and applications in the belief that understanding why a formula works is necessary to proper application of the formula, treatment of isotopic reactions and isotopic fractionations from a theoretical and practical point of view, and comparison of the behavior of aqueous solutions at elevated temperatures and pressures with the behavior at one atmosphere.

BRONWELL, A. B. (ed.), **Science and Technology in the World of the Future,** John Wiley & Sons Ltd., London, 1970, XXII+394 pp., figs. and tables, £5.75.

Science and Technology in the World of the Future presents a timely speculative approach to the future of applied science and technology in the next quarter century. Nineteen contributors present philosophical projections of rapidly evolving fields that will alter our way of life and our structures of knowledge. Some articles discuss such topics as hypersonic air transport traveling at 7,000 miles per hour, the unlocking of the atomic nucleus, an instantaneous world-wide communication network, and other topics. This book is written in non-mathematical language for scientists and engineers. To students in engineering orientation courses, it provides an introduction into the profession and a projection of the future that they will be creating.

BUNGE, M. (ed.), **Problems in the Foundations of Physics** (Series: *Studies in the Foundations Methodology and Philosophy of Science*, No. 4), VIII+162 pp., figs., DM 54.

CONTENTS. - *I:* M. BUNGE, *A Mathematical Theory of the Dimensions and Units of Physical Quantities*. - *II:* D. G. B. EDELEN, *Reflections on Variational Principles and Invariance Theory*. - *III:* P. HAVAS, *Galilei- and Leorentz-Invariant Particle Systems and their Conservation Laws*. - *IV:* P. G. BERGMANN, *Foundations Problems in General Relativity*. - *V:* E. J. POST, *Geometry and Physics: A Global Approach*. - *VI:* A. FINE, *Probability in Quantum Mechanics and in other Statistical Theories*. - *VII:* A. J. KÁLNAY, *The Localization Problem*. - *VIII:* A. KOMAR, *Semantic Foundations of the Quantization Program*. - *IX:* R. ARIS, *Algebraic Aspects of Formal Chemical Kinetics*. - *X:* S. A. KLEINER, *Criteria for Meaning Changes in Theoretical Physics*. - *XI:* T. W. SETTLE, *The Relevance of Philosophy to Physics*.

BUTERY, A., **Mathématiques pour les sciences sociales,** VOLUME 4: **Intégration; processus continus** (Series: *Systèmes-Décisions*), Presses Universitaires de France, Paris, 1971, VI+232 pp., 33 F.

Cet ouvrage est une introduction au calcul intégral. Il s'adresse essentiellement aux étudiants et aux élèves des classes préparatoires aux concours d'enseignement supérieur commercial. Selon les orientations de l'enseignement, il peut être abordé en premier, ou éventuellement en second cycle. Le calcul intégral est l'outil nécessaire à l'étude des processus continus, que ceux-ci soient physiques ou économiques. Plus

difficile à aborder que les méthodes d'études des processus discrets (suites et séries), son utilisation se révèle plus simple. Près du quart de l'ouvrage est consacré aux applications du calcul intégral aux sciences de l'économie et de la gestion. De plus, le calcul intégral est à la base du calcul des probabilités et de la statistique. Un chapitre initie le lecteur à ces importants domaines.

BUZON, J. et coll., **Structure et propriétés moléculaires,** VOLUME I: **Hydrocarbures et fonctions univalentes** (Series: *Monographies de Chimie Organique*), Masson & C.ie, Paris, 1970, XVI+942 pp., figs. and tables, 290 F.

Le tome VII des Monographies de Chimie Organique est consacré à l'examen des relations entre les propriétés physiques des molécules organiques et leur structure. On sait combien l'utilisation des spectres, en particulier, joue un rôle capital et même l'emporte sur les méthodes purement chimiques dans l'identification des composés connus et dans l'établissement de la formule des composés nouveaux. C'est la systématique chimique qui détermine le caractère de l'étude entreprise. Sa trame est formée par une fonction chimique ou par un type de structure. En face d'une famille chimique donnée, se trouvent confrontées les différentes méthodes physiques d'étude structurale: moments dipolaires, spectres optiques, résonance magnétique nucléaire. Ce n'est donc pas dans l'esprit du physico-chimiste, cherchant surtout à établir et à illustrer les relations théoriques correctes que sont examinées les propriétés. C'est essentiellement le point de vue du chimiste organicien qui a prévalu, cherchant à préciser les structures jusque dans leurs détails, allant au-delà de ce que peuvent donner les analyses théoriques les plus poussées. Cela n'est possible que par une vaste confrontation empirique de données précises très nombreuses, permettant de délimiter le domaine de validité de certaines relations structurales et de localiser quelques aberrations, dont la systématique n'est pas toujours bien définie.

CAGNET, M., FRANÇON, M. and MALLICK, S., **Atlas optischer Erscheinungen** (Ergänzungsband), Springer-Verlag, Berlin, 1971, 36 pp., plates, DM 88.

Diese Zusammenstellung ist eine Ergänzung zu dem im Jahre 1962 erschienenen Atlas optischer Erscheinungen. Er enthält 11 Farbund 4 Schwarz-Weiss-Tafeln. Die wiedergegebenen Erscheinungen betreffen: a) Interferenz, Beugung und Polarisation; b) Interferometrie und Interferenzmikroskopie; c) Holographie und holographische Interferometrie. Jeder Tafel ist ein einfacher Text beigefügt, der in Verbindung mit schematischen Abbildungen die photographischen Erscheinungen erklärt. Dieser Band wird Studenten und Lehrer der Optik interessieren.

CARNAP, R., **I fondamenti filosofici della fisica** (Introduzione alla filosofia della scienza), Il Saggiatore, Milano, 1971, 368 pp., figs., L. 2.500.

Questo volume è la rielaborazione di un seminario ripetutamente tenuto da Carnap nel corso del suo insegnamento universitario. Dato il contesto in cui si è sviluppato, esso affronta in modo lucido e rigoroso, ma al tempo stesso elementare, alcuni dei nodi cruciali della scienza contemporanea (teoria della relatività generale e meccanica quantistica), illuminando inoltre i suoi tratti formali e strutturali più importanti. Tutti i temi vengono affrontati utilizzando in modo informale le moderne tecniche della logica matematica. I risultati più originali sono ottenuti da Carnap a proposito della distinzione analitico-sintetico, di cui viene dimostrata la possibilità rispetto all'intero linguaggio della scienza, mediante l'uso del cosiddetto enunciato di Ramsey, che è stato recentemente al centro di molteplici dibattiti relativi anche al problema dell'eliminabilità dei termini teorici.

CARRIER, R., **Biochimie médicale** (Principes généraux de l'analyse en biochimie médicale et notions élémentaires de méthodologie instrumentale), Librairie Maloine S.A., Paris, 1971, 176 pp., figs., 36 F.

L'ouvrage du Dr Carrier est un outil de travail pour l'étudiant et le technicien en biochimie médicale. Les notions générales de méthodologie instrumentale sont particulièrement adaptées aux besoins du laboratoire d'hôpital mais elles sont applicables à toute analyse biochimique. Les principes généraux de l'analyse sont exposés d'abord et avant tout dans une optique clinique. Les chapitres consacrés à la cueillette, la préservation et la préparation des échantillons devraient être indispensables au personnel infirmier des hôpitaux modernes. L'analyse biochimique ne se fait pas qu'à l'intérieur du laboratoire. Elle commence au chevet du patient et il est indispensable que les infirmières et les techniciens réalisent l'importance de la *qualité* de leur travail et comprennent les répercussions qu'elle peut avoir sur le résultat final des analyses dont dépend, en dernier ressort, le bien-être du malade.

CHRISTOPHOROU, L. G., **Atomic and Molecular Radiation Physics** (Series: *Wiley Monographs in Chemical Physics*), John Wiley & Sons Ltd., London, 1971, x+772 pp., figs. and tables, £10.50.

This book gives a picture of many apparently different areas of research in atomic and molecular photophysics and atomic and molecular collision processes and it discusses the relevance and importance of these fundamental studies, at atomic and molecular level, to radiation science and biology. It covers the recent research on atoms and molecules (small and complex), obtained both by photophysical and particle collision processes, and discusses the importance of such studies to the understanding of the interaction between radiation and matter and biological action of molecules. The physics of polyatomic molecules is stressed as well as its basic role in biology.

CHVIDCHENKO, I. (ed.), **Large Space Programs Management** (Proceedings of the European Colloquium Organized by the Centre National D'Etudes Spatiales), Gordon and Breach, London, 1971, XXII+342 pp., figs., £8.10.

CONTENTS. - I. Management Principles and Aids. *I: A Management Model for a Large Project. - II: Program Management Techniques. - III: L'espace est'il parkinsonien? Les moyens de conduite d'un projet. - IV: Coordination d'un projet international: Le programme Hawk. - V: L'organisation du projet Héos. - VI: L'organisation des projets spatiaux dans un laboratoire de recherches. - VII: Management Information. Oil for the Space Program Machine. - VIII: Principes de gestion des projets spatiaux. - IX: Cost Estimation of Satellite Projects.* - II. Organization and Structure. *X: Problèmes de maîtrise d'oeuvre industrielle, maître d'ouvrage, maître d'oeuvre. - XI: L'équipe de projet de l'industriel maître d'oeuvre d'un grand programme spatial. - XII: Organisation d'un programme chez le maître d'oeuvre. - XIII: Les consortiums.* - III. Contract. *XIV: Evolution du fondement des marchés publics du juridique à l'économique. - XV: La propriété industrielle dans les marchés d'étude et de développement. - XVI: ELDO Contracts Procedures: Present Situation and Future Aspects. - XVII: Le contrat avec intéressement.* - IV. Quality Control. *XVIII: Aspect contractuels de la qualité. - XIX: A la recherche de la qualité. - XX: Organisation of Quality Control.* - V. Discussions.

Circuits à semiconducteurs dans l'industrie, VOL. I: **Appareils de mesure,** Masson & C.ie, Paris, 1970, 556 pp., figs. and tables, 75 F.

L'objet de cet ouvrage est d'indiquer les possibilités et le mode d'emploi des dispositifs à semiconducteurs dans la construction des appareils de mesure; en fait, c'est une étude basée sur les solutions les plus récentes en matière de construction (matériaux y compris), applications, avantages, inconvénients et possibilités de développement futur des appareils analogiques et numériques destinés à la mesure des grandeurs électriques, non électriques et biologiques. L'ouvrage comprend trois chapitres consacrés à la mesure des grandeurs électriques, non électriques et biologiques, le tout complété d'un chapitre introductif et d'un autre destiné aux méthodes et aux appareils de mesure numériques.

CÔME, D., **Les obstacles à la germination** (Series: *Monographies de Physiologie Végétale*, No. 6), Masson & C.ie, Paris, 1970, 162 pp., figs. and tables, 60 F.

Le but de cet ouvrage est l'analyse des causes de l'absence de germination. L'auteur s'est en effet attaché principalement à rechercher les mécanismes physiologiques, souvent

fort complexes, impliqués dans ces phénomènes. Après une introduction et un bref historique qui situent les problèmes étudiés, l'auteur tente de préciser quelques termes souvent mal définis tels que germination, dormance, inhibition, vitesse de germination. Il examine ensuite comment interviennent certain facteurs (l'eau, l'oxygène, la température, la lumière et les inhibiteurs) pour empêcher (ou permettre) la germination. Si le rôle de l'eau est classique, celui de l'oxygène est beaucoup moins bien connu. La température est toujours un facteur très important, mais son intervention dans l'acquisition de l'aptitude à germer reste inexpliquée. Depuis la découverte du phytochrome, pigment responsable de la sensibilité des semences à l'éclairement, les connaissances sur le mode d'action de la lumière ont considérablement progressé. L'existence d'inhibiteurs de la germination est indiscutable, mais on ne sait pas toujours très bien quel rôle précis leur attribuer.

COOKES, H. St. L., **The Eyes, Brain and Nerve System in Relation to the Earth's Magnetism** (An Explanation of why the Dowser's Pendulum and other Media Move, and other Phenomena), Devon Rustics Ltd., Bideford, 1971, 132 pp., diagrams, £2.25.

CONTENTS. - *I: Introduction. - II: Experiments. - III: The Pendulum. - IV: The Eyes. - V: Sensitiveness. - VI: The Eyes in Relation to Light. - VII: The Eyes in Relation to the Earth's Magnetism. - VIII: The Eyes in Relation to Light and the Earth's Magnetism. - IX: The Eyes in Relation to Movement. - X: The 'Pole', 'Opposite Pole', 'Base Pole', and 'Cycle of Movements'. - XI: Colours. - XII: The Elements. - XIII: The Magnet. - XIV: Water Divining. - XV: The Human Body. - XVI: Conclusion.*

CUTTER, E. G., **Plant Anatomy: Experiment and Interpretation,** PART 2: **Organs** (Series: *Contemporary Biology*), Edward Arnold (Publishers) Ltd., London, 1971, VIII+344 pp., figs., £6.50.

This book describes the basic structure and function of plant organs from a developmental standpoint and considers the causes underlying the development of particular structures. To this end the various experimental approaches to the study of plant anatomy are discussed. These include surgical experiments, grafting, chemical and hormonal treatments, histoautoradiography, histochemistry and tissue and organ culture. The results of these experiments are used to extend and interpret more fully the classical knowledge of plant anatomy gained by observation with the light microscope and with transmission and scanning electron microscopes, and especially to gain some insight into the factors controlling the differentiation and arrangement of particular tissues and cell types, and the development of particular forms of organs. The developmental approach of the book results in an extensive treatment of plant meristems. This volume is concerned with the organisation, or orderly arrangement, of tissues into the plant organs, and with how this is brought about; thus it provides a fairly complete survey of the relationships between plant structure, growth and metabolism at various levels of differentiation. Numerous illustrations of both basic structure and the results of experimental treatments are included.

DAVIES, G. L., **The Earth in Decay** (A History of British Geomorphology, 1578-1878), Elsevier Publishing Company, Amsterdam, 1969, XVI+390 pp., plates, Dfl. 55.

The book is a study of the evolution of British thought upon the subject of the origin of the Earth's topography and it traces the history of the science which is now known as geomorphology through the 300 years which followed the publication of William Bourne's *Treasure for Travellers* in 1578. It discusses the changing views of British scholars on the subject of the Earth's history, and traces their beliefs about the origin of all orders of landforms, from the continents and ocean basins down to individual mountains and valleys. Attention is paid to the intellectual background against which the scholars worked, and the influence of this background upon geomorphic thought is closely analysed; especial concern is paid to the vicissitudes through which the fluvial doctrine passed before it finally earned acceptance during the middle years of the nineteenth century. Foreign influences on British scholars are considered where necessary.

DUBACH, U. C. and SCHMIDT, U. (eds.), **Recent Advances in Quantitative Histo- and Cytochemistry** (Methods and Applications), (Series: *Current Problems in Clinical Biochemistry,* Vol. 3), Verlag Hans Huber, Bern, 1971, 364 pp., figs. and tables, sFr. 78.

CONTENTS. - I. Methodological Aspect. *I: Structure of Animal Tissue after Freezing. - II: Microbiological Assay in Quantitative Histochemistry. - III: Quantitative Microscope-Photometric Determination of Enzyme Activities in Cryostat Sections. - IV: Some Recent Refinements of Quantitative Histochemical Analysis. - V: The Extension of Micro-Analysis and Histochemistry to Coenzyme A and Certain Coenzyme A dependent Enzymes. - VI: Methods for the Isolation for Cells with Special Reference to the Pancreatic Islets.* - II. Micro- and Ultramicromethods for the Analysis of Cellular Metabolism. *VII: Micromethods for Protein and Enzyme Analysis. - VIII: Quantitative Histochemistry of Glucose Metabolism in the Islets of Langerhans. - IX: Determination of Elemental Composition in Subcellular Portions of Neurons. - X: Cytochemical Analysis by Laser Microprobe-Emission Spectroscopy.* - III. Applications. *XI: The Metabolic Flux in Single Neurons during Ischemia and Anaesthesia. - XII: Brain and Nerve Tissue. Biochemical Correlates of Neural Activity. - XIII: Quantitative Histochemical Studies of the Eye and the Ear. - XIV: Studies of Embryonic Bone and its Modification by Parathyroid Extract. - XV: Quantitative Histochemistry of the Endocrine Pancreas. - XVI: Anatomical and Methodological Problems of Quantitative Histochemistry in Renal Tissue. - XVII: NaK ATPase in the Rat Nephron Related to Sodium Transport. - XVIII: The NaK-activated ATPase in Isolated Cell Membranes of the Rat Kidney Cortex. - XIX: Selection of Enzymes for Activity Measurements in Energy Metabolism.*

DUMITRIU, A., **Theory and System** (Series: *Euromediterranea,* No. 2), Cappelli Editore, Urbino, 1970, VI+58 pp., L. 600.

The author points out that nowadays the idea of theory has been replaced by the idea of system, and therefore the conception on science of our times bears a certain, quite particular significance. Science, however, cannot reduce to system, which is only one of its aspects.

CONTENTS. - *I: Introduction. - II: Aristotle's Concept of Theory. - III: Formal of Science. - IV: Relativity of Primitive Terms, Axioms and Theorems in Formal Systems. - V: Fundamental Objection. - VI: Logical Conventionalism. - VII: Aristotle's Solution. - VIII: Conclusions.*

EBEL, F. and BIRNBAUM, O., **Schöne und seltsame Welt der Orchideen,** Edition Leipzig, Leipzig, 1971, 208 pp., tables and coloured plates, DM 54.

Dieses einzigartige und Buch ist das Ergebnis der Zusammenarbeit zweier kenntnisreicher Orchideen-Liebhaber, der eine wissenschaftlicher Botaniker, der andere ein bekannter Fotograf. Der Text, der jede einzelne Abbildung begleitet, ist in sich geschlossen und gibt spezielle Erläuterungen über die verschiedenen Orchideenarten, Gattungen, Subtriben, Triben und Familien, Ordnungen und Klassen sowie über Zucht und Vorkommen. Im Anhang finden sich noch zahlreiche Schwarz-weiss-Abbildungen von Orchideen in ihrem natürlichen Lebensraum sowie Ergänzungen zu den Bildtexten. Der Leser findet Angaben über Blütengrössen, Fachausdrücke, Morphologie, Orchideen-Illustrationen aus 5 Jahrhunderten sowie ausführliche bibliographische Hinweise.

ENERGLYN and BREALEY, **Analytical Geochemistry** (Series: *Methods in Geochemistry and Geophysics,* No. 5), Elsevier Publishing Company, Amsterdam, 1971, XVI+426 pp., figs. and tables, Dfl. 80.00.

This book is an attempt by a geologist and a chemist to produce a form of introductory manual to analytical techniques for use in the laboratory which are commonly required for the analysis of rocks and minerals. Their intention is to introduce some analytical techniques to those geologists who did not study chemistry as undergraduates. Its objective is to guide the geologist rather than to attempt to instruct him in chemistry. All the methods chosen have proven value in obtaining chemical data from rocks and minerals. A fairly comprehensive list of references has been provided for those intending to pursue geochemical investigations.

CONTENTS. - *I: Introduction to Geochemistry.* - *II: Qualitative Analysis.* - *III: Quantitative Chemical Analysis.* - *IV: The Chemical Analysis of the Minor Elements.* - *V: Emission Spectrography.* - *VI: Flame Photometry.* - *VII: X-Ray Spectrography.* - *VIII: X-Ray Diffraction.* - *IX: Fluorimetry.* - *X: Chromatography.*

ERIXON, S.† (ed.), **The Possibilities of Charting Modern Life** (Series: *Wenner-Gren Center International Symposium*, No. 13), Pergamon Press Ltd., Oxford, 1970, XIV+170 pp., £3.00.

At its regular session on 12 March 1966 « The Nordic Council for Anthropological Research », Stockholm, decided to arrange a symposium for modern anthropological research. This was held at the Wenner-Gren Center in Stockholm on 7-11 March 1967. The Committee appointed to realize the symposium considered that general ethnology should be given the same possibilities as European regional ethnology, with regard both to theoretical questions and to the exemplification of the material.

CONTENTS. - Sigurd Erixon: In Memoriam, by J. GRANLUND. - Introduction, by S. ERIXON†. - Address of Welcome, by S. ERIXON†. - *I: Etnological Investigation of the Present*, by S. ERIXON†. - *II: Functional Theory in the Light of Tuareg Culture*, by J. NICOLAISEN. - *III: Applied Ethnology as an Example of Ethnological Research on the Present*, by G. NELLEMANN. - *IV: The Traditional Sex Roles in a Changing Social Structure*, by A.-B. HELLBOM. - *V: Investigation of a Village in Norrbotten, 1963-4*, by A.-M. NYLÉN. - *VI: Frontier Research in Tornedalen*, by H. HVARFNER and A. VILKUNA. - *VII: Report of an inter-Nordic Joint Scientific Research Plan: Insurance in Connection with the Children's Birth and First Year of Life*, by L. WEISER-AALL. - *VIII: Ethnology of the Present-Theoretical and Practical Possibilities*, by K. KOLSRUD. - *IX: Two Analytical Perspectives in the Study of Folklife*, by O. BLEHR. - *X: Urban Ethnology and Research on the Present*, by S. B. EK. - *XI: Comparative Milieu Studies. Some Topical Points of View*, by A. ESKERÖD. - *XII: Ethnological Research on the Present in Finland: Some Problems and Aspects*, by I. TALVE. - *XIII: Ethnological Research and Teaching at the University of Iyväskylä*, by A. VILKUNA.

ESCALLE, J.-E., **Après l'infarctus** (Dans l'exercice journalier de la médecine praticienne), (Series: *Pour Le Praticien*), Librairie Maloine S.A., Paris, 1971, 140 pp., tables, 28 F.

L'infarctus du myocarde, maladie fréquente, survenant souvent comme une «épidémie», atteint toutes les professions et depuis environ une vingtaine d'années, s'observe chez des malades de plus en plus jeunes. L'auteur étudie les différentes modalités cliniques préexistantes à l'infarctus. Il divise les diverses possibilités et difficultés de la convalescence suivant les âges des malades. Chaque étape de cette convalescence est examinée et suivie pas à pas. C'est dans les détails et en montrant leur importance, que l'auteur accompagne son malade, au milieu de son entourage, dans la vie quotidienne, pendant la convalescence, jusqu'à la reprise de l'activité dite normale. Il détaille les différents loisirs, les énumère, les jauge et leur donne leur place dans la vie après l'infarctus. Les régimes alimentaires particuliers, si importants pour le bon équilibre du malade, sont étudiés suivant qu'il existe ou non althérosclérose, la possibilité d'une insuffisance cardiaque, les cas avec troubles digestifs. Un grand chapitre énumère les différentes médications, depuis les anticoagulants, les vaso-dilatateurs, les tonicardiaques, les calmants, etc., pour que le médecin puisse choisir selon les diverses éventualités susceptibles de se présenter.

FAURRE, P., **Navigation inertielle optimale et filtrage statistique** (Series: *Méthodes Mathématiques de l'Informatique*, No. 1), Dunod, Paris, 1971, XVI+446 pp., figs., 175 F.

Cet ouvrage présente les méthodes récentes de filtrage statistique (filtres de Wiener et de Kalman) utilisées pour la synthèse des systèmes de navigation optimaux. Il contient un exposé complet et progressif des méthodes de filtrage statistique: les connaissances requises étant du niveau des mathématiques de l'ingénieur et des bases du calcul des probabilités. La présentation des systèmes de navigation par inertie est telle qu'elle peut être abordée, soit par des ingénieurs chargés de leur construction et de leur synthèse, soit par des utilisateurs plus soucieux de l'aspect opérationnel. Les composants inertiels (gyroscopes, accéléromètres et plates-formes) sont envisagés en détail dans des chapitres différents et un chapitre important présente les calculateurs embarqués universels et spécialisés.

FINNEY, D. J., **Probit Analysis,** 3rd ed., Cambridge University Press, London, 1971, XVI+334 pp., and tables, £5.80.

Probit analysis is a system of statistical techniques chiefly concerned with the relation between the *frequency* of occurrence of a specific response and the numerical amount of a *dose* or other stimulus that tends to induce the response. This book, now in its third edition, is for scientists (especially biologists) and technologists concerned with experiments in which probits or closely related techniques are now in common use. It is a manual on statistical aspects of dose-response experiments in which the response is quantal, including both the planning of the experiment and the analysis of results. The basic methods are explained and illustrated in terms that can be understood by those who have little mathematical knowledge.

FLÜGGE, S., **Practical Quantum Mechanics,** VOL. I and VOL. II, (Die Grundlehren der mathematischen Wissenschaften, No. 177 and 178), Springer-Verlag, Berlin, 1971, XIV+342 and XII+288 pp., figs., DM 70 and DM 60.

CONTENTS. - **Volume I.** - I. General Concepts. - II. One-Body Problems without Spin. *I: One-Dimensional Problems. - II: Problems of Two or Three Degrees of Freedom without Spherical Symmetry. - III: The Angular Momentum. - IV: Potentials of Spherical Symmetry. - V: The Wentzel-Kramers-Brillouin (WKB) Approximation. - VI: The Magnetic Field.* - **Volume II.** - III. Particles with Spin. *I: One-Body Problems. - II: Two- and Three-Body Problems.* - IV. Many-Body Problems. *I: Few Particles. - II: Very Many Particles: Quantum Statistics.* - V. Non-Stationary Problems. - VI. The Relativistic Dirac Equation. - VII. Radiation Theory. - Mathematical Appendix.

FORSDYKE, A. G., **Meteorological Factors in Air Pollution** (Technical Note No. 114), World Meteorological Organization, Geneva, 1970, X+32 pp., figs. and tables, No price stated.

The growing awareness of the problems of increasing air pollution from urban industrial sources has led quite naturally to a requirement for more information on the subject. Since this phenomenon is one of relatively recent origin there are few classical texts to which the meteorologist, engineer or layman may refer in order to broaden his knowledge or help him solve particular problems. The Working Group on Atmospheric Pollution and Atmospheric Chemistry of the WMO Commission for Atmospheric Sciences recognized this deficiency at one of its first meetings and requested Mr. A. G. Forsdyke to prepare a Technical Note on the subject. Mr. Forsdyke has produced a work designed specifically for the non-specialist who from time to time will be required to solve problems or provide advice on matters relating to air pollution. He describes the text as « intended for the ordinary meteorologist and non-meteorologist - not for the air pollution expert ».

FRANCONI, C. (ed.), **Magnetic Resonances in Biological Research,** Gordon and Breach, London, 1971, XII+408 pp., figs. and tables, £10.20.

This book contains the reports of physicists, chemists and biologists working in the field of magnetic resonance spectroscopy applied to systems of biological interest. These reports were read at the Third International Conference on Magnetic Resonances in Biological Research, held at S. Margherita of Cagliari, Italy. The reviews and reports read at the conference and included in this volume concern chiefly studies on the interactions of small molecules with proteins, ESR studies of metalloproteins and in particular of hemoglobin, NMR structural and conformational studies of polypeptides and proteins.

It must at once be said that the papers read at the conference confirmed the first expectations that new horizons in molecular biology are being opened by the particular view points of the magnetic resonances methods.

GARDNER, M., **Nouveaux divertissements mathématiques,** Dunod, Paris, 1970, VIII+242 pp., figs., 24 F.

Il y a trente ans, les divertissements mathématiques n'étaient qu'un artifice, point trop désagréable, pour compléter la culture mathématique des écoliers et les amener à consentir un effort supplémentaire: pour beaucoup de gens, ils représentaient une sorte de devoirs de vacances. Le genre s'est profondément modifié et ne s'adresse plus aujourd'hui au même public. Il ne s'agit plus de parfaire les connaissances mathématiques d'un enfant mais d'étendre, à travers les mathématiques, la culture générale du grand public curieux et de l'amener à réfléchir à certaines applications inattendues ou insoupçonnées de quelques théories mathématiques complexes à la vie pratique. L'ouvrage évoque, en particulier, le fameux problème de la carte à quatre couleurs et la structure du nombre pi. L'arithmétique binaire est à la mode avec l'avènement des ordinateurs; un chapitre en dévoile quelques aspects.

GIESE, A. C., **Fisiologia della cellula,** Piccin Editore, Padova, 1971, XX+708 pp., figs. and tables, L. 12.000.

CONTENTS. - Introduzione: *Definizione della citofisiologia. - I: Applicazione alla cellula delle leggi della termodinamica.* - I. L'organizzazione funzionale della cellula. *II: L'organizzazione della cellula. - III: La biochimica della cellula. - IV: Le proprietà colloidali della cellula. - V: La struttura e le funzioni degli organelli cellulari. - VI: L'organizzazione chimica e le funzioni del nucleo.* - II. L'ambiente della cellula. *VII: L'acqua, i gas e la pressione nell'ambiente della cellula. - VIII: Gli elettroliti nell'ambiente della cellula. - IX: Le radiazioni nell'ambiente della cellula. - X: La temperatura come fattore dell'ambiente cellulare.* - III. Gli scambi di materiali attraverso le membrane cellulari. *XI: La natura chimica e la struttura delle membrane cellulari. - XII: Il movimento di acqua attraverso le membrane cellulari: l'osmosi. - XIII: Il movimento di soluti attraverso le membrane cellulari in risposta ad un gradiente di concentrazione. - XIV: Il trasporto attivo attraverso le membrane cellulari e la pinocitosi.* - IV. Le trasformazioni dell'energia e della materia nella cellula. *XV: Gli enzimi della cellula. - XVI: La liberazione di energia nella cellula. - XVII: I potenziali di ossido-riduzione nei processi di ossidazione cellulare. - XVIII: La respirazione cellulare, la fermentazione e la luminescenza. - XIX: La fotosintesi. - XX: Le biosintesi. - XXI: Regolazione e controllo del metabolismo cellulare.* - V. L'eccitabilità e la contrattilità. *XXII: I potenziali di azione della cellula. - XXIII: Sviluppo, propagazione e trasmissione dei potenziali di azione. - XXIV: La contrattilità. - XXV: Aspetti chimici della contrattilità.* - VI. Accrescimento e divisione della cellula. *XXVI: L'accrescimento cellulare. - XXVII: La divisione cellulare.* - VII. Prospettive della fisiologia cellulare. *XXVIII: Prospettive della fisiologia cellulare: profilo storico.*

GORBATSKII, V. G., **Exploding Stars and Galaxies,** Israel Program for Scientific Translations, Jerusalem, 1970, VI+122 pp., figs., $9.00.

Our understanding of the Universe has greatly expanded in recent years. Among the most significant topics tackled by the modern tools of astronomy and astrophysics were the explosions in galactic nuclei - cosmic catastrophes of extraordinary power - and quasars - extremely distant objects of exceptionally high luminosity. These phenomena attracted the attention of wide circles of scientists, and the repercussions of these discoveries were not limited to physics and astrophysics. Explosive processes are a common phenomenon in the stellar universe. Recent discoveries only confirmed the important role of explosions in the evolution of stars and giant star systems. So far, however, no account of the whole range of cosmic explosions - from solar flares to exploding galactic nuclei - has been published for the layman. The aim of this book is to fill this gap to a certain extent. Some of topics considered in this book have been so far treated in scientific papers only. This includes, for example, the role of explosions in the evolution of celestial bodies. For this reason the book may prove of some interest for the more qualified readers too.

GRASSÉ, P.-P. (ed.), **Traité de Zoologie. Anatomie, Systématique, Biologie,** Vol. XVI: **Mammifères,** Issue III: **Musculature des membres, musculature peaucière, musculature des monotrèmes, arthrologie,** Masson & C.ie, Paris, 1971, 1.210 pp., figs. and tables, 390 F.

Le fascicule 3 du Tome XVI contient la fin de la partie consacrée à l'étude des muscles. Elle a pour auteurs trois éminents anatomistes: Mme Françoise K. Jouffroy, MM. J. Lessertisseur et R. Saban. Les muscles, par leur forme, leurs insertions, portent la trace du passé de l'espèce, mais ils traduisent aussi tout ce que le comportement, les « habitudes » imposent aux structures. Les transformations de la musculature ne sont pratiquement jamais isolées; elles accompagnent des changements du squelette et du système nerveux. La coordination des variations de ces trois ensembles d'organes: système nerveux, squelette, musculature, est assurément l'une des grandes caractéristiques des Mammifères et des Reptiles thériodontes qui leur ont donné naissance. Les chapitres concernant la musculature des membres ont été rédigés par Mme F. K. Jouffroy, spécialiste de l'anatomie de ces parties. L'exposé des données purement anatomiques est suivi d'un chapitre donnant un large aperçu de la myologie fonctionnelle. Dans ce fascicule, l'arthrologie des Mammifères est traitée en 321 pages. Mme J. D. Souteyrand-Boulenger s'est chargée de rédiger ce chapitre. Un index alphabétique des matières, très détaillé et fort de 46 pages, permet au lecteur de se servir, avec facilité, de ce volume en tant qu'ouvrage de référence et de haute érudition.

GROUCHKO, D., **Operations Research and Reliability** (Proceedings of a NATO Conference held at Turin, Italy, June-July, 1969), Gordon and Breach, London, 1971, xvi+626 pp., figs. and tables, £14.35.

A conference sponsored by the Scientific committee of NATO was held in Turin, from the 30th of June to the 4th of July 1969, with participation of specialists from 15 countries. The program of this conference combined fields of operations research and reliability. Methods used in the various papers are characterised by the spirit of operations research, reliability being the field of application. A general presentation of the subject of the conference and of the scientific activity of NATO are given in the introductory papers by Professor Auguste Bargoni and Professor Jacques H. Lestel.

GRUNDMANN, Ch. and GRÜNANGER, P., **The Nitrile Oxides** (Versatile Tools of Theoretical and Preparative Chemistry), (Series: *Organische Chemie in Einzeldarstellungen,* Vol. 13), Springer-Verlag, Berlin, 1971, viii+242 pp., tables, DM 98.

Contents. - *I: Introduction. - II: Physical Properties. - III: Preparation. - IV: Chemical Reactions Involving Only the Nitrile Oxide. - V: Addition Reactions Leading to Cyclic Structures: 1,3-Dipolar Cycloadditions. - VI: Addition Reactions Leading to Open-Chain Structures; 1,3-Additions. - VII: Miscellaneous Reactions. - VIII: Physiological Properties and Applications of Nitrile Oxides. - IX: Tabular Survey.*

GRÜSSER, O.-J. and KLINKE, R. (eds.), **Pattern Recognition in Biological and Technical Systems** (Proceedings of the 4th Congress of the Deutsche Gesellschaft für Kybernetik, held at Berlin, Technical University, April 6-9, 1970), Springer-Verlag, Berlin, 1971, xii+414 pp., figs., DM 89.

The papers read at the previous meetings of the Deutsche Gesellschaft für Kybernetik encompassed the entire field of biological, psychological and technical cybernetics. For the Fourth Congress, it was decided to concentrate for the first time on one area of cybernetics. The Program Committee (O.-J. Grüsser, H. Marko, H. Mittelstaedt) selected papers on the psychology, biology and technology of pattern recognition in the belief that a common consideration of research in these areas would be especially useful. Additionally, a lecture on the origins of the science of cybernetics was included in the program and is placed at the beginning of this volume. This contribution is of particular interest because the facts concerning the scientific accomplishments of Felix Lincke (1840-1917) and Hermann Schmidt (1894-1968) in this field are not widely known.

GURZADYAN, G. A., **Planetary Nebulae,** D. Reidel Publishing Company, Dordrecht, 1970, xiv+314 pp., figs., tables and plates, Dfl. 53.

This book gives a comprehensive survey of the observational material available at that time and of the earlier theoretical work on planetary nebulae. A substantial portion of the book is devoted to Dr. Gurzadyan's own research, especially on the origin, evolution and morphology of these objects. The present translation incorporates nearly eighty pages of new material prepared by Dr. Gurzadyan. The most important of the work published subsequently is referred to in editorial footnotes. To increase the utility of the list of references, many of which are to works published in the USSR, the titles, when available, have been added to the citations. References to English translations have also been included.

CONTENTS. - *I: Basic Observational Data. - II: The Origin of Emission Lines. - III: The Problem of Distances and Dimensions of Planetary Nebulae. - IV: Temperatures of Nuclei, Electron Temperatures and Electron Concentrations. - V: The Continuous Spectrum of Planetary Nebulae. - VI: Radiative Equilibrium of Planetary Nebulae. - VII: Double-Envelope Nebulae. - VIII: The Stability of the Forms of Gaseous Envelopes. - IX: Magnetic Fields in Planetary Nebulae. - X: The Origin of Planetary Nebulae.*

HANDSCOMBE, E., **Electrical Measuring Instruments** (*The Wykeham Technological Series*, No. 2), Taylor & Francis Ltd., London, 1970, x+116 pp., figs. and tables, £1.25.

CONTENTS. - *I: Introductory. - II: Brief History of Electrical Measuring Instruments. - III: Classification of Instrument Types and their Uses. - IV: Design. - V: Materials and Finishes. - VI: Presentation. - VII: Manufacturing Techniques. - VIII: Extension of Range. - IX: The Special Problems of Aircraft Instruments. - X: Transducers. - XI: What of the Future?*

HARDIN, G., **Birth Control,** The Bobbs-Merrill Company, Inc., Indianapolis, 1970, XII+130 pp., figs. and tables, $1.95.

The problem of birth control has a great importance in the life of every responsible individual. The author of this book, a biologist, shows the scientific aspects of the problem and tries to offer a practical guide to solve it. He believes that every child should be a wanted child, both for the sake of the parents and for the sake of the child.

CONTENTS. - *I: Older than History. - II: The Theoretical Courage of Theoretical Men. - III: Every Child a Wanted Child. - IV: The Local Geography of Birth Control. - V: Breaking the Taboo. - VI: Nature's Own Method of Contraception. - VII: «I've Got Rhythm!»-Or Have I? - VIII: What is Natural Anyway? - IX: Is it Safe? - X: Thinking Makes it So. - XI: Can I Accept the Irrevocable? - XII: Lost Anything? - XIII: Is Morality Relative? - XIV: How Facts Influence Attitudes. - XV: Two Ambiguous Methods. - XVI: Which is Best? - XVII: A World Within Our Grasp.*

HARRÉ, R., **The Method of Science** (*The Wykeham Science Series*, No. 8), Taylor & Francis Ltd., London, 1970, XII+124 pp., figs., £1.25.

The History and Philosophy of Science are usually presented around those developments in mathematical physics and astronomy which culminated in the work of Sir Isaac Newton, and those biological studies that centre round the theory of Darwin. This book emphasizes the achievements of different men and looks at science in a different way. The experimental tradition is emphasized, and magnetism is taken as the central problem around which the growth of physics is sketched.

CONTENTS. - *I: Science Before Copernicus. - II: The Magical Tradition and Atomism. - III: The Early History of Magnetism. - IV: William Gilbert. - V: The Origins of the Knowledge of Plants. - VI: Early Modern Ideas on the Life of Plants. - VII: The Life of Stephen Hales. - VIII: The Sap. - IX: The Air. - X: Vegetable Life.*

Inquinamenti dell'aria, dell'acqua e del suolo, Dott. A. Giuffrè Editore, Milano, 1970, XII+284 pp., L. 3.000.

In questo volume, pubblicato a cura dell'Istituto per la Documentazione e gli Studi

Legislativi, sono raccolti gli atti del convegno sugli inquinamenti dell'aria, dell'acqua e del suolo, svoltosi nel Novembre del 1969. Gli intervenuti illustrano con dovizia di dati i danni ed i pericoli derivanti dagli inquinamenti di varia natura ed illustrano i mezzi che la scienza e la tecnica oggi sono in grado di offrire per difendere l'ambiente. In appendice il volume contiene anche gli strumenti legislativi oggi vigenti in Italia e le proposte di legge intese a regolare la delicata materia.

JAGER, K. W., **Aldrin, Dieldrin, Endrin and Telodrin** (An Epidemiological and Toxicological Study of Long-Term Occupational Exposure), Elsevier Publishing Company, Amsterdam, 1970, 234 pp., figs. and tables, No price stated.

Before the widespread use of pesticides, insects, mites, nematodes, weeds and fungi caused crop losses which had drastic effects on human life and the economy. In 1941 DDT was introduced as an insecticide and since then a large number of organic compounds have been synthesised and introduced as pecticides. At the present time they are considered to be indispensable for the production of an adequate food supply for an increasing world population and for the control of insect-borne diseases. Many pesticides are very toxic substances and some are persistent in character. For agricultural and public health purposes this persistence is, at times, essential. For example, rural malaria control would have been impossible without persistent insecticides. This is one of the main reasons why the organochlorine insecticides such as DDT and dieldrin, which possess this characteristic, have been amongst the most successful insecticides yet discovered. The need for frequent application does not occur with these compounds. This volume first reviews briefly the different aspects of the pesticide problem as a general background for the further study of human health data.

JAMMER, M., **Storia del concetto di forza** (Studio sulle fondazioni della dinamica), Feltrinelli Editore, Milano, 1971, 294 pp., figs., L. 3.800.

Scopo di quest'analisi storico-filosofica della nozione fisica di forza è quello di metter l'accento sull'importanza di questo concetto nel pensiero fisico contemporaneo. Questo libro è stato scritto nella convinzione che la fisica contenga un messaggio per la filosofia, nello stesso modo in cui le riflessioni filosofiche sono idonee a fecondare il pensiero fisico. Questa mutua relazione tra fisica e filosofia fu chiaramente intesa e opportunamente espressa dai filosofi italiani della natura nel Cinquecento, Patrizi, Telesio, Bruno e i loro seguaci, scientificamente più preparati, Benedetti, Borelli, e, soprattutto, Galileo. Di fatto il loro contributo allo sviluppo del concetto di forza nella sua fase più antica non fu meno decisivo del contributo che, in tempi recenti, Marcolongo, Levi-Civita e Amaldi hanno dato al moderno sviluppo della dinamica.

Journal de l'année (Tous les événements du monde du 1er juillet 1970 au 30 juin 1971), Larousse Editeur, Paris, 1971, VI+478 pp., figs., No price stated.

Une équipe de 120 collaborateurs a travaillé jour après jour pour élaborer cette synthèse de l'année, sous la direction de Maurice Barrois. Dans le flot d'informations que déversent constamment journaux, radio et télévision, le Journal de l'année a retenu ce qui était fondamental, classé et hiérarchisé les événements et les idées qui ont agité le monde. Le Journal de l'année enregistre, depuis cinq ans, tous les événements politiques, artistiques, religieux, quotidiens qui font le monde contemporain.

KENNEDY, D. M., **Birth Control in America** (The Career of Margaret Sanger), Yale University Press, London, 1970, XVI+320 pp., £3.95.

In this volume Mr. Kennedy describes Mrs. Sanger's great personal influence on the course of the birth control movement and examines those elements of her thought and character which shed light on the nature of twentieth-century feminism and reform. He shows that Mrs. Sanger contributed as much to women's continuing subordination as she did to their liberation. Similarly, he describes how she took the birth control issue with her as she moved from a commitment to radical anarchism to middle-class respectability, thereby transforming what she had conceived as a proletarian weapon in the class struggle into a conservative instrument of social control. Mr. Kennedy also explores the heritage of nineteenth-century attitudes about the family, women, and sex which influenced the reception given to Mrs. Sanger's proposed reform.

KING, L. S., **The Road to Medical Enlightenment 1650-1695,** Elsevier Publishing Company, Amsterdam, 1970, x+210 pp., Dfl. 40,00.

In the second part of the 17th century a whole series of brilliant scientists, born into what was already an atmosphere of innovation, came to the height of their powers. Many systems of ideas were under attack, including Galenism, the metaphysically-based system of medicine which had reigned supreme since the 2nd century A. D. Many new systems were being put forward. Dr. King selects for special study the ideas of five men. Three relied heavily on metaphysics. Riverius, who clung to Galenism while others were rejecting it; Van Helmont, who leant on the neoplatonism of Paracelsus; and the famous English scientist Robert Boyle, who rested his medical theory on the 'new' atomism. In contrast were the rationalist Sylvius, who sought a basis for medicine in chemistry rather than in metaphysics, and the empiricist Thomas Sydenham, who similarly spurned metaphysics, claiming that this theories derived directly and solely from what was observable.

LAMOTTE, M. and BOULIÈRE, F. (eds.), **Problèmes d'écologie: L'échantillonnage des peuplements animaux des milieux aquatiques,** Masson & C.ie, Paris, 1971, 294 pp., figs. and tables, 90 F.

Les deux premiers chapitres de l'ouvrage, dus respectivement à A. Chodorowski et G. Tufféry, passent en revue les appareils et méthodes qu'utilisent les hydrobiologistes pour capturer les Invertébrés vivant dans les eaux continentales, stagnantes ou courantes, et en déterminer les densités. Dans le troisième chapitre, J. Daget présente les techniques d'échantillonnage des poissons dans les eaux douces. Les trois chapitres suivants traitent des techniques d'échantillonnage des Invertébrés marins: zooplancton (A. Bourdillon), macrofaune (J.-R. Reys et B. Salvat) et microfaune des sédiments meubles (J. Renaud-Mornant). Enfin, le dernier chapitre du livre, de J. Daget et P. Petit, discute des méthodes d'échantillonnage des peuplements de poissons marins.

CONTENTS. - *I: L'échantillonnage des peuplements d'invertébrés des eaux continentales stagnantes. - II: L'échantillonnage des peuplements d'invertébrés des eaux continentales courantes. - III: L'échantillonnage des peuplements de poissons d'eau douce. - IV: L'échantillonnage du zooplancton marin. - V: L'échantillonnage de la macrofaune des sédiments meubles marins. - VI: L'échantillonnage de la microfaune des sédiments meubles marins. - VII: L'echantillonnage des peuplements de poissons marins.*

LARGEAULT, J., **Enquête sur le nominalisme,** Editions Nauwelaerts, Louvain, 1971, xvi+454 pp., 950 Fr.B.

CONTENTS. - I. La formulation du problème des universaux. *I: Des différents sortes de nominalisme. - II: Le problème des universaux et les origines du nominalisme.* - II. Quelques formes de nominalisme au Moyen Age. *I: Le nominalisme au XI*e* siècle. - II: Abélard: Les universaux et le langage. - III: Ockham: Les universaux et l'ontologie. - IV: La doctrine ockhamienne des relations.* - III. Nominalisme et empirisme à l'époque moderne. *I: Le nominalisme médiéval et la philosophie moderne. - II: Le nominalisme baroque de Hobbes. - III: Berkeley: un nominalisme intuitiviste. - IV: Langue et calcul d'après Condillac. - V: Le nominalisme de Hume. - VI: Du dissonantisme à l'harmonisme. - VII: Nominalisme et économie politique au XVIII*^{e} *siècle.* - IV. Nominalisme et technique logique à l'époque contemporaine. *I: Le nominalisme et la philosophie des sciences. - II: Le nominalisme et les tendances actuelles de la philosophie de la logique et des mathématiques. - III: Le nominalisme de Russell et de Lesnievski. - IV: Le nominalisme de Goodman et de Quine. - V: Le nominalisme et les calculs logiques classiques. - VI: Les procédés de formation imprédicatifs. - VII: Le problème de l'infini.* - Conclusion.

LEGRAND, G., **Algèbre linéaire et multilinéaire et géométrie différentielle** (Cours professé à l'Ecole Polytechnique), Masson & C.ie, Paris, 1971, 342 pp., 80 F.

Ce livre reproduit, à quelques variantes près, le « Cours d'Algèbre et Géométrie » de l'Ecole Polytechnique, professé par l'auteur de 1964 à 1969. Sa lecture n'implique que peu de connaissances préalables: les définitions élémentaires d'algèbre (groupes, anneaux, corps), ainsi que la notion de déterminant d'une matrice carrée, et, pour les chapitres de géométrie différentielle, les notions fondamentales de topologie générale, ainsi que le calcul différentiel dans les espaces affines de dimension finie.

CONTENTS. - *I: Compléments sur les groupes. - II: Espaces vectoriels. - III: Applications linéaires. - IV: Formes bilinéaires et sesqui-linéaires. - V: Algèbre tensorielle. - VI: Algèbre extérieure. - VII: Espaces affines. - VIII: Variétés différentiables. - IX: Calcul différentiel extérieur. - X: Etude des surfaces d'un espace euclidien de dimension 3.*

LEIPP, E., **Acoustique et musique,** Masson & C.ie, Paris, 1971, 340 pp., figs., 80 F.

CONTENTS. - I. Notions de physique et de technologie acoustique. *I: Généralités. La chaîne de communication des messages acoustiques. - II: Nombres, puissances, logarithmes. Unités de mesure et diagrammes. - III: Mouvements périodiques. Sinusoïdes. Loi de Fourier. - IV: Génération et propagation du son. - V: Les composants d'une chaîne électro-acoustique. - VI: Enregistrement et reproduction du son. Electrophones et magnétophones. - VII: Structure physique des messages sonores. Représentation graphique du son.* - II. L'audition des sons. *VIII: Un modèle fonctionnel de l'audition. Le système auditif humain. - IX: Audition des sons; la sensation d'intensité. - X: Audition des sons; la sensation de hauteur et d'intervalle. - XI: Audition des sons. Sensation de timbre et de forme.* - III. Les instruments de musique. *XII: Généralités sur les instruments de musique. - XIII: Les instruments à cordes. Principes de fonctionnement. - XIV: Quelques archétypes d'instruments à cordes. - XV: Les instruments à vent. Principes de fonctionnement. - XVI: Quelques archétypes d'instruments à vent. - XVII: Les instruments à percussion.* - IV. Introduction aux musiques expérimentales et à l'acoustique des salles. *XVIII: Les musiques expérimentales. - XIX: L'acoustique des salles.*

LOMBARD, J., **Psychologie énergétique et typologie fonctionnelle,** Librairie Maloine S.A., Paris, 1971, XXIV+318 pp., figs., 48 F.

CONTENTS. - I. Energétique et dynamique psychiques. *I: Energétique, physiologie, psychologie. - II: De l'énergétique à la fonction. - III: Energétique de l'évolution.* - II. Recherche d'une dynamique humaine. *IV: Mouvements et évolution de l'énergie psychique. - V: L'affectivité. - VI: Dynamique des échanges et de l'élaboration.* - III. Vers une psychologie fonctionnelle. *VII: Fonctions psychologiques. - VIII: Mécanismes et types fonctionnels.* - IV. Vers la personnalité. *IX: Structure et instances psychiques. - X: Le moi, l'individualité, la personnalité.* - Appendice: *Remarques et suggestions pratiques. Problèmes humains.*

LOWE, P. G., **Classical Theory of Structures** (Based on the Differential Equation), Cambridge University Press, London, 1971, XIV+216 pp., figs. and tables, £5.40.

Dr Lowe's book is for the most part traditional in content, the topics covered forming part of normal undergraduate courses in civil and structural engineering. His approach is, however, quite different to that of most modern treatments, which are based on an energy formulation; instead, Dr Lowe bases his formulation on the underlying differential equations, the novelty of his approach being that he exploits the concept of discontinuity while, at the same time, placing the usual emphasis on continuity.

LUTZ, H. (ed.), **Invertebrate Organ Cultures** (Colloquium on Experimental Embryology, Clermont-Ferrand, April 1968), Gordon and Breach, London, 1970, XIV+252 pp., figs. and tables, £9.25.

CONTENTS. - *I: Methods and Media for Invertebrate Organ Culture,* by N. LE DOUARIN. - *II: Study of Differentiation and Morphogenesis in Coelenterates by Culture* In Vitro, by L. GOMOT. - *III: Studies on the Morphogenesis of Planarian Regeneration Blastemas in Organotypic Culture,* by C. ZILLER-SENGEL. - *IV: Sex Differentiation of*

Mollusks in Organ Culture, by L. GOMOT. - *V: An Experimental Study of the Effect of Nerve Ganglia on Morphogenis of the Penis and on the Genital Activity of* Crepidula fornicata Phil. (*Gastropod Mollusk*), by P. LUBET and W. STREIFF. - *VI: Experimental Data on the Effects of Removal of Nerve Ganglia of Pelecypod Mollusks*, by P. LUBET. - *VII: The Organotypic Culture of Insect Tissues*, by TH. LEUDET. - *VIII: The Metamorphosis of the Mesenteron of* Galleria mellonella. *Histological Study in Culture. Influence of Various Factors*, by P. NARDON and G. PLANTEVIN. - *IX: Organotypic Culture* In Vitro *and Endocrine Mechanisms in Crustaceans*, by J. BERREUR-BONNENFANT. - *X: Experimental Analysis of Sexuality in* Asterina gibbosa *with the Particular Use of Organotypic Culture*, by R. DELAVAULT and J. BRUSLÉ. - *XI: Trophins, Humoral Factors, and Neurosecretion in Invertebrates*, by M. DURCHON.

MARTINEZ, E., **Le tredici stelle** (Series: *Periscopio Minore*, No. 9), Massimo Editore, Milano, 1970, 396 pp., plates, L. 1.500.

L'opera è la storia delle tredici colonie inglesi dalle quali ebbero origine gli Stati Uniti d'America: dalle scoperte di Colombo, Caboto, Vespucci e Verrazzano sino alla metà del secolo decimottavo, quando si acuì il conflitto tra le colonie e la madre patria. L'autrice si propone di illustrare, non solo la genesi della nazione americana, ma anche le sue contraddizioni, che fa derivare in parte dal fatto che i suoi abitanti furono « uomini senza medioevo » che, staccatisi da una società in qualche misura progredita, furono costretti dalle circostanze ad accettare una vita primitiva.

CONTENTS. - Introduzione: *L'America precolombiana*. - *I: Il periodo dei Tudor (1485-1603)*. - *II: Il primo periodo degli Stuart (1603-1649) e del Commonwealth (1640-1659)*. - *III: Il secondo periodo degli Stuart (dalla Restaurazione - 1660 al 1689)*. - Conclusione: *Le colonie angloamericane dal 1689 al 1763.*

MASON, B. (ed.), **Handbook of Elemental Abundances in Meteorites** (Series: *Extraterrestial Chemistry*, Vol. I), Gordon and Breach, London, 1971, x+556 pp., figs. and tables, £14.60.

The editor has brought together the extensive information about the abundances of the elements in the different classes of meteorites. One chapter is devoted to each element or group of closely-related elements, and the authors of the individual chapters are usually scientists who have been responsible for much of the analytical work on these specific elements. The greater part of the data collected in this book has been obtained during the past decade.

MAXWELL, A. E. (ed.), **The Sea** (Ideas and Observations on Progress in the Study of the Seas), VOLUME 4: **New Concepts of Sea Floor Evolution,** PART I and II: **Regional Observation Concepts,** John Wiley & Sons Ltd., Chichester, 1970, Part I: XII+792 pp., figs. and tables, £15.25, Part II: XIV+664 pp., figs. and tables, £15.50.

This volume is the continuation of the series published in 1963 under the editorship of Maurice N. Hill. Originally, only three volumes covering physical oceanography, composition of sea water, comparative and descriptive oceanography, and the history of the earth beneath the sea had been planned. However, the accumulation of a vast amount of knowledge in the relatively short period since those volumes were published, has necessitated continuing the series. Volume Four, *New Concepts of Sea Floor Evolution*, is divided into two sections: General Observations, and Regional Observations and Concepts. The first section contains 20 original, previously unpublished articles that treat the knowledge of the sea floor by scientific discipline on a worldwide basis. The second section of *New Concepts of Sea Floor Evolution* includes sixteen articles. Fourteen of them present a more intensive treatment of certain regional areas and integrate geological and geophysical data.

McGONNAGLE, W. J., **Nondestructive Testing,** 2nd ed., Gordon and Breach, London, 1971, XII+456 pp., figs. and tables, No price stated.

Nondestructive testing—the application of physical principles for detecting inho-

mogeneities in materials without impairing their usefulness—is being called upon more and more to meet the requirements of a rapidly advancing technology. The latest methods and techniques of this growing field are discussed in this book, with emphasis on the advantages, disadvantages, and limitations of each. Originated as a textbook for the beginning student and technician in the field, *Nondestructive Testing* offers a thorough discussion of fundamental topics.

McGREEVEY, W. P., **An Economic History of Colombia 1845-1930** (Series: *Cambridge Latin American Studies*, No. 9), Cambridge University Press, London, 1971, XIV+330 pp., figs. and tables, £4.60.

Between 1850 and 1930 the local economies of several major primary-producing countries in Latin America became increasingly dependent on foreign trade. Professor McGreevey examines the relations between the external sector and the domestic economy by analysing the history of economic development in Colombia in this period. Part I traces the development of the economy from the period of exploitation under Spanish rule until 1845. In Part II the author makes an overall assessment of the movement of *per capita* product between 1845 and 1885. In order to assess the impact of trade on Colombian economic development Professor McGreevey has assembled a new and more reliable set of estimates of Colombian foreign trade between 1845 and 1930. He pays particular attention to the reasons for economic decline in the latter half of the nineteenth century. Part III examines the economic development of Colombia between 1890 and 1930 from a subsistence economy with little specialisation and exchange to a market-oriented agricultural economy with greater division of labour and a more extensive trade network.

MEINCK, F., STOOFF, H. and KOHLSCHÜTTER, H., **Les eaux résiduaires industrielles,** Masson & C.ie, Paris, 1970, VIII+866 pp., figs. and tables, 150 F.

La traduction française de la quatrième édition de l'ouvrage, paru en Allemagne sous la signature de F. MEINCK et collaborateurs, met à la disposition des ingénieurs, des étudiants, des chercheurs, un instrument d'étude et de travail. Après avoir décrit l'origine, le volume, la composition et les caractéristiques des eaux résiduaires industrielles, l'ouvrage expose les considérations générales qui sont à la base du rejet de ces eaux dans les cours d'eau. Le chapitre IV, très important, décrit en détail les eaux résiduaires des diverses industries, avec indication des points de production d'eaux résiduaires, le volume de ces eaux, leur composition, leurs propriétés, les moyens d'épuration, et le rejet. Des installations de traitement, de construction française, sont décrites dans l'ouvrage. Un chapitre donne les toxicités des constituants des eaux résiduaires vis-à-vis de la faune et de la flore aquatiques. Une autre partie de l'ouvrage donne la définition du coefficient de pollution, ou équivalent-habitant, et indique les coefficients de pollution des diverses branches de l'industrie.

MOISIL, Gr. C. (ed.), **Logique, automatique, informatique,** Editions de l'Académie de la République Socialiste de Roumanie, Bucarest, 1971, 456 pp., figs., Lei 34.

Le recueil contient l'exposé de quelques recherches dues à un groupe de chercheurs de Roumanie, sous la direction de l'Académicien Gr. C. Moisil, sur des problèmes de logique pure et d'algèbre appliquée à la logique, à l'automatique et à l'informatique. Des résultats variés sont donnés, en partant de la logique pure jusqu'à ses applications à l'étude des automates, sans négliger pour autant l'informatique et la programmation aux calculateurs. Les matériaux traitent: ici les proportions de type supérieur, la minimisation des automates; la catégorie des algèbres lukasiewicziennes; les algèbres de Morgan; l'utilisation des fonctions récurrentes à l'étude des automates, etc.

MORPURGO, G., **Il mondo della cellula** (Series: *Biologia per i giovani*), Zanichelli Editore, Bologna, 1971, 116 pp., figs., L. 2.200.

Questo libro si propone di dare ai ragazzi, ai quali è destinato, una nozione il più possibile precisa della struttura della cellula e dei suoi processi vitali. L'Autore ha introdotto nella sua esposizione anche aspetti più generali della problematica biologica,

come il dibattito nella generazione spontanea o come il ruolo dei microorganismi nell'economia generale dell'ambiente naturale.

CONTENTS. - *I: Struttura e funzioni della cellula.* - *II: Gli organismi unicellulari.* - *III: Il differenziamento e l'organizzazione cellulare.* - *IV: La struttura di alcuni dei principali organi dell'animale.* - Glossario. - Indice analitico.

MORRIS, N. M., **La commande automatique dans l'industrie** (Principes fondamentaux et techniques de base), Masson & C.ie, Paris, 1971, VIII+214 pp., figs. and tables, 65 F.

CONTENTS. - *I: Généralités sur le contrôle industriel.* - *II: Moyens électroniques utilisés en contrôle industriel. Composants de base.* - *III: Moyens électroniques utilisés en contrôle industriel. Régulateurs simples.* - *IV: Réaction positive et contre-réaction. Application à l'étude des amplificateurs.* - *V: Système de régulation. Diagrammes de réponses harmoniques.* - *VI: Systèmes de régulation. Stabilité et performances.* - *VII: Moyens électrotechniques utilisés en contrôle industriel.* - *VIII: Techniques d'amplification utilisées en contrôle industriel.* - *IX: Asservissement en vitesse. Régulation de tension.* - *X: Asservissement en position.* - *XI: Techniques analogiques utilisées en contrôle industriel.* - *XII: Techniques digitales utilisées en contrôle industriel.* - *XIII: Moyens hydrauliques utilisés en contrôle industriel.* - *XIV: Moyens pneumatiques utilisés en contrôle industriel.*

NELSON, A. F., **The Development of Lester Ward's World View**, Branch-Smith, Inc., Fort Worth, 1970, VIII+68 pp., $3.95.

CONTENTS. - I. Ward's Life: Effort Rewarded. - II. The Period of Ward's First System: 1876-83. *I: Early Articles.* - *II: The Foundations of Dynamic Sociology.* - *III: Inorganic Evolution.* - *IV: Organic Evolution.* - *V: Spontaneous Variations and Natural Selection.* - *VI: Change Through Use.* - *VII: Sexual Selection.* - III. From Materialistic Telism to Voluntarism: 1884-92. *VIII: Published Papers of 1888-90.* - *IX: Anti-Weismannian Essays, 1891-3.* - *X: The Voluntarism of the Psychic Factors.* - IV. Rethinking Voluntarism: 1893-1900. *XI: « Status of the Mind Problem ».* - *XII: « The Natural Storage of Energy ».* - *XIII: Outlines of Sociology.* - V. Ward's Neo-Hegelian System: 1901-13. - VI. Epilogue.

NEVEU, J., **Bases mathématiques du calcul des probabilités**, Masson & C.ie, Paris, 1970, XII+214 pp., 68 F.

Ce livre reproduit l'essentiel d'un cours de 3e cycle à la Faculté des Sciences de Paris: il se propose de fournir aux lecteurs une base mathématique sérieuse de la Théorie des Probabilités. Le but premier de ce cours est d'apprendre au lecteur à manier cet outil puissant que constitue la Théorie de la Mesure, et de lui permettre ensuite d'aborder avec aisance tel ou tel chapitre particulier de la Théorie des Probabilités. De nombreux exercices complètent le texte. Etant donné la nature très technique du sujet exposé, il paraît souhaitable que le lecteur s'astreigne à lire et résoudre la plus grande partie de ces exercices.

CONTENTS. - *I: Espaces de probabilité.* - *II: Intégration des variables aléatoires.* - *III: Espaces produits et fonctions aléatoires.* - *IV: Espérances conditionnelles et martingales.* - *V: Théorie ergodique et processus de Markov.*

PACHOLCZYK, A. G., **Radio Astrophysics** (Nonthermal Processes in Galactic and Extragalactic Sources), W. H. Freeman and Company Ltd., Berkshire, 1970, XXIV+270 pp., figs. and tables, £6.10.

Designed for courses in radio astronomy for senior and graduate students, this book is a presentation of the physical processes—especially of the nonthermal processes—believed to be responsible for the radiation of electromagnetic waves at radio frequencies from astronomical sources. The author emphasizes the fundamental physics of radio astronomy instead of observational results or current problems, and for the most part descriptive data are represented only by appropriate references to the literature. The book thus fills a gap between primarily observational books on radio astronomy and theoretical texts on classical electrodynamics.

PIATIER, A., CAHUZAC, P. and CHAMBADAL, L., **Economie et mathématiques** (Eléments et exercices), VOLUME 2: **Analyse statistique et applications à l'économie,** Presses Universitaires de France, Paris, 1971, 446 pp., figs. and tables, 40 F.

CONTENTS. - I. Eléments d'analyse statistique. - Introduction: *Statistique et probabilité. - 0: Symbolique. - I: Statistique descriptive. - II: Calcul des probabilités. - III: L'induction statistique. - IV: Etude sommaire de quelques exemples de processus aléatoires.* - II. Application des méthodes statistiques à l'économie. *I: Distributions statistiques. Valeur centrale. Dispersion. - II: Indices. - III: Ajustements. - IV: Séries chronologiques: étude des variations saisonnières. - V: Corrélation. Régression. - VI: Lois statistiques. - VII: Tests de signification et de comparaison. - VIII: Enquêtes et sondages.*

PICARD, H. and ANTONINI, A., **Traitement médical étiologique de la coxarthrose,** Librairie Maloine S.A., Paris, 1971, 160 pp., figs., 20 F.

Après plusieurs années de mise au point le Docteur Picard et le Docteur Antonini, présentent la reconstitution tissulaire appliquée à la coxarthrose. Les auteurs font un exposé théorique de leur thérapeutique en partant de l'analyse des fonctions de l'autodéfense. Partant de leur remise en ordre à l'aide de moyens spécifiques et ordonnés (métaux et métalloïdes, électrolytes, sérums tissulaires) les auteurs décrivent leur technique. En dernier chapitre, ils nous présentent leurs résultats avec statistiques, observations cliniques et radiologiques (normalisation de la trame osseuse et régénération des interlignes détruits).

POHL, R. W., **Trattato di Fisica,** VOLUME I: **Meccanica, acustica e termologia,** Piccin Editore, Padova, 1971, XVI+352 pp., figs. and tables, L. 8.000.

CONTENTS. - *I: Introduzione, misure di lunghezza e di tempo. - II: Rappresentazione di moti, cinematica. - III: Fondamenti della dinamica. - IV: Applicazioni dell'equazione fondamentale. - V: Concetti ausiliari, lavoro, energia, impulso. - VI: Moti rotatori di corpi rigidi. - VII: Sistemi di riferimento accelerati. - VIII: Alcune proprietà dei solidi. - IX: Statica dei liquidi e dei gas. - X: Dinamica dei liquidi e dei gas - XI: Le oscillazioni. - XII: Onde progressive e radiazione. - XIII: Concetti fondamentali. - XIV: I° principio della termodinamica ed equazione di stato dei gas perfetti. - XV: Gas reali e vapori. - XVI: La temperatura e la parte cinetica dell'energia interna. - XVII: Fenomeni di trasporto, in particolare la diffusione. - XVIII: La funzione di stato entropia. - XIX: Trasformazione di energia interna in lavoro.*

POUQUET, J., **Les sciences de la Terre** (A l'heure des satellites), (Series: *Le Physicien,* No. 5), Presses Universitaires de France, Paris, 1971, 260 pp., figs. and tables, 22 F.

CONTENTS. - I. Principes de base de la télédétection. *I: Le spectre électro magnétique. - II: Visible et proche infra rouge du spectre électro magnétique. - III: Partie invisible du spectre électro magnétique (Infra rouge et hyper fréquences). - IV: Radiations émises et radiations réfléchies. Aspects concrets.* - II. Les satellites artificiels et la télédétection. *Introduction: Types de satellites. - I: Le problème de l'heure réelle. - II: Instrumentation à bord des satellites. - III: Satellites destinés aux sciences de la Terre. - IV: Traitement des informations reçues des satellites météorologiques.* - III. Applications aux sciences de la Terre. *I: Satellites habités. - II: Principes généraux d'interprétation. - III: Exemples géopédologiques. - IV: Exemples non géopédologiques.* - Conclusion Générale: *Perspectives d'avenir.*

Radiation Including Satellite Techniques (Proceedings of the WMO/IUGG Symposium held in Bergen, August 1968), (Technical Note No. 104), XXXII+556 pp., figs. and tables, No price stated.

The Executive Committee of WMO, at its seventeenth session (1965), approved the convening of a symposium on the uses of satellite data. As several seminars had already been held on the uses of satellite photographic data, it was decided that this symposium would emphasize the uses of radiation data obtained from satellites. It later became evident that this symposium could with advantage be combined with a symposium being planned by the Radiation Commission of the IAMAP (IUGG) and it was accordingly agreed to hold a WMO/IUGG Symposium on « Radiation, Including Satellite Techniques » in Bergen, Norway, during the period 22-28 August 1968. It was further decided to publish extended summaries of all papers presented in the form of a WMO Technical Note; the present publication is issued in accordance with this decision. The papers presented cover a wide range of specialized fields including satellite instrumentation, spectroscopy and radiation climatology, all of great importance for the successful culmination of the Global Atmospheric Research Programme and the World Weather Watch.

RECHCIGL, M., Jr. (ed.), **Enzyme Synthesis and Degradation in Mammalian Systems,** S. Karger, Basel, 1971, XVI+478 pp., figs. and tables, sFr. 90.

This treatise offers a review of the present knowledge of the various factors that control enzyme activity as well as the regulatory mechanisms involved in enzyme synthesis and degradation, with special emphasis on the mammalian systems. This book will be of interest and assistance to graduate students and advanced researchers working in a wide range of disciplines including general biologists, biochemists, molecular and cell biologists, enzymologists, comparative and developmental biologists, zoologists, physiologists, pathologists, geneticists, embryologists, endocrinologists, pharmacologists and nutritionists.

ROEDERER, J. G. and ZÄHRINGER, J. (eds.), **Physics and Chemistry in Space,** Four vols., Springer-Verlag, Berlin, 1970-1971; VOL. I: **Geomagnetic Micropulsations,** by J. A. JACOBS, VIII+180 pp., figs., DM 36; VOL II: **Dynamics of Geomagnetically Trapped Radiation,** by J. G. ROEDERER, XIV+166 pp., figs., DM 36; VOL. III: **Geochemical Exploration of the Moon and Planets,** by I. ADLER and J. I. TROMBKA, X+244 pp., figs., DM 58; VOL. IV: **The Optical Aurora,** by A. OMHOLT, XIV+198 pp., figs., DM 58.

The series "*Physics and Chemistry in Space*" covers the following main topics: Solar Physics, Cosmic Rays, Interplanetary Plasma and Fields, Physics of the Magnetosphere, Radiation Belts, Aurora and Airglow, Ionosphere, Aeronomy, Moon, Planets, and Interplanetary Condensed Matter, Satellite Geodesy, Satellite Meteorology, Satellite and Rocket Astronomy Techniques, Exobiology.

ROLLIN, P., **Phytochrome, photomorphogenèse et photopériodisme** (Series: *Monographies de Physiologie Végétale*, No. 7), Masson & C.ie, Paris, 1970, 136 pp., figs. and tables, 60 F.

Le phytochrome est un pigment présent chez tous les végétaux, excepté les champignons. Il joue un rôle fondamental dans la croissance et la différenciation, c'est-à-dire dans la morphogenèse. Son étude doit permettre la compréhension de nombreux problèmes de biophysique et de biologie cellulaire: photochromisme, interaction chromophore-protéine, mécanisme de la régulation de la croissance et de la différenciation cellulaire. Les photoréactions liées au phytochrome paraissent d'une grande importance pour la vie des végétaux dans les conditions naturelles. Les observations, faites sur le terrain et au laboratoire, ouvrent la voie à de nombreuses applications dont peu ont été jusqu'ici mises en pratique. Cet ouvrage repose sur une abondante bibliographie comportant les publications les plus récentes.

CONTENTS. - *I: Introduction. - II: Les photomorphoses dans le règne végétal. - III: Méthodes d'étude du phytochrome et résultats. - IV: La réaction à haute énergie. - V: Le mode d'action du phytochrome. - VI: Relations entre le phytochrome et les rythmes endogènes: le photopériodisme. - VII: Signification écologique. Applications pratiques. - VIII: Conclusions générales. - IX: Addendum. - X: Bibliographie.*

ROUBINE, E., Introduction à la théorie de la communication, Three Vols., Masson & C.ie, Paris, 1970, VOL. I: **Signaux non aléatoires,** 216 pp., figs., 80 F; VOL. II: **Signaux aléatoires,** 156 pp., figs., 64 F; VOL. III: **Théorie de l'information,** 160 pp., figs., 64 F.

Consacré à l'outillage mathématique de la théorie de la Communication l'ouvrage a pour origine un enseignement donné à l'Ecole Supérieure d'Electricité depuis plusieurs années. Mais la matière en a été considérablement étendue. Le livre est présenté en trois fascicules traitant respectivement du signal non aléatoire, du signal aléatoire et de la théorie de l'information. Le Tome I est une mise au point moderne de la théorie du signal, c'est-à-dire, en fait, pour les spécialistes de la Communication, des relations entre propriétés temporelles et propriétés spectrales des signaux. Le Tome II est consacré aux signaux aléatoires dont l'application la plus importante en Communication est la représentation du bruit. Mais on peut considérer que leur théorie a une portée bien plus vaste. Aussi un développement assez complet a-t-il été donné notamment aux processus faiblement stationnaires et à leur analyse harmonique. Le sujet n'est pas toujours facile, aussi a-t-on donné une présentation assez classique et aussi concrète que possible. Le Tome III est la présentation devenue aujourd'hui classique de la théorie de l'information. L'accent a été mis sur la théorie du codage qui en est peut être l'aspect le plus important à la fois par des applications techniques immédiates mais aussi par le rôle théorique qu'elle joue dans la formation des théorèmes fondamentaux.

Satellite and Computer Applications to Synoptic Meteorology (Lectures presented during the Scientific Discussions at the Fifth Session of the Commission for Synoptic Meteorology, Geneva, 15 June - 3 July 1970), World Meteorological Organization, Geneva, 1971, XIV+88 pp., figs. and tables, No price stated.

At its fifth session, in 1970, the Commission for Synoptic Meteorology conducted a programme of scientific discussions based on a series of invited lectures on two subjects of current interest: the use of satellite information in synoptic meteorology and numerical methods for detailed weather forecasting. The rapid progress made in recent years on both topics has had a direct impact on the development of the operational work in synoptic meteorology. Conventional techniques used in analysing and forecasting weather situations are now being supplemented by new ones. As a result, the requirements for the type of processed information to be exchanged internationally and the formats to be used for such exchange are changing. An explanation of the research underlying these changes, which was made possible through the scientific discussions at its fifth session, enabled the Commission to visualize future trends in synoptic meteorology which are important for its work. In view of the great interest shown in the various papers which were presented and to enable them to be read by those who were unable to attend the session, it was decided to publish these papers in a suitable form, and the present publication is the outcome of this decision.

SCHMIDT, A., Philosophische Studien zur Populationsgenetik, Veb Gustav Fischer Verlag, Jena, 1970, VIII+78 pp., DM 7.

CONTENTS. - *I: Die Stellung der Populationsgenetik im System der Wissenschaften. - II: Populationen als Ebene der biologischen Systemhierarchie. - III: Probleme bei der Untersuchung von Populationen. - IV: Anwendung der Populationsgenetik.*

SEGRÉ, E., Enrico Fermi, fisico (Una biografia scientifica), (Series: *Saggi Zanichelli,* No. 11), Zanichelli Editore, Bologna, 1971, VI+284 pp., plates, L. 2.800.

Emilio Segré, professore di fisica a Berkeley (California), Premio Nobel 1958 per la scoperta dell'antiprotone, mostra in questa biografia di Fermi, come la sua versatilità scientifica non fosse manifestazione di un sia pur geniale eclettismo, ma, al contrario, espressione di una visione unitaria dei problemi della scienza e risultato di una rara capacità di considerare gli apparecchi sperimentali e le tecniche matematiche non come fini ultimi dell'attività di ricerca, ma unicamente come strumenti per la comprensione delle leggi della natura. Di Fermi, Emilio Segré fu prima allievo e poi collaboratore

negli anni fra il 1927 e il 1935, a Roma. Emigrato negli Stati Uniti pochi mesi prima di Fermi, fu con lui a Los Alamos nel laboratorio che studiò la costruzione delle prime bombe atomiche; dopo la fine della guerra i due scienziati mantennero assai frequenti contatti di lavoro e di amicizia fino alla morte di Fermi, avvenuta nel 1954. Questa biografia è perciò basata su molte testimonianze personali dell'autore, per la maggior parte inedite; inoltre Segré ha compiuto un paziente lavoro di ricerca, raccogliendo informazioni sulla giovinezza di Fermi, sui suoi studi prima a Roma e poi alla Scuola Normale di Pisa, sugli inizi della sua carriera professionale. Per il periodo dei primi anni del depoguerra, in cui Fermi fu influente consigliere del governo degli Stati Uniti sulle questioni nucleari, malgrado che molti documenti siano ancora segreti, Emilio Segré ha potuto illuminare la decisa posizione presa da Fermi contro la nascente psicosi di stampo maccartista sulle questioni relative ai segreti atomici e i suoi suggerimenti in favore di tentativi di accordi internazionali per il disarmo. In appendice al libro sono pubblicate le interessantissime lettere inedite scritte tra il 1917 e il 1926 da Fermi al giovane amico Enrico Persico, che divenne poi professore di fisica teorica a Roma.

TAYLER, R. J., **The Stars: Their Structure and Evolution** (*The Wykeham Science Series*, No. 10), Taylor & Francis Ltd., London, 1970, XII+208 pp., figs. and tables, £1.50.

Many branches of physics such as gravitation, thermodynamics, atomic physics and nuclear physics are combined in determining the structure of stars. Physical conditions in stars are more extreme than on Earth and a successful understanding of their structure should show how valid it is to extrapolate established physical laws to these conditions. Considerable progress has been made in explaining the observed properties of stars, but many observations are not fully understood. A major aim of this book is to introduce the reader to work in a developing subject and the uncertainties of present theories are often emphasized.

CONTENTS. - *I: Introduction.* - *II: Observational Properties of Stars.* - *III: The Equations of Stellar Structure.* - *IV: The Physics of Stellar Interiors.* - *V: The Structure of Main Sequence Stars.* - *VI: Early Post-Main-Sequence Evolution and the Ages of Star Clusters.* - *VII: Advanced Evolutionary Phases.* - *VIII: The Final Stages of Stellar Evolution: White Dwarfs, Neutron Stars and Gravitational Collapse.* - *IX: Conclusions and Probable Developments.* - Appendix: *Thermal Equilibrium.*

TONINI, V., **Scienza dell'informazione, cibernetica, epistemologia** (Series: *L'uomo e la società*, No. 14), Bulzoni Editore, Roma, 1971, 392 pp., L. 5.000.

CONTENTS. - I. Modelli di conoscenza in fisica. *I: Le teorie fisiche all'inizio dell'era atomica.* - *II: I fondamenti fisici del conoscere.* - *III: L'informazione naturale.* - II. Teoria elementare dell'informazione. *IV: Definizione di processo.* - *V: La teoria delle informazioni in senso stretto.* - III. La scienza dei sistemi. *VI: Sistemi e analisi di proprietà.* - *VII: Lo stato 'zero'.* - *VIII: Realizzazione dei modelli.* - *IX: Sistemi di controllo e di comando.* - *X: Strategie della ricerca.* - *XI: Realtà e relatività del processo informazionale.* - IV. Identificazione dei sistemi. *XII: Sistema meccanico e sistema indeterminato.* - *XIII: Entropia e differenziazione.* - *XIV: Processo stocastico.* - *XV: La scienza dei sistemi evolutivi.* - *XVI: L'operatore informazionale.* - V. La conoscenza della natura. *XVII: Epistemologia.* - *XVIII: La teoria della scienza e l'esplicazione del mondo.* - *XIX: La trasformazione scientifica del mondo.* - VI. La nuova cibernetica. *XX: Cibernazione e sinettica.* - *XXI: Gli sviluppi della scienza dei sistemi verso il futuro.* - *XXII: Logica e semantica.* - *XXIII: Cibernetica della mente.* - *XXIV: Teoria generale delle previsioni e disegno dell'avvenire.* - *XXV: Dalla paleocibernetica alla sinettica.* - *XXVI: Logica dei valori.*

VON FRITZ, K., **Grundprobleme der Geschichte der antiken Wissenschaft**, Walter de Gruyter, Berlin, 1971, XXXVI+760 pp., figs., DM 98.

In der ersten Hälfte des Buches gibt der Verfasser einen gedrängten Überblick über die Geschichte der griechischen Wissenschaft mit Betonung der folgenden Grund-

probleme: 1. der erkenntnistheoretischen Grundlagen der Wissenschaft; 2. der axiomatisch-definitorischen Grundlegung der Mathematik; 3. der wissenschaftlichen Begriffsbildung, vor allem in der Physik; 4. der positiven und negativen Auswirkung der Anwendung des Prinzips des Schönen in der Wissenschaft, vor allem in der Astronomie; 5. des von den Modernen fast überall missverstandenen eigentlichen Sinnes der aristotelischen Teleologie. Die zweite Hälfte des Buches besteht aus nicht oder nur wenig verändert neu abgedruckten früheren Arbeiten des Verfassers, in denen er das, was in der ersten Hälfte in einem grösseren Zusammenhang diskutiert wird, im Detail nachzuweisen und zu begründen versuch hat.

VUCINICH, A., **Science in Russian Culture (1861-1917),** Stanford University Press, Stanford, 1970, XVI+576 pp., $18.50.

This volume, the second in a series of three, is a comprehensive history of modern Russian science from the great flowering of the 1860's to the beginning of the soviet era. The author views the growth of modern Russian intellectual culture from the standpoint of the development of scientific thought. He traces the growth of the main scientific traditions, the changes in scientific and social philosophy, and the relation of science to ideology, religion, and education. Account has also been taken of literary currents, economic and technological developments, political upheavals in Russia and in the West, and the changing pattern of social stratification. The period was one of notable accomplishment in virtually all the sciences, dominated by such giant figures as Mendeleev, Chebyshev, and Pavlov. Considerable attention is also paid to the institutional base of Russian science (the academies, universities, and learned societies), and to the impact of the leading Western thinkers on Russian scientific thought.

ZEL'DOVICH, Ya. B. and NOVIKOV, I. D., **Relativistic Astrophysics,** VOL. **I: Stars and Relativity,** The University of Chicago Press, London, 1971, XVIII+522 pp., figs. and tables, £10.80.

Zel'dovich and Novikov's *Relyativistskaya astrofizika*, published in Russian in 1967, marked the first attempt to deal with the entire field of relativistic astrophysics as a unified whole. Because so many developments have occurred on the observational and theoretical fronts since 1967, the authors have updated and substantially rewritten their work in two volumes for this first authenticated English edition. Volume I, *Stars and Relativity*, presents introductions to such fundamental subjects as the general theory of relativity, thermodynamics and nonequilibrium processes for matter at ultrahigh temperatures and densities, stellar structure and evolution, and stellar dynamics. It then shows how these theoretical tools have been used in the last decade to build models for supernovae, explosions in galaxies, quasars, cosmic X-ray sources, pulsars, and gravitational-wave sources. Volume II, *The Universe and Relativity*, will explore cosmological observations and the evolution of the universe.

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Rivista "SCIENTIA" Editrice
Sede legale - Milano, Via Donizetti 55
Registrata presso la Cancelleria del Tribunale di Milano col N. 338
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